Notes

Preface

1. Albert Spaulding railed against the notion that “the only purpose of archaeology is to make archaeologists happy... a philosophical position which cannot be tolerated in a scientific context” (Spaulding 1953:590). I agree: Just because you call yourself an archaeologist does not mean you get to define “archaeology” any way you want. That’s why they call it a discipline.

2. I am aware of objections to the term “prehistory,” taken to mean that nonliterate societies have no history before some Euro-American writes one for them. And, yes, I am talking about “history” in the narrow, conventional academic sense. But I do not mean that tribes had no history in the general sense of the term. More than anything else, this book argues (again and again) that the ancient Southwest had a vibrant, dynamic past, which archaeology has failed to capture. The way archaeology might capture that past is to write a narrative, a “history” in the academic term. And that is what I mean by pre-history: a history accessed archaeologically, sans documents required by academic history. Much more on this later, especially in chapters 5 and 6.

3. I wrote a Master’s thesis in 1978 (on Mimbres, not Chaco) with a chapter challenging the ahistoricity of (then) New Archaeology. My committee expunged it: They thought it would hurt me, professionally. Walter Taylor’s name came up, as I recall.


5. I am weary unto death of Chaco and its intrigues. I could have used Mimbres–Casas Grandes, or Sinagua, or Hohokam, or other eventful episodes in the Southwest. All have vivid pre-histories that have suffocated under the strictures of American Anthropological Archaeology. But (unfairly) none of them have the widespread name recognition of Chaco. In chapter 7, I urge others to apply the argument and methods discussed in this book to the Southwest’s other historical highlights and they are many. Someone other than me should do that.

6. Young Walter Taylor, all those many years ago in A Study of Archeology, asked the right question: “Archeology: History or Anthropology?” (Taylor 1948:25), coming down in favor of the latter. As the Grail Knight told Dr. Jones: “He chose poorly.”

7. “Sometimes you have to slap them in the face just to get their attention,” as the Ghost of Christmas Present explained to Frank Cross, just before she hit him with a toaster. I quote the great classicist Moses Finley: “This essay has a pervasively critical tone, which is neither accidental nor ‘unconscious’...it is concerned with the way ancient historians go about their work, what they say or do not say, what they assume or overlook” (Finley 1990:60). On another tack, our better angel of style Steven Pinker admits to a “deliberately impolite tone” in his articles about obtuse academic writing—which comes in for some grief from me in chapter 5. A sharp poke works better than a gentle nudge, if you are in a hurry. And I am.
8. Anna O. Shepard was not the first or the last to confront this conundrum; but, in her no-nonsense way, she faced it forthrightly: “Can the common human tendency to resent criticism and the resultant fear of offending be corrected by reason? We would like to think that all anthropologists maintain an impersonal attitude toward their work, and that they welcome criticism as a valuable aid. But if we have not yet attained this level, must we not admit it and reckon with the facts?” (Shepard 1937:182).

9. For example, "state." If we'd gotten Chaco into that Old Boys Club back in the 1980s, when the term "state" was common parlance, much of this book would have been unnecessary. Now "state" has been theorized and problematized into a paste; people wrinkle their brows when you say the word. But Chaco belongs in the space-that-was-formerly-state, and to get it there we have to go back to the future, and make our case the old-fashioned way. Should have happened forty years ago. The late 1980s—post-Chaco Project, pre-NAGPRA—was a strategic moment missed because of Pueblo Space: “State” was then (and now) deemed unseemly for any society in the Southwest.

10. I've heard of people reading two copies of *History of the Ancient Southwest* simultaneously, one for text and the other for footnotes. Notes are online both for ease of use and to support a point I will make in chapter 6. The structure of this book is part of my argument.

Chapter 1: Chaco in the Twentieth Century

1. And what about my biases? As the American philosopher Harry Callahan said: A man’s got to know his limitations. In my declining years I know some (maybe most?) of my peccadillos, my dispositions, my prejudices, my mistakes (all three o’em!).

   I'm an old White male, agnostic and cynical, wheezing my way up the down escalator toward that Great Seminar Room in the Sky. I have been told many times I have the sense of humor of a fourth-grader, and I cannot argue with that; but at least I have a sense of humor.

   I hoped to study Classical Archaeology but my freshman advisor, finding I had no Greek or Latin, shunted me off to Anthropology’s archaeology—which (he must have thought) any idiot could do. At the time, I had no idea what Anthropology was. (I’m still wondering.) So, I majored in Anthropology, but my real interest was History. My doctoral training was in close proximity to Lewis Binford, but not directly “under” him (as they say; a terrifying thought). New Archaeology was energized and interesting, but early and often I questioned New Archaeology’s and Processual Archaeology’s ahistoricism, throwing history on the dust bin of...history. That got me in trouble.

   I read a lot of theory, and find some of it useful. I subscribe or identify with no particular school of archaeology (if such still exist) save this: Archaeology is history or it is nothing. But I’d like to use that history in scientific ways: Generalizing, finding patterns and processes. I think the truth is out there: I’m a closet positivist. Binford was a strong persuader.

   Three years of fundraising for Crow Canyon sowed seeds of doubt. Asked why a potential patron should write a check for archaeology rather than the Children’s Hospital, I could not find answers that satisfied me. Those early doubts redoubled and redoubled again during ten years of NAGPRA (Lekson 2010b) which shook my faith in archaeology; I almost quit the field.

   I was asked, over and over, first by ricos and then by Indians, why archaeologists do what they do, what good accrues. With ricos, I must have answered satisfactorily; we raised lots of money. But with Indians...no, I could not justify archaeology in the
face of their outrage. I could not defend Southwestern archaeology to Indians and, ultimately, to myself. Maybe the Indians were right: We should pull the plug. This book is, in part, an argument with myself against that conclusion. Revising the text for a final time, October 8, 2017 (in Bratislava, of all places), I’m not sure I’ve convinced myself: Southwestern archaeology’s small triumphs seem hollow in the face of Native anger. In any event, we revisit this topic in chapter 5. Perhaps by the time I’m revising chapter 5, I’ll make up my mind…

2. And which Gustavo Verdesio (2010), nodding to Foucault, calls “regimes of visibility”—the archaeology we CAN see given our colonial lenses, blinders, expectations (my words, not his).

3. The paraphrased quote: “And this is important, because if the entire hominid fossil record were to be rediscovered tomorrow and analyzed by paleontologists with no horses already in the race, it is pretty certain that we would emerge with a picture of human evolution very different from the one we inherited” (Tattersall 2015:213). Tattersall insists that history matters more in his discipline—paleoanthropology—than in other branches of paleontology because of the subject matter: Humans. “In this field, more than most, what we think today continues to be very intensely influenced by what we thought yesterday—and the day before that” (Tattersall 2015:xii); that should go double for archaeology, because we deal with anatomically modern humans, critters much like ourselves.

4. A tactic used throughout this book: Featuring one or a few people to represent broad movements or ideas. For example, Vine Deloria Jr. might represent Indian annoyance with archaeology—he’s pretty good at that—but there are legions of Indians who are pissed off about archaeology (and not all of them are fond of Deloria). That tactic—focusing on one or a few persons or one or a few publications—leaves me open to critique of personal attack, but it also got this particular job/book done. You will notice that this book covers a lot of territory, and (as explained elsewhere) hopefully with accuracy if not precision. If I wanted both accuracy AND precision, the effort would expand to encyclopedia length. I don’t want that, you don’t want that…

5. Gender balance tilted strongly male, but strong women were there too: Bishop and Lange 2001; Davis 1995; Lamphere 1992; Parezo 1993; D. Smith 2005.


7. The literature on Europe’s reaction to the New World is vast and vastly interesting. Four accounts I have found very useful are Lee Eldridge Huddleston’s (1967) Origins of the American Indians: European Concepts, 1492–1729; George Kubler’s (1991) Esthetic Recognition of Ancient Amerindian Art; Benjamin Keen’s (1971) The Aztec Image in Western Thought; and Stephen Greenblatt’s (1991) Marvelous Possessions: The Wonder of the New World. Keen and Kubler, in particular, should be read by every practicing North American archaeologist. I am not aware of comparable reviews of
Asia's reaction to the New World. Of course, Gavin Menzies (2003) insists that China explored the New World in 1421 (and the Mediterranean a decade later). So maybe they were not surprised by Columbus’s news.

8. Rousseau and, much later, Hegel dismissed Native America as essentially history-less—a conclusion convenient for colonialism.

9. Al Schroeder (1979) provides a catalog of the devastation. Farming peoples in southern Arizona suffered much the same histories of colonization as the Pueblos, and like the Pueblos remain on or near their ancestral lands. As did many of the less settled, hunter-gatherer tribes. Although their histories are fraught with displacement and suppression, the Navajo Nation and most Apache groups remain today more or less on their Native lands. In New Mexico, a particular exception was the Chiricahua Apache, the last “wild” tribe to be reduced. When Geronimo came in the last time, in 1886, all the Chiricahua were declared prisoners of war and sent to prison first in Florida, then to coastal Alabama, and finally in Oklahoma. They were freed in 1912, but their homelands in New Mexico and Arizona were gone, settled by Whites. Most returned to join the Mescalero Apache in their reservation; many stayed on in Oklahoma.

10. Once the pyramids of the Mississippi Valley and the vast geometric monuments of Ohio had been demoted to “mounds” and naturalized as Native, there was no evidence for civilizations—Indian or Lost Race—east of the Great Plains. There are many accounts of early American investigations of eastern woodlands prehistory; one of the most engaging is Roger Kennedy’s (1994) *Hidden Cities: The Discovery and Loss of Ancient North American Civilization*, which emphasizes the impact of Native prehistory on early American thought. Another “must read.”

11. Albert Gallatin, one of the last real (if late) Enlightenment figures in American intellectual life (he died in 1849, deploring the Mexican War). Gallatin singled out the Pueblos among the many Native societies of North America—one of the first advocates of Pueblo exceptionalism—vis-à-vis “warlike” Navajos and Apaches. His research on the American Indian was largely an armchair affair, reading reports and corresponding with explorers. Compiling information on scores of tribes, he fastened on the Pueblos, approvingly: “If I have dwelt longer on the history these people [the Pueblos] than consistent with the limits of this essay, it is because it has been almost the only refreshing episode in the course of my researches” (Gallatin 1848:xcvii; see also Bieder 1986:50). Gallatin recognized that the Pueblos more closely resembled the settled villagers of Mesoamerica than any other North American tribe. He considered migration north out of Mexico, but settled on selective borrowing; in any event, he concluded that “this singular phenomenon deserves particular attention” (Gallatin 1848:liv). Gallatin may have been the first major American intellectual to declare the Pueblos exceptional. He certainly was not the last.

12. From the start, American Anthropology was Indianology. Steven Conn makes this point about “American anthropologists in the late nineteenth century”:

Whatever their methodological differences, whatever the differences in the materials they studied, archaeologists, linguists, ethnographers, and physical anthropologists in this country all shared the same subject: Native Americans. In the United States at the turn of the twentieth century, Native Americans served as the glue that held together the incipient, almost inchoate anthropology. Conversely, anthropology became the sole disciplinary home for the study of Native Americans. By the end of the nineteenth century, there could have been no American anthropology without Indians to give it some semblance of disciplinary coherence, and there was virtually no serious study of
Indians that was not anthropological. At a disciplinary level, the two were bound together inextricably. (Conn 2002:155)

13. Morgan and Bandelier together laid the foundations of Southwestern archaeology. Morgan shuffled off to his Reward before Boas got off the boat. To be sure, Boas did major foundational work too (Stocking 1960); but Morgan had already established the field as a valid scientific endeavor. In my opinion, Boas himself was less influential than Morgan on the ultimate course of archaeology of North America (although many of Boas’s students went on to dabble in the dirt and discover wonderful things). In Stocking’s (1974) Franz Boas Reader, the word “archaeology” merits only a half-dozen index references, and those mostly point to the work of others. Boas gets only a few mentions in Bruce Trigger’s (2006) History of Archaeological Thought, mostly through his students or as a generalized “Boasian anthropology.” Boas was too busy salvaging ethnology to worry much about archaeological pots and rocks. In his essay on “The History of Anthropology,” Boas credits archaeology as essentially the “evolution of culture.” “Of course, in many cases the chronological question cannot be answered, and then the archaeological observations simply rank with ethnological observations of primitive people” (Boas in Stocking 1974:23–36).

14. Morgan was president of that organization in 1880.

15. E.g., Bahn 2014; Dyson 2013; Trigger 2006. We make students read these histories, but I’m not sure students understand why they matter. The importance of the difference between American Archaeology (science) and European archaeology (history) cannot be overstressed. To 99 percent of the world, it’s academic; but to understand American Archaeology, you must realize that it emerged in a very different intellectual context than almost any other archaeology of its time.

The difference continues to confound us. Matthew Johnson, a leading British theorist, stated: “For many in North America, archaeology is seen as a subfield of anthropology, or at the very least the two disciplines are seen as closely linked. In Europe, a straw poll of the views of most archaeologists would suggest that the sister discipline of archaeology is history” (Johnson 2010:185).

16. Morgan, arguably, made more of a mark on modern times than almost any other American Anthropologist. Here’s my pitch: Karl Marx and, more importantly, Friedrich Engels enthused about Morgan’s work, and cited Morgan to support their theories of social evolution which eventually gave us Communism. Communism and various unpleasant reactions to it shaped much (most?) of twentieth-century history. Morgan, of course, did not directly cause the Soviet Union or the People’s Republic of China, but his works in translation were part of the Marxist canon, and required reading for academically inclined Marxist theoreticians. I suspect they weren’t reading Margaret Mead.

In Houses and House-life (Morgan 1881, which Engels read very carefully), Morgan argued that all New World societies were communal; and (hot dog!!!) communalism began in the Four Corners and diffused outward throughout the New World. Since Morgan’s view of the Four Corners was heavily Chacoan, one could say that Marxism began with Chaco. It would not be true; but one could say it. Houses and House-life was translated into Russian in 1934, when Russia enjoyed a breather between Civil War and Great Patriotic War, as Doma i domashnyaya zhizn’ amerikan-skikh tuzemtsev (Morgan 1934).

17. Southwestern ruins (in models and life-sized mock-ups) and Pueblo Indians (Hops and Zunis, often in cheesy costumes) were staples at fairs and expositions from 1876 onward: e.g., at Chicago’s 1893 Columbian Exposition, St. Louis’s 1904 Louisiana

18. The “Romantic View” and alternative pre-histories were not only revelations or speculations or academic debates: Some had serious policy implications. The Book of Mormon appeared in 1830, and the State of Deseret (comprising not only Utah but also Nevada, most of Arizona, a large chunk of Colorado, and a bit of New Mexico) was proposed in 1849. The “Mormon War” ran its course in 1857–1858. There were (happily) very few casualties in the Mormon War, but there were in fact casualties: People died over an alternative archaeological ideology. As of course people died in earlier persecutions of Latter-Day Saints, which pushed them into the West.

19. Most famously, not on a Mexican map but on a map compiled in Mexico City by the Prussian polymath Alexander von Humboldt (1806, 1811), which was the basis for many later maps, such as the 1847 Disturnell map which caused the geographic embarrassment redeemed by the Gadsden Purchase.

I recently saw a Mexican map of 1845 which, at Aztec Ruins location on the Rio Animas, had this text: “Gran des ruinas de los Aztecas” (Garcia 1845)—which was not so indicated on Humboldt’s earlier maps. The conventional story has “Aztec Ruins” being the invention of nineteenth-century Anglo settlers; perhaps the new colonists accepted (and anglicized) the judgment of the older colonists? The map also notes “Cheque” at about the place (less certainly than Aztec) where Chaco should be—phonetically close.

20. Forgive the Cold War allusion, but an earlier attempt, “Tortilla Curtain” (Minnis 1984), while funny, did not find favor. This was written before Trump and Trump’s Wall; Trump’s Wall isn’t remotely funny. And I’ve always liked the drama of Churchill’s 1946 quote. The ex-PM turned a fine phrase, although he may not have originated this one. “Iron curtain” had been used before, very early in 1945, by Joseph Goebbels—also in relation to the impending Soviet ascendancy in Eastern Europe. And apparently there is an even earlier physical referent: I have seen a real “iron curtain” in the Budapest Opera House, a nineteenth-century thirteen-ton metal monster which could be draped (or, rather, dropped) across the stage to prevent accidental fires in the scenery from cooking customers in orchestra seats. I suppose it could also be used to shield subpar sopranos from rotten vegetables and stale kaiser rolls. “Iron Curtain” is the exact translation of its Hungarian name, and I understand the safety feature was widely adopted across Europe in the days of gaslights.

21. I focus here on Anthropology’s hijack/appropriation of America’s ancient past, working mainly from histories of science, such as Robert Bieder’s (1986) classic Science Encounters the Indian. American Anthropological Archaeology in effect said, we—not History—will take care of ancient Native America (if any). Steven Conn (2004), a historian at Ohio State University, has given us an excellent account of the same events, from History’s side: History’s Shadow: Native Americans and Historical Consciousness in the Nineteenth Century. American intellectual life in the nineteenth century was a small pond, so many of its denizens appear in both Bieder and Conn. But Conn’s perspective as a historian is essential to understanding how Anthropology got away with the Great History Heist. A few of the early historians (most importantly for the Southwest, Prescott and Bancroft) footnoted speculations about American prehistory, but as History professionalized, that turf was ceded to Archaeology. Conn concludes his book with a quote from late-nineteenth-century antiquarian Stephen Peet, who wondered why American History begins with Columbus: “The question is whether this shall continue to be so.” And Conn replies: “That question remains.”
Conn’s (1998) *Museums and American Intellectual Life, 1876–1926* is also essential reading, exploring the Anthropology-History semifracas in the context of museums and museology.

22. Morgan subdivided each stage: Lower, middle, higher; and he thought Indians stalled out somewhere around Middle Barbarism.

23. Morgan (1876) *Montezuma’s Dinner*. For the Southwest, Morgan was preceded by and indebted to Albert Gallatin (1761–1849), a polymath politician (we met above) whose documentary research before the Mexican War convinced him “that the southwestern tribes had no kings or nobility and thus no ‘serf of degraded cast’; no clique comprised of despot, favored cast, and priests…but rather a government in the hands of a council of old men,” from Gallatin’s (1848) introduction to “Hale’s Indians of North-west America.” Gallatin’s remarkable and remarkably long essay—a book-length introduction to a book—is worth reading.

24. Hewett was a confirmed Morganite. Writing about a “New World Culture Type” which encompassed and characterized all Native societies of both North and South America, he declared them all communal and essentially egalitarian:

   It is the glory of the American Indian race that it developed a type of government entirely different from that of the European and more effective. The welfare of the people was the supreme end of government. If individuals became prominent, they were never personally glorified. In America the idea of monarchy had no place. The European, and in this I include the American of today, relinquishes painfully his preconceived ideas. “Empires of the Montezumas” seem necessary for his intellectual satisfaction. May we now drop these childish classifications and see the Indian in the light of his finer achievement in government: that is, a type free from monarchical authority? The typical government throughout the Americas was republican. (Hewett 1930:42)

   —by “republican” he meant an “elected” council of elders.

25. Most histories of Southwestern archaeology start the clock somewhere in the nineteenth century, perhaps as early as Lieutenant Simpson’s reports on Chaco Canyon in 1849 (Simpson 1964). But the ball really started to roll much later, with Adolph Bandelier’s remarkable surveys of 1880–1892. Lieutenant Simpson’s observations (like most of the early accounts) were incidental to his real job, which was chasing Navajos. Bandelier was employed by the Archaeological Institute of America to do archaeology and nothing else—the Southwest’s first professional archaeologist! Since then, thousands of Southwestern archaeologists (yours truly included) elbowed our way for a place at the trough, making our livings on someone else’s past. Thanks, Adolph!

26. Bandelier is not shy about his opinions, which owed much to Morgan. For example: “The usual supposition is that Casas Grandes was the ‘capital’ of a certain range or district, and that the smaller ruins are those of minor villages…. But I doubt whether there was any governmental tie uniting the villages on the Rio de Casas Grandes between the Boquilla and Corralitos with those near Janos or those near Ascension, even if all these groups were contemporaneously occupied. It is inconsistent with the nature of Indian institutions that clusters geographically separated should be politically connected.” (Bandelier 1892:570, emphasis added). That attitude about “the nature of Indian institutions” carried over far into the twentieth century, with turf-based objections to Chaco, its “outliers,” and its region. We simply know that Indians didn’t do stuff like that: Inconsistent with the nature of Indian institutions. But, they did.

28. Bandelier 1892:592. Actually, he was quoting himself from an earlier report, with his opinion unchanged. His conclusions to his 1892 Final Report are worth quoting at length:

Thus the tales of slow wanderings, or rather shiftings, of Indian clusters from colder to warmer climes across the Southwest, become by no means improbable; but such movements must not be imagined to have been on the same scale as the irruption of vast hordes, such as Europe witnessed in the early part of our era, and which early writers upon Spanish America have conceived to have occurred in Mexico in prehistoric times. [that is: Aztec migrations from a southwestern Aztlán] I say this not in order to censure deserving men who centuries ago took pains to record the fading traditions of tribes then first becoming known to Europeans. At their time ethnology was not yet a science, and they wrote according to the prevailing state of knowledge, and according to the points afforded them for comparison. Hence arose misconceptions and honest exaggerations, which have become deeply engrafted upon ethnological thought, and have cast a veil over ethnological facts. The movements of tribes have been slow and disconnected; there has been, it seems, a general tendency to drift towards the tropics, but never in a continuous stream…

This is a picture of the prehistoric past of the Southwest, somewhat different from that which, modelled upon the ancient history of Europe, has often been presented. On a previous occasion I thus wrote to the Institute on the subject: “The picture which can be dimly traced of this past is a very modest and unpretending one. No great cataclysms of nature, no waves of destruction on a large scale, either natural or human, appear to have interrupted the slow and tedious development of the people before the Spaniard came. One portion rose while another fell; sedentary tribes disappeared or moved off, and wild tribes roamed over the ruins of their former abodes” [quoting himself in Fifth Annual Report to AIA, p. 85]…

Further than what I have intimated in these pages, I do not venture to go for the present. The time has not yet come when positive conclusions in regard to the history of the Southwest can be formulated. In the course of the past ten years new methods of research have been developed in ethnology, as well as in archaeology, and at some future day these may lead to the solution of questions which at present are perhaps not even clearly defined.

Santa Fe, New Mexico
April 20, 1891

29. Cushing’s “My Adventures in Zuni” were published in Century magazine 1882–1883. “Cushing’s sympathetic view of Pueblo life was published at the same time as racist articles depicting Plains Indians unfavorably during the High Plains Wars. His report demonstrated the basic humanity of the Zunis to readers accustomed only to negative stereotypes and inflammatory depictions of Indian warriors” (Traugott 2012:86).

Cushing’s work was criticized by his time’s establishment, perhaps unfairly. He was considered a showman and not a serious scholar. David Wilcox wrote an essay— “Restoring Authenticity: Judging Frank Hamilton Cushing’s Veracity” (Wilcox 2003) looking at Cushing’s critics; he concluded thus about two of his most famous detractors, Frederick Webb Hodge and Jesse Walter Fewkes: “Both rose to fame by
trampling on the reputation and discrediting the ideas of the ‘Sun’ of a previous era, Frank Hamilton Cushing. Were they liars? Let us say that they may have been unscrupulous in pursuit of their ambitions for fame and power” (Wilcox 2003:201–203).

Cushing is remembered at Zuni, but not fondly (e.g., Phil Hughte 1994, A Zuni Artist Looks at Frank Hamilton Cushing)—the first of many obnoxious, invasive anthropologists. There’s an old Zuni joke—and it’s a bitter joke—that goes something like this: A Zuni family consists of a mother, a father, their children, and an anthropologist.

30. Bandelier, Powell, and Hewett were more significant to the Southwest, a place in which Boas conducted little research. Some of Boas’s students did important work in the region: Elsie Clews Parsons, A. L. Kroeber, Leslie Spier, and Ruth Bunzell were all to various degrees associated with Boas. And of course Ruth Benedict. “But if there was a disciplinary dynamic that drew Boasians to the area, it seems also clear that some of them—sharing to a considerable extent the backgrounds, motives, sensibilities, experiences, and impressions of nonanthropologist intellectuals—also felt the pull of [D. H.] Lawrence’s ‘invisible threads of consciousness’” (Stocking 1989:220).

31. Timothy Pauketat, speaking of the (my words) Glass Ceiling over Cahokia, noted that “it was based on the common sense of the nineteenth and early twentieth centuries—that American Indian nations could not have accomplished anything worthy of note. Today some worry that this national legacy can still be seen in contemporary archaeological theorizing: consider the dehistoricizing of evolutionist constructs, such as the chiefdom, or the out-of-hand dismissal of historical complexity via Occam’s Razor. And the issue has come to a head over Cahokia, a place Alice Kehoe has noted is ‘hidden in plain sight’ because of the cumulative biases of our intellectual heritage” (Pauketat 2007:135).

32. The triangle formed by Taos in the north, Milligan Gulch (a large Piro Pueblo) in the south, and Hopi in the west. The hinterlands and resource areas of the many Pueblos reached far beyond the geometric construct. A comparable area in Europe might span Brussels to Paris to Stuttgart, encompassing considerable ethnic, linguistic, and national diversity, among and between populations more densely connected than the “Pueblos.”

33. An intermediate category, rancheria, designated settlements less permanent than Pueblos but more stable (and predictable) than the Indios Barbaros. In the Southwest, the best-known application was to southern Arizona’s O’Odham peoples, who graded from “No Village” hunter-gatherers, to “Two Village” seasonal-round-with-agriculture, to “One Village” settled farmers.

34. The Spanish used the term pueblo for the Nahua altepetl, a small polity comprising a town and its hinterlands (which we meet again in chapter 3); contra Charles Gibson, The Aztec under Spanish Rule (1964), who translated the colonial category “pueblo” simply as “town.” An early eighteenth-century dictionary of Castilian Spanish produced by the Royal Academy had this to say about “pueblo”:

Pueblo: El Lugar ó Ciudad que está poblado de gente. Pueblo tanto quiere decir como ayuntamiento de gentes de todas maneras, de aquella tierra dó se llegan.... Se toma también por el conjunto de gentes que habitan el lugar. (Real Academia Española 1726; ellipsis of citations, cross-references)

The first definition of pueblo is, of course, “town.” But the second definition indicates that pueblo was also a political unit, centered on a town and including its territory. Of course there were still other meanings of pueblo in the early eighteenth
Notes to Pages 11–12
century, one of which is “la gente común y ordinaria de alguna Ciudad ó poblacion, a distincion de los Nobles.” Indeed, it is possible to speak of “Pueblos within Pueblos” (Johnson 2017) for political subdivisions within a polity.

35. In central Mexico, Pueblo de Indios was one term used for the Native political form we will encounter later in this book as “altepetl”; and also for towns created by colonial policy. It was a colonial policy (encouraged by royal decree in 1545) to “reduce” and aggregate Natives into towns, many of which persist today; but the forms taken by those towns probably mirrored pre-Columbian patterns (e.g., Gutiérrez Mendoza 2012; Ouweneel 1999).

36. E.g., Jorgensen’s (1980) analysis of 172 tribes north of Mexico. Clustering programs do exactly that: Given an array of cases, such programs will move more similar together, less similar apart, based on whatever measures and statistics are employed; many programs decide for themselves what’s important and what’s trivial to a comparison. Caveat emptor. But, what if Jorgensen’s sample stretched south of the border? With whom, then, would Pueblos cluster? Someone—not me—should redo Jorgensen, extending the sample to the south.

37. The various groups we call “Pueblo” did not, to my knowledge, get a vote on this terminological assemblage. I suspect they accept it today because they have no choice. Just as they are, legally, all “Indians,” they are, legally, all “Pueblo.” Roger Echo-Hawk (2010 and elsewhere) has written eloquently on the absurdity of “Indian” as a category or a race; he insists that he is not Indian, he is Pawnee. Are Pueblo people content to be “Pueblo”? I don’t know.

38. Murdock’s (1981) Atlas of World Cultures, the last edition of a compendium initially published in the late 1960s. The notion of Pueblo Culture, of course, goes much further back in American Anthropology: In A. L. Kroeber’s 1939 Cultural and Natural Areas of Native North America, the “Pueblo Subculture Type” was described (or not) thus: “The true Pueblo culture is so distinctive, and so well known both ethnographically and archaeologically, that its detailed discussion here is unnecessary.... It constituted a localized and self-contained culmination” (Kroeber 1939:34–35).

39. Kohler 2012:213. This is a very useful paper, and one of the few recent analyses that tackles the problem of pan-Pueblo similarities. “Sprachbund” is a speech community of different groups who share many loan-words, perhaps lingua franca(s), and with many multilingual members. See also Mills 2008. There is, alas, little interest in such things today. NAGPRA and postmodern particularity reinforce a long-held, nearly universal belief among Southwesternists that “my valley’s different”—emphasizing difference over similarity, local over regional.

40. In my opinion, then, the principal factor generating the great degree of cultural continuity across the Pueblo Southwest during these 700 years [600–1300] was the frequent population movement among its subregions.... These movements not only helped create a Sprachbund; they also helped synchronize (albeit imperfectly) culture change across this region through time. The mixing provided by such large-scale movements would have been reinforced by mobility at smaller scales. Since most communities within these regions would have been too small to be successfully endogamous especially prior to 1300, regular local movement of women (or men) across social group boundaries can also be anticipated and undoubtedly reinforced the effects of the larger inter-regional movements. (Kohler 2013:222–225)

41. Kohler acknowledges the “operation” of Chaco/Aztec: “The political and religious system centered on Chaco Canyon influenced most of the Pueblo world during
the Pueblo II (PII) period (ca. 900–1140) and...would have encompassed different language groups. Remnants of this system continued to influence much of the Northern San Juan region for at least a century thereafter. I do not emphasize these influences below—because I suspect they built on and were made possible by existing similarities largely produced by other mechanisms—but I do not deny their operation" (Kohler 2013:215). He introduces the concept of a "hierarchical society transition" or HST, to complement the Neolithic Demographic Transition; intriguing! I look forward to the further adventures of HST!

42. Southwestern archaeology needs something like Cathy Gere's (2009) fascinating *Knossos and the Prophets of Modernism*. I had hoped to write such a book someday, but this chapter is probably as far along that path as I'll go. Sifting through a hundred years of archaeological writing, finding and noting thousands of knee-jerk, throwaway references to ancient communalism and spirituality...it's more than I can handle. It's a project waiting for the right doctoral student: smart, cynical, with a high tolerance for hooey.

43. Pueblo Mystique and Pueblo Space operate much like the Maya Mystique, pervasive but mistaken ideas of Maya civilization that controlled archaeology in the Maya region through the 1950s. According to David Webster (2006), the key elements of the Maya Mystique were: Unique and exceptionally gifted people; egalitarian ("priests & people"); centers as vacant ceremonial constructs; inscriptions related to esoteric astronomy, calendrical and ritual; and peaceful. Like Pueblo Space, there was powerful “popular” buy-in for this, which in turn influenced archaeology (Webster 2006:130–133). The parallel is risibly close, except for “inscriptions.” Ironically, “what finally put paid to the mystique was...the inscriptions. Ultimately it is the epigraphic messages left by the Maya themselves that convinced Mayanists of the existence of kings and nobles, their impressive households, their wars, and many other unexpected things. The irony is that these unique messages were required to open up our minds that the Maya were not so unique after all” (Webster 2006:151).

Webster notes, ruefully, that it was almost impossible to recognize the Mystique when one was in it, working under that umbrella. It pervaded Maya archaeology for decades, and it took exceptional discoveries to break the spell: For example, the discovery of a clearly royal tomb at Palenque and, above all, the decipherment of the inscriptions. And then Maya made sense (Webster 2006:149–150). Much the same could be said for Pueblo Mystique and Pueblo Space—except we have no inscriptions, only the archaeological facts of Chaco, and other anomalies.

44. I'm not making this up: This stuff happens and you can get in trouble pointing it out. As doubtless I will. Here's an account by Charles Briggs of the trouble Sam Gill got into with his deconstruction of "Mother Earth":

Historian of religion Sam Gill argues that “the notion of Mother Earth as a Native American goddess has been created to meet various needs of Americans of European ancestry” (1987:106). He suggests that it is a story fashioned by white scholars “that supports a range of social, economic, and political relations, very likely oppressive” (1987:128). Lacking a basis in Native American spirituality, the Mother Earth-as-goddess concept helped shape European Americans' self-defintions and bolstered their political-economic ascendancy. Gill claims that the power of this hegemonic image is so strong that Native Americans have internalized the concept of Mother Earth during the past 100 years, thus transforming a white fiction into Native American social reality. One of the foci of his book is the use of these conceptions by
Native American activists in countering threats to land loss and in building a pan-Indian identity (1987:145–146).... Rather than thanking Gill for clearing away white false consciousness, however, a number of Native American writers responded in a highly critical fashion [accusing him of cultural imperialism]. (Briggs 1996:436)

45. The “eternal and unchanging” bit was an obvious draw for weary moderns: Something fixed and permanent in a chaotic world. “In their constructions of the prehistoric and living peoples of the Southwest, Anglo imaginations claimed to see, sought to share, and ultimately brought to market an ethnographic present that remained somehow unchanged over centuries, while clearly having been surrounded by momentous events” (Hinsley 1990:469). Elsewhere, Hinsley (1996:196) called this “stasis as aesthetic therapy.”

But “eternal and unchanging” makes things difficult for archaeology, which is all about change.

46. If a higher authority is needed, I offer Peter Whiteley: “Some presumptions I attributed...to New Agers derive in part from [university] classroom inventions of Native Americans: timeless, history-less spiritualists at harmony with one another and in tune with nature—and this is the story many students continue to want to hear” (Whiteley 1993:143).


48. “Together, the anthropologists, archaeologists, writers, artists, railroad owners, concessionaires and traders created a view of the American Southwest that appealed to persons of the East and brought money to the Southwest in the form of tourists, land developers and philanthropists” (Thomas 1999:126). It was not entirely a home-grown enchantment: Frank Cushing—part ethnologist, part showman—pitched Zuni in person to Boston society in the 1880s: “Cushing brought rich travelling theatre [of Zuni men] eastward in 1882, a year before William F. Cody first took his Wild West show on tour” (Hinsley 1989:180). This made a deep impression on the Brahmins and Gilded Age journalists such as Sylvester Baxter: “The Zunis became the special property of artists and poets, and their pueblo has served as the playground for mimetic anthropologists from Cushing to Dennis Tedlock.... Not surprisingly, Baxter was one of the first to formulate this view of a conflict-free, communal order” (Hinsley 1989: 203); “As the mythicization of an Apollonian Southwest began to take hold in cosmopolitan circles of the east, Zuni society was falling apart through drunkenness, disease, dissension and death” (Hinsley 1989:202). See also Hinsley and Wilcox 1996, McFeely 2001.

49. Pueblo Mystique was partly real and mostly fiction, but it was not ethically “wrong”—in its time. At the turn of the last century, inventing traditions was all the rage. All over Europe, boosters and taste-makers were creating mythical pasts that make the Pueblo Mystique look tame. Think Druids or Celts; or earlier the fantasy worlds of Walter Scott and Richard Wagner. These may have begun as innocent diversions or
amusements, but they too often became propaganda (Cantor 1991; Hobsbawm 1983; Lowenthal 1998). In Hobsbawm’s view, “invention of tradition” typically involves a group or society inventing a tradition for themselves—often a nation-state reinventing itself, along faux “traditional” lines with some claim to historicity. Europe was “Mass-Producing Traditions: Europe 1870–1914” (Hobsbawm 1983) at much the same time Santa Fe was inventing the Pueblo Mystique and Buffalo Bill was inventing the Wild West.

And, intriguingly, about that same time Mexico nationalized its prehistory, creating a national identity centered on Aztecs (at least in central Mexico; e.g., Brading 2001; see also Powell 1968). Santa Fe’s shenanigans pale in comparison. After the dust settled from the Revolution of 1910, Aztec identity became government policy. The influential Porfiriato Minister of Public Education, José Vasconcelos, strongly promoted Aztec national identity in the 1920s through mural art, museums, and—of course—public education. His philosophy of a “mixed race,” *raza cósmica* (Vasconcelos 1997[1925]), remains potent today both in Mexico, and in the United States as La Raza.

This is not to disparage Mexico’s Aztec heritage or the reality of La Raza; rather to emphasize that things like this were happening during the creation of the Pueblo Mystique (and Pueblo Space) both here and abroad. What happened in Santa Fe wasn’t unusual; except within the United States, where it was indeed unusual. Exceptional, by design.

50. Not alone, of course. The Chamber of Commerce pitched in; and Hewett and the Chamber had epic battles over who controlled the brand (Chauvenet 1983:109–120). And don’t forget the artists! Beginning in the 1910s and 1920s and continuing today, artists and writers fell under the Pueblo Mystique and took it in many new directions. “Artists and writers in the Taos and Santa Fe colonies challenged this federal policy of suppressing aboriginal culture. They had come to cherish their Pueblo neighbors; they found them charming, useful, and instructive; useful to painters as models, to writers as source of inspiration for verse, and substance for fiction and nonfiction composition” (Gibson 1981:286). For the literary scene, see Weigle and Fiore (1982). Art historian Jana Perkovik gives most of the credit to art:

> The artists were the first to revolt.... Santa Fe’s city leaders took the opportunity to re-imagine the city’s traditional architecture and rich cultural history. They engaged the artist community to restore and preserve Santa Fe’s old buildings. To ensure it was clear that they were rebuilding something distinct, something local, they called it the “City Different.” Now that was something the tourists would take to.... As Umberto Eco would say, it was more representative of reality than reality itself. (Perkovik 2015)

Archaeologists often crossed disciplinary divides between art and archaeology (for example, Ken Chapman: Chapman and Barrie 2008, Munson 2008; and even Edgar Hewett: Chauvenet 1983:135–138). Hewett, after initially assisting its formation (Nelson 2016; Villela 2005), famously feuded with the Santa Fe Art Colony. Those bright young things viewed Hewett as a dotard relic, as the Supreme Leader might say. (It does not pay to get old.) The Taos art colony (firmly separate from Santa Fe, as the Taos Society of Artists) was all wrapped up in Taos Pueblo—and a few other Pueblos: San Juan, San Ildefonso, Laguna. Their mystical fascinations eclipsed Hewett’s scholarly approach. For example, writer Mary Austin (AKA “God’s Mother-in-law”): “Regarding herself as something of a prophet, [Austin] envisioned an American acculturation that she termed ‘Amerindian’ in which an evolving American
culture combined with Native American and Hispano rootedness to produce a new cultural synthesis” (Tobias and Woodhouse 2001:97). “She was especially interested in anything that might throw light on the [Pueblo] ‘psychology of communism’...in equating the practice of communal landholding among Pueblos with something she might describe as ‘communism’” (Chauvenet 1983:140). D. H. Lawrence, a somewhat reluctant member of the Taos circle, loved the Southwest but not so much the Pueblo Mystique: “The Indian bunk is not the Indian’s invention. It is ours” (quoted in Scott 2015:188). See also Traugott 2012:Chapter 5; and Scott 2015; for a concentrated dose, Wood 1997; Mather and Woods 1986.

The Indian Market symbolizes the shift of Santa Fe style to Pueblo style. The first market—the Indian Fair—opened under the overall direction of Hewett in 1922 as an adjunct to the increasingly Puebloan Santa Fe Fiesta. At first a sideshow, today Indian Market (as the Fair became) vastly overshadows the older Fiesta. Hewett bowed out, or was forced out in 1927—“his role as culture and art arbiter in Santa Fe was being usurped by a lively and growing Anglo artistic community” (Bernstein 2012:75).

51. “Hewett’s work on the 1906 Antiquities Act and its subsequent passage by the U.S. Congress set the stage for the creation of the Southwest. Hewett recognized early that the antiquities of the Southwest were unique and deserved preservation and study by professionals” (Thomas 1999:132).

52. Hewett is under-biographed. One book-length study, Chauvenet 1983, veers close to hagiography. Hewett’s personal papers disappeared—rumor has it, burned at his request—but he appears, sometimes obliquely, sometimes acutely, in contemporary media and in the memoirs of his contemporaries; e.g., his lieutenant Ken Chapman (Munson 2008). He had as many detractors as admirers. In those polite Victorian times, if one had nothing nice to say, one said nothing; so Hewett is also curiously absent from or distant in many published reminiscences. But unquestionably Hewett was the central figure in the archaeological tilt of Santa Fe and Santa Fe Style, which begat Pueblo Style.

Hewett’s centrality in the invention of the Southwest and Santa Fe was the subject of several articles—my favorites, Richard Frost’s (1980) “The Romantic Inflation of Pueblo Culture” and George Stocking’s (1982) “The Santa Fe Style in American Anthropology”; and even a dissertation, Jeffrey Thomas’s (1999) dissertation, “Promoting the Southwest: Edgar L. Hewett, Anthropology, Archaeology and the Santa Fe Style”; here, a sample:

Hewett’s work, and the work of others in the Southwest, transformed the region into a place of imagination, a colony for artists, a place where Native Americans and Hispanics lived according to cultural norms that pre-dated the United States. Research has seldom been used so effectively to influence culture. (Thomas 1999:2)

53. One of the biggest factors in Hewett’s disagreements with the northeastern establishment was his status as a booster of the region. Hewett used the archaeology of the region to publicize it as an area of considerable interest to tourists and tourist-centered enterprises. Chief among these booster activities was the development of the “Santa Fe Style”...Hewett therefore used the science [of archaeology] as a method of promoting Santa Fe and the American Southwest. (Thomas 1999:48)

54. Since Santa Fe’s reinvention in the 1920s, “City Different” has been its official nickname. I’ve heard Santa Fe called other things: “The place where old archaeologists go
to die”—an anonymous quote from a retired archaeologist who is today alive and well in the City Different. And “a geriatric theme-park”—another anonymous quote, because I never met the hipster who wrote angrily to the Santa Fe New Mexican after an octogenarian’s Escalade crunched his bicycle. (The bicycle was parked and riderless.) Santa Fe is like other invented places: Beale Street or the French Quarter. But Santa Fe is older and richer and sober. NOT a party town like Beale or Bourbon or Duval.

55. An index of disdain: New Mexico (along with Arizona) was the very last state admitted in 1912 into the Lower 48. North Dakota and Idaho—not exactly hubs of civilization—reached statehood two decades before; even the Indian Territories in Oklahoma became a state five years before New Mexico was finally, grudgingly allowed to join the club.


57. Hard times for the City Different: “In 1912, the city may have seemed at the ebb of its fortunes…. [T]he city no longer enjoyed the preeminent stature it once held. Santa Fe lost much of its commercial hegemony when the Atchison Topeka and Santa Fe Railroad bypassed the city for Albuquerque in the 1880s, and Fort Marcy’s military reservation closed in the 1890s…. [T]he city had not prospered. The population had been dwindling since a peak in 1880, and by 1910, the United States census count indicated the city had just over 5,000 people” (Moul and Tigges 1996:138).

58. Albuquerque got the university, Santa Fe got the penitentiary. In 1908, the University in Albuquerque flirted with Pueblo Revival style, lathering mud-brown stucco over Victorian Hodgin Hall—to the horror of Albuquerque’s civic leaders, who rose up and brought down the university president, William Tight, deemed responsible for unacceptable primitivism in modernizing Albuquerque. President Tight’s Pueblo Revival at the university was a flash in the pan: wrong time, wrong place. John Gaw Meem later, in the 1930s, Pueblo’d up the place, and won awards for his work.

59. Santa Fe’s reinvention as a cultural and historical center began before Hewett, with the creation of a historical society which really came into prominence in 1880, with archaeology in its portfolio: “to snatch from oblivion the wonderful evidences of the prehistoric peoples of the Southwest” (Tobias and Woodhouse 2001:50, quoting the inaugural address of the historical society on February 21, 1881).

60. If not Anglo history, then maybe Anglo art? In the 1910s and 1920s (long after the Santa Fe Ring), northern New Mexico became famous/notorious for its artists’ colonies, which grew like weeds (planted by Hewett; Nelson 2016, Viella 2005) in Santa Fe and Taos (e.g., Scott 2015; Traugott 2012). The air was thick with artistic temperament: Bohemian, unruly, avant-garde—and not to most peoples’ taste, until they were romanticized (and collected) decades later. Polite readers back east might enjoy being titillated by artists’ escapades—think: Mabel Dodge Luhan and Georgia O’Keefe—but artsy scandals did not fill trains, hotels, and restaurants.

61. Stensvaag (1980) recounts how the Historical Society of New Mexico began as a pioneer booster club and then graduated to the old Spanish families, before Hewett stocked its board with his creatures and folded it into his vision of early Colonial and then Indian heritage.

62. For an excellent architectural review, see Chris Wilson’s (1997) The Myth of Santa Fe: Creating a Modern Regional Tradition. And for proceedings of conferences which brought Southwestern archaeologists and architects together: Markovich, Preiser and Strum 1990; Morrow and Price 1997; and Price and Morrow 2006. Today of course Santa Fe has strict guidelines for architecture in its historic core; preservation has become a mantra if not a mania “The preservationist/conservationist is emerging as
one of the region’s heroes, and his or her identity represents a continuing romanticism of the Native American ethos” (Francaviglia 1994:35–36).

It worked: *Santa Fe Style* is the name of a twice-yearly magazine published by Sotheby’s International Realty; each issue a glossy 100-plus page catalog of three-million-dollar mud-brick homes. Once, when we were fortunate to occupy the Jack Lambert House at the School of American/Advanced Research, a colleague from cosmopolitan Mexico City visited. We showed him our (temporary) house, proudly pointing out the exposed vigas, the corner fireplace, the Saltillo tiles. This, we bragged, is how ricos live in Santa Fe! Ah, said he, searching from something nice to say: “*rustico, rustico*—many houses in Mexico now reuse old church doors and woodwork.”

People in Santa Fe spend enormous amounts of money to live in peasant houses—amenitized and wine-cellar, but still the architecture of *campesinos* and *labradores*.

63. Hewett was the director of archaeology and anthropology exhibits at the San Diego Panama–California Exposition. “The Santa Fe railway spent a quarter of a million dollars at the San Diego fair creating an entire Pueblo apartment house ‘as seen at Taos and Zuni’ complete with Indians making pottery” (Frost 1980:58).

64. History—faux and real—won out over modernism, or at least a brand of early urban renewal then called “City Beautiful” (Moul and Tigges 1996). With only a few nods to City Beautiful, Santa Fe chose City Adobe.

65. For the new version, Hispanics dressed as conquistadors and hidalgos. “The revived Fiesta celebration of 1919 was a turning point for the festival. The Fiesta was moved from its Fourth of July date to September. The Museum of New Mexico and School of American Research, made up of eastern-educated Anglos, organized the event. The Fiesta focused on the three dominant cultures of Santa Fe with a day devoted to each. Although the Fiesta was incorporated into the Museum of New Mexico’s cultural re-vival effort, Hispanic participation dropped sharply” (Lovato 2004:49). By the late 1920s, many events were limited by fees and tickets, and “the infamous Zozobra” (Lovato 2004:49), a gigantic puppet invented by the anti-Hewett art community, as part of “Pasatiempo,” a counterculture counter-Fiesta. With the passing years, Zozobra’s climactic demise (he goes up in flames) came to dominate the proceedings. The Indian Market—now a huge event in Santa Fe—began as a small side show to Fiesta in 1922, selling curios; “overshadowed by mariachi music and the Mexican flavor of Santa Fe Fiesta, in 1962 the Indian Market finally moved to its own August weekend” (Bernstein 2012:108). By the final decades of the twentieth century, Indian leaders began to question any Pueblo involvement in a celebration of *la reconquista*. The Fiesta has, in large part, gone back to the local Hispanic community. See C. Wilson 1997:181–231 and—for more recent takes—Bernstein 2012 and Horton 2010.

66. We’d just fought a war with Spain, and we were busily governing colonies we’d judged Spain unfit to govern. For the generation that came through and immediately after the Splendid Little War, the Black Legend was real and contemporary, stoked by yellow journalism’s run-up to war. Outside New Mexico, the southwestern Spanish were belittled as semicivilized (consider Lummis’s sensational accounts of the Penitente brotherhoods) while Indians—Pueblo Indians in particular—were promoted to Noble Savage (a formulation I owe to Severin Fowles).

67. There were other tribes in New Mexico, but the Navajos and Apaches had very recently been our enemies. Geronimo, whom we met above, kept half of the US Army busy until 1886. His sins, real or imagined, stained “Apache” generally. He became an attraction at fairs and exhibitions—and for his notorious past, “the worst Indian that ever lived.” Quite a contrast to the Pueblos. While it would be wrong to call the Pueblos docile—there were many times in the history of each Pueblo when they
demonstrated very practical politics—Pueblos were “tame” (not civilized, but tame) compared to the “savage” Apaches and Navajos. Or so the logic went, and thus they were displayed at fairs and expositions: The fierce old Apache warrior and the industrious, peaceful Pueblos, side-by-side for your consideration.

68. Hewett’s elaborate displays of Indians at the Santa Fe Fiesta and the Southwest Indian Fair tended to reinforce their separate, highly romanticized image in spite of his belief that the events were helping to carve a place in American society for Indians. That these were commercial ventures, in spite of their assertions to the contrary, cannot be denied. Indian cultures were put on sale as a means of educating the public, and ultimately reforming an ethnocentric Indian policy determined to destroy Indianness as a precondition for assimilation. The aim of the new policy initiative was self-determination for the Indians, but the selling of their cultures by well-meaning white patrons like [Mary] Austin and Hewett seems to have compromised this intention…. The abundant paternalism of the movement was matched only by the competition and egotism of those involved. While Austin and Hewett professed to have the Indians’ best intentions at heart, their constant assertions that they were the only ones who really knew the Indians and that their organizations were the only legitimate ones dedicated to saving Pueblo cultures undercut the thrust of their work and brought confusion to the reform campaign. Had Austin and Hewett been able to set aside their egos and focus on their common interests, they may have garnered more influence on federal policy. (Meyer 2001:206–207)

69. Archaeology’s prominence in this round of myth-making is highlighted in a chapter of a standard history of Santa Fe: “The Search for a New Direction: Archaeology, Health and Civic Organization,” chapter 4 in Tobias and Woodhouse 2001:49–67. “By the middle of the first decade of the twentieth century, local attractions [old Hispanic administrative buildings, churches, etc.] crystallized into a more comprehensive picture. By then archaeology had moved to the foreground” and “archaeology loomed as a major path for Santa Fe’s cultural development” (Tobias and Woodhouse 2001:70–71).

70. “The central players in the partnership involved in building the museum included four prominent Santa Feans: lawyer and rancher Frank Springer, who was president of the Maxwell Land Grant and who did legal work for the Santa Fe railway; journalist turned banker Paul A. F. Walter; archaeologist and culture promoter Edgar Lee Hewett; and Hewett’s able assistant, commercial artist Kenneth Chapman. Their alliance brought together financial, legislative, cultural and artistic interests” (Traugott 2012:96)—as well as unexplored connections to journalism and the AT&SF!

71. Later the School of American Research, and even later the School for Advanced Research. Santa Fe beat out Mexico City, Los Angeles, Albuquerque, and even Boulder, Colorado (!) for the site of the new School of American Archaeology. Hewett was quite a salesman.

72. In 1905, the AIA named Hewett its second “Fellow in American Archaeology,” following literally in the steps of Adolph Bandelier (Thompson 2000:308). The School of American Archaeology was initially sponsored by the Archaeological Institute of America, which wanted Hewett to found the institution in Mexico City. AIA soon cut their ties with Hewett and Santa Fe.

73. “Hewett and Lummis became collaborators in the development of the image of the Southwest, a task that both undertook with zeal” (Thomas 1999:129–30). Lummis was not as bedazzled by the Pueblos as Hewett and others; Lummis “did not create the
latter-day sentimental adulation of the Pueblo Indians, but he drew American attention to these enticingly different people…. The influence of his writing was immense” (Frost 1980:56–57).

74. Santa Fe didn’t pretend to BE a Pueblo, of course. It began as one, when the Spanish capitol was relocated atop a Native town. But real Pueblos surrounded the town, and were easily accessed in situ. The AT&SF ran right through three of them.

75. There were also immediate frictions with the existing historical society, which Hewett did not run; “The major focus of the society had been New Mexico history [that is, Spanish and Anglo]; that of the archaeologist was Native Americans. The shift of emphasis may have fostered concern among Hispano intellectuals that their heritage was being slighted” (Tobias and Woodhouse 2001:57).

76. “The infusion of Pueblo forms in the mid-teens added compositional irregularity and, for Anglo-Americans, a more fascinating, non-European form of the exotic” (C. Wilson 1997:145). Pueblo style was taken to caricature: A new state capitol proposed in the 1960s to replace the old Victorian (that had supplanted the Palace); it was round, inspired by Pueblo kivas translated into modern materials. When its design ran into opposition, its roundness was justified by its architect as “very old in the history of New Mexico, having its origin in the pueblo Kivas,” and the thing was built: Round like a kiva, but with a nice Territorial trim. Consider Santa Fe’s Inn of Loretto: The form is explosively Pueblo—even more Taos than Taos—but its name is conventual. And its stairway miraculous, unlike the omnipresent “kiva ladders,” second only to silhouetted coyotes as a Santa Fe cliché.

77. So Manitou Cliff Dwellings (at Colorado Springs) and NaTeSo Pueblo (in Indian Hills) imported Indians from various Pueblos—in the case of Manitou, forging relationships and bonds that have lasted for decades. I’m told there are/were Pueblo families living in Colorado Springs who moved there for Manitou.

Hewett was an unofficial sponsor, or at least supporter, of Manitou Cliff Dwellings—perhaps a measure of his ambivalence towards Mesa Verde (discussed elsewhere in this chapter). Mesa Verde was hard to reach. Manitou was easy: At the end of the streetcar line in Colorado Springs, itself linked by rail to all points east. Why wreck your lower back on a buckboard bouncing into Mesa Verde, when you could see it all at Manitou for a nickel and sleep in a decent hotel? Someone should write a history of Manitou; for now, see Lekson 2009b and Lovato 2007.

78. Writing about a “New World Culture Type” which encompassed and characterized all Native societies of both North and South America, Hewett declared them all communal and essentially egalitarian:

It is the glory of the American Indian race that it developed a type of government entirely different from that of the European and more effective. The welfare of the people was the supreme end of government. If individuals became prominent, they were never personally glorified. In America the idea of monarchy had no place…. We can truthfully say that these surviving Pueblo communities constitute the oldest existing republics…. The people managed their affairs through chosen representatives…. The Pueblos exemplify… the community type of social structure as distinct from the state or national type. (Hewett 1930:42, 72)

Neil Judd said much the same about Pueblo Bonito, in a 1925 National Geographic article: “an experiment in democracy—an experiment which ripened into full bloom and then withered—a full half-millennium before our Pilgrim Fathers dared as similar venture on the bleak coast of New England” (Judd 1925:262).
79. Richard Frost (1980:5) continues: “Their [Pueblos’] positive qualities had grown larger than life. They were admired as ceremonialists and artists. Their pottery was sought by discriminating connoisseurs and curio-hunters. The beauty of their villages was interpreted in oil paintings displayed in prestigious eastern art galleries. Books and magazines sympathetically portrayed Pueblo life, and the style of their architecture inspired the remodeling of the capital city of New Mexico. The Pueblo Indian romance, a generation in the making, was fully ripe.”

80. Art and Archaeology 9(1) (1920) contains a half-dozen articles assembled by Hewett which show the early marketing of Pueblo aesthetics, the Santa Fe Fiesta, the Santa Fe/ Taos art scene, and so forth.

81. Fred Harvey (who deserves a chapter here, but won’t get it) and his Indian Detours (Dilworth 2001; Dilworth and Babcock 1989; McLuhan 1985; Weigle 1989). For more on AT&SF’s role in creating the Pueblo Mystique, see Weigle and White 1988:52–65, and chapters in Weigle and Babcock 1996.

82. See, for example, Dorsey 1903. George Dorsey was the Curator of Anthropology at Chicago’s Field Museum. The AT&SF commissioned and published a popular book, “Indians of the Southwest,” a guidebook along the AT&SF route. After steaming through a few Plains tribes, the tour begins—where else?—in Santa Fe. All of the Pueblos are described, and advice offered for how and when to visit. “For several of the more important groups of ruins of the Southwest, Santa Fe forms a convenient starting-point” (Dorsey 1903:53). The book contains useful names and references (hotels, trading posts, etc.) for reaching distant Mesa Verde and Chaco. After many chapters on Hopi, and shorter considerations of Navajo, Apache, and Piman peoples, the train rolls into southern California and those tribes get a closing chapter. All aboard!

83. Hewett’s tireless promotion of New Mexico’s ancient ruins annoyed some of his contemporaries, who wanted to promote New Mexico’s and Santa Fe’s other charms and enchantments (Thompson 2000:280)—which, beyond dry air for invalids, were few in the early decades of the twentieth century.

84. A campaign started by Alice Fletcher Cunningham and Matilda Cox Stevenson; Hewett soon took over as the Pajarito Park’s most ardent and steadfast promoter. Thompson 2000:275–284. See also Altherr 1985.


86. The use of the Native American past as a source of Anglo American identity, however, meant that a specific rationale for the value of Southwestern antiquity to the modern inheritors of the land had to be created. The “archaeology” that emerged from this debate, manifest in the activities of author Charles Lummis, archaeologist and educator Edgar Lee Hewett, politician-scientist Frank Springer, and their contemporaries, disavowed the exclusively scientific agendas of professional anthropologists in favor of one that combined science, education, and regional pride. Largely ignored by modern scholars, the idea of archaeology as [regional] heritage spread widely through the region between 1900 and 1920 and played a central role in the construction of a new identity for the Southwest. (Snead 2002:19)

87. The Mystique affected a range of American intellectual and artistic life, in small but important ways. Leah Dilworth, for example, in her 1996 Imagining Indians in the Southwest: Persistent Visions of a Primitive Past, recounts the impacts of the Mystique
(not her word) on modern art and poetry. Chris Wilson chronicled the architectural history in *The Myth of Santa Fe: Creating a Modern Regional Tradition* in 1997. Eliza McFeely in 2001 traced *Zuni and the American Imagination* in literature and letters. Jerold Auerbach, in 2006’s *Explorers in Eden: Pueblo Indians and the Promised Land*, traced how the nineteenth-century writers rendered Pueblos in Biblical terms, with a recursive conflation of Pueblos with the ancient Holy Land. More recently, Sascha Scott viewed the matter through the fine arts, in a remarkable 2015 study, *A Strange Mixture: the Art and Politics of Painting Pueblo Indians*. I list these titles not to dress windows, but to make this point: Most graduate students training to work in the Southwest have never read them; instead, we insist that they be familiar with the works of French social philosophers (whom we meet, unavoidably, in chapter 6).

88. A famous public intellectual, like her friend Margaret Mead. Mead wrote Benedict’s biography in 1974; among many others, thereafter: Caffrey 1989; Modell 1983; and Young 2005.

89. From *Patterns of Culture*:

The Zuñi are a ceremonious people, a people who value sobriety and inoffensiveness above all other virtues. Their interest is centered upon their rich and complex ceremonial life.... Their prayers also are formulas, the effectiveness of which comes from their faithful rendition. The amount of traditional prayer forms of this sort in Zuñi can hardly be exaggerated.... The heads of the major priesthoods, with the chief priest of the sun cult and the two chief priests of the war cult, constitute the ruling body, the council, of Zuñi. Zuñi is a theocracy to the last implication. Since priests are holy men and must never during the prosecution of their duties feel anger, nothing is brought before them about which there will not be unanimous agreement.... Personal authority is perhaps the most vigorously disparaged trait in Zuñi. A man who thirsts for power or knowledge, who wishes to be as they scornfully phrase it “a leader of his people” receives nothing but censure and will very likely be persecuted for sorcery.... He avoids office. He may have it thrust upon him, but he does not seek it.... The lack of opportunities for the exercise of authority, both in religious and in domestic situations, is knit up with another fundamental trait: the insistence upon sinking the individual in the group. In Zuñi, responsibility and power are always distributed and the group is made the functioning unit.... Just as according to the Zuñi ideal a man sinks his activities in those of the group and claims no personal authority, so also he is never violent. (Benedict 1934[1989]:59, 61, 67, 98–99, 103, 106)

Benedict’s Zuni had a tilde over the “n”; for consistency in this book, I’ve de-tilded her Zuni. Denver has a major street that traffic reporters pronounce zoon-AYE. And another that’s AY-coma, the latter half like the medical condition. No kidding.

90. By 1974 1.6 million copies had sold (Mead 1974) and new editions appeared every few years thereafter. It’s never been out of print, and currently (March 2018) ranks no. 148 among all anthropology books on Amazon—eighty years after it was first published!

91. While Benedict’s anthropology has fallen out of favor among professionals, in the words of one biographer, “Patterns of Culture would be named a classic by most anthropologists” (Young 2005:1). Several lengthy biographies are academic books, an indication of her importance within the discipline, and there can be no question of her impact on the broader audience. I make this declaration because my focus on Benedict has been questioned by anthropologists who blow her off as a poor ethnologist and a failed theorist. She may have been both, but she carried much clout.
92. Consider Hopi: Peter Whiteley (1998:12) summarizes the vast "para-ethnography on Hopi—often garnering a wider audience than the writings of anthropologists—has proliferated since the late nineteenth century" in “a variety of genres.” For example: “Popular art films (Godfrey Reggio’s *Koyaanisqatsi* and *Powaqatsi*) relocate simulated Hopi representations in a truly postmodern play of extraterritorial transmutations. The immediate antecedent for the latter is [Frank] Water’s *Book of the Hopi*, probably the most widely selling book on Hopi culture, despite its notorious confabulation of fact and imagination…. At present (summer 1997) controversies—especially at Third Mesa over the publications of New Age savagist Thomas Mails (1996)—reproduce long-standing dislike for Water’s work in particular. The formal ethnographies, with their guild jargons, at least have the virtue of being less squarely in the public eye.”

93. Stocking (1989:222) quoting Margaret Mead: “After armistice brought an end to ‘this tornado of world-horror,’ when it had ‘seemed useless to attempt anything but a steady day-by-day living,’ Benedict searched once more for expedients to ‘get through these days.’ Although she rejected the advice of a friend to ‘move to the [Greenwich] village and have a good time, and several love affairs,’” Benedict instead turned to the consolations of Anthropology and, eventually, Zuni. She was enthralled by Zuni. One of Benedict’s several biographers described the impression Zuni made on Benedict, who was at that time also a published poet: “Her poetry hinted at the extent to which Ruth had succumbed to the Southwest, as had others before her—ethnographers, travelers, poets, and artists. In poems inspired by the Southwest, Ruth as last ‘loosened’ the ‘psychic machinery,’ a change she acknowledged in prose on her second fieldtrip” (Modell 1983:172). “The Pueblo setting filled Ruth Benedict with close to a religious feeling…. She said, ‘When I’m God I’m going to build my city there’” (Modell 1983:173). Hook, line, and sinker: Pueblo Mystique.

94. The influence of Indian policy on the creation of the Pueblo Mystique would need another book, yet to be written. Sascha Scott (2015:90–94) makes the case that the tensions between government assimilationist policies and preservationist philosophies among the White supporters of the Pueblos contributed substantially to the now-legendary Pueblo conservatism; that is, the Pueblo Mystique of extraordinary conservatism came at least in part from politicking and lobbying for the preservation of Pueblo culture. The anti-Dance policies of US government assimilationist policies in the 1920s drove Pueblo ceremonialism literally underground, a repeat of Spanish colonial repression. See Scott 2015:132–137. Note, this was in the 1920s—relatively recent times.

95. “Collier and other advocates for Pueblo culture explicitly tied Indian salvation to white salvation, seeing Pueblo culture as a positive counterexample to American society’s materialism and spiritual corruptness” (Scott 2015:99).

96. Collier’s *The Indians of North America* in 1947 and *Patterns and Ceremonials of the Indians of the Southwest* in 1949. I was flabbergasted to see Collier as the prime example listed on Wikipedia of “Postmodernist Anthropology.” (Geertz and Margery Wolf get tangential nods; accessed Sept. 23, 2017.) You should be flabbergasted that I consult Wikipedia.

97. From Collier (1949[1962]:33):

> What these seekers from all over the world pursue, and find, is various, and includes all that North Americans seek and find. It includes archaeology, and the “living archaeology” of tribes whose dynamic past is moving them today; crafts and arts, and the organizational techniques of developing ancient crafts
and arts to modern use while conserving, even increasing, their traditional inspirations.... the saving of the land from water and wind erosion, a “number one” problem of our whole planet, in whose solution a number of the Southwestern tribes are leading the world; and such vistas of psychology and metapsychology as drew Carl Jung from Zurich to Taos Pueblo.

98. “Not strong central authority, but that social genius of pluralistic holism, which Cushing described at Zuni, has kept the Pueblos undissolved while ages of time, and wars, famines and forced migrations have rolled over them. Forcibly dominating central authority is contrary to that particular kind of social genius, a genius of freedom within complex and reciprocal structure, which has enabled the Pueblos to outlast all else besides. And their great political-social significance today is their achievement of freedom within order, order within freedom, and pluralistic unity” (Collier 1962:155).

99. Richard Frost (1980:59) summarized Collier’s vision of the Pueblos:

They embodied the ancient wisdom of tribal man, whose strength lay in the submergence of ego-identity to communal identity, in social reciprocity, artistic creativity, and aesthetic intercourse with the cosmic powers, because they believed the universe was a “living being” that required the sustained will of man for survival. Education, personality, and social institutions were all shaped towards these ceremonial, cosmic ends; the effect upon the individual was not confining but liberating. This “spiritual culture,” according to Collier, was as old as Paleolithic man. In its antiquity, the Indian sense of time transcended the past and future alike; the “enduring past and enduring future” were conjoined, eliminating “linear, chronological” time.... The Indians were one with the land, not exploiters but “co-workers with it; they believed they were eternal as it was eternal.” Organizationally, the Pueblos were sophisticated, self-governing bodies, deeply democratic and self-disciplined, free of commercial motives, class subjugation, and the subservience of women. Thus they were morally superior to Greek city-states. Their lives were constantly expressed through symbolic art—through ceremonies, dances, songs, myths, masks, pottery, weaving, and painting.

100. The Hopi literature is vast. Laird’s (1977) Hopi Bibliography lists almost three thousand entries—and that was forty years ago!


102. “If the San Francisco–based hippies had a favorite tribe, it was the Hopi—likely reflecting that a few of them had read Frank Waters’ Book of the Hopi and that some Hopi people willingly, at least initially, interacted with the Anglo-seekers” (S. Smith 2012:65).

103. Waters himself is a field of study; e.g., McLeod 1994. There once was a Frank Waters Society and there still is a Frank Waters Foundation. http://www.frankwaters.org/.

104. Connections between Haight-Ashbury and Hopi generated a (small) scholarly literature; for example: Geertz 1994; McLeod 1994; McCaffery 2005; S. Smith 2012.

105. “Chachunga [Hopi leader] was gracious but urged the hippie assemblage to go back to their homes and be careful on the highway. He was old, needed his rest, and was going home himself.... But the critics could not kill the sentiments at the heart
of the project. The cultural appeal of Indianness was just getting started” (S. Smith 2012:74).

106. “...be Happy” attrib. to Meher Baba, Guru to the Stars. “...be Hopi” attrib. to Janice Day, the gracious proprietress of the Tsakurshovi Trading Post.

107. “In the present literary canon of the New Age, Book of the Hopi...has acquired widespread distinction as a sacred text, a foundational document which affirms non-Native rights of access to Native American religious cultural property” (McLeod 1994:301).

108. “In the proliferating literature the New Age, the works of Thomas E. Mails, a non-Native member of the Lutheran ministry, are singular for their advocacy of non-Native appropriation of Native American religious performance traditions.... [He] offers a do-it-yourself paho-making ceremony and kachina dance for non-Native readers and, elsewhere in the text, a mail order form for make-your-own paho kits” (McLeod 1994:301).


In keeping with nineteenth-century theories of cultural evolution, it was assumed that knowledge acquired about extant Southwestern societies could be “reversed,” so to speak, and used to interpret archaeological materials. In this way—working from the known to the unknown, from the living to the dead by means of analogy—prehistoric cultures could be identified and connected with their surviving descendants. Ethnology and archaeology were opposites sides of the same coin. Concentrating mainly on Puebloan groups, early ethnologists seized upon information contained in myths and so-called migration legends to construct elaborate hypotheses about the origins of basic forms of social organization [citing Cushing, Bandelier, Fewkes, and the two Mindeleffs]. Although most of these hypotheses were subsequently rejected as untestable and unduly speculative...a few workers, following Cushing’s lead, combined excursions into conjectural history with lengthy descriptions of ongoing cultures. (Basso 1979:15)

110. A. V. Kidder, in his seminal Study of Southwestern Archaeology:

A review of the subject matter of Southwestern archaeology must necessarily begin with a consideration of the still inhabited pueblos of New Mexico and Arizona. These fascinating communities preserve the ancient culture of the Southwest almost in its aboriginal purity; even the most sophisticated of them are little more than veneered by European civilization, and we can still, as Lummis has so aptly phrased it, “catch our archaeology alive.” (Kidder 1924[1962]:142–144)

111. The canonical Classic Ethnographies: Are they accurate? Hang around Pueblos long enough and you will hear tales about anthropologists, from Cushing to White, and how their informants fed them...rubbish. These tales may or may not be true, but one anthropologist’s compressed experiences at a particular Pueblo at a particular time seem like a shaky framework upon which to build pre-history. This is not to devalue the Classic Ethnologies, but to insist that we cannot treat them as gospel. Recall the unpleasantness between Goldfrank and Benedict, two ethnologists in the Golden Age who saw very different things in the Pueblos (Bennett 1946). And see Young 2005.

112. “Standard texts from ethnology’s Golden Age were cited to prove or support very contradictory accounts of the ancient past—a scholastic enterprise which, consequently, most archaeologists avoided” (Spielmann 2005).
113. I offer no citations, nor need I. I myself am of a certain age and (as we said back then) I watched it all go down.


115. Whiteley 1998:7: “If Hopi ethnography has come to a sort of end, the trajectory of its demise may be of more than ordinary interest, since anthropology practically begins at Hopi and Hopi is substantially represented, both descriptively and analytically, in virtually every theoretical paradigm since Morgan’s evolutionism…. Since then, a partial list of those who have conducted at least some ethnographic work at Hopi reads like a metonymic who’s who of the earlier disciplinary history within the United States.”

116. Cushing was among the first of a long, long line of anthropologists who felt their curiosity was more important than Zuni’s privacy; Pandy 1972.

117. Regarding the Direct Historic Approach, its most famous proponent, Julian Steward, had this to say about the Southwest:

> It is, in fact, a striking commentary on the divergent interests of archaeology and ethnology that in the Southwest the gap represented by the four hundred years of the historic period remained largely unfilled, while archaeology devoted itself mainly to prehistoric periods, and ethnology, to the ceremonialism and social organization of the modern Pueblo. And yet it was during this four hundred years that the Pueblo had contacts with one another, with the nomadic and seminomadic tribes, and with the Spaniards, that account for much of their present culture.” (Steward 1942:338; emphasis added)

118. “He was not an ethnologist—in fact, the distinguished anthropologist A. L. Kroeber considered Hewett’s weakness in ethnology a serious flaw in his archaeological skills—but he learned over the years from the Indians” (Frost 1980:58). “From Hewett’s perspective, archaeology was augmented by ethnology, not vice versa” (Snead 2005:30).

119. Lummis (1905) used “catch our archaeology alive” in reference to California folk music. “Archaeology” was elastic, back then; and, as today, susceptible to a well-turned catchphrase (as it were).

120. In his 1929 essay “The Present Status of Archaeology in the United States,” Judd observes, “Although we have come to think of them as separate fields for investigation, it is impossible absolutely to divorce archaeology and ethnology” (Judd 1929:410), but he goes on to chide ethnology for ignoring material culture: “In our ethnological researches, study of material culture has rather gone out of fashion during the last quarter century; emphasis has been placed on fast-disappearing languages, ceremonies, and social organizations” (Judd 1929:404). Indeed, for Judd (trained in Classics, not ethnology):

> As we interpret it in this country, archaeology has to do with prehistory. Where written history begins there archaeology ends. Archaeology seeks to supply the text for those chapters that obviously preceded historical beginnings. Archaeology is the backward extension of recorded history!” (Judd 1929:401)

121. Ethnology was OK with that: Archaeologists might be half-baked ethnologists, but if they pasted Pueblos back on the past, they probably would do no harm. Pueblo ethnographer Edward Dozier, himself Tewa with a PhD from UCLA, summed it up: “All of the structures now found in the Pueblos existed in prehistoric times” (Dozier 1970:209).
122. Maybe the second Golden Age. It could be argued that the late nineteenth-century pioneer period—Cushing, Fewkes, Stephens, Bourke et al.—was Golden; but was it ethnology? Not the way the ethnologists went at it in the 1930s and 1940s, with anthropology up and running and fully stocked with methods, jargon, and attitude.

123. Barbara Babcock (1990), in the introduction to a collection of essays titled *Inventing the Southwest* addresses the ethnological bias: “I know that Ruth Benedict was not alone in her characterization of the Pueblo, that there is a romantic, nostalgic attitude on the part of anthropologists as well as artists which sees the organic character of preliterate life as preferable to the heterogeneity of modern life.”

“In the case of Southwestern ethnology,” John Bennett pointed out (in his contribution to *Inventing the Southwest*), “these tendencies may often assume a special form conditioned by the pervading sense of mystery and glamour of the country itself. A good deal of ethnology and archaeology in the Southwest has been done with a kind of eager reverence for turquoise, concho belts, Snake Dances, and distant desert vistas” (1946:364–65). Bennett’s article in *Southwestern Journal of Anthropology* is well worth reading for ethnology at mid-twentieth-century: He noted two distinct ethnological visions of the Pueblos, the dominant being Benedict’s happy-peaceful/etc. idealization, the other Esther Goldfrank’s (1945) grittier, sharper, less pleasant (for us) view of Pueblo culture: “These two interpretations have appeared not entirely as explicit, formal, theoretical positions, but more as implicit viewpoints” (p. 362). “Implicit viewpoints”: The Pueblo Mystique in ethnology? Bennett continues: “What is perhaps most interesting—and not a little amusing—is that these controversies, so plainly a matter of value and preference, endure as long as they do without some objective attempt to sit down and realistically arbitrate the matter. The Puebloists have been firing their respective interpretations back and forth for a decade, yet none have seen fit to dig into the real issues—at least in print. It is not, perhaps, an easy thing to do, since it requires a good deal of self-objectivity and humility.” Benedict won the “debate” with Goldfrank—but there was a lot of *ad hominem* in the outcome.

124. Spielmann 2005:194–195. “After the ethnological immersion of archaeologists like Fewkes at the turn of the century, southwestern archaeology developed remarkably independent from southwestern ethnology, and often in ignorance of what the ethnographic record entailed” (Spielmann 2005:197). The idea of “Pueblo” was ever-present in the archaeological imagination, but it was an ideal idea, uninformed by the actual nitty-gritty of ethnological research—which, by midcentury, had begun to poke holes in the Pueblo Mystique (e.g., Brandt 1994).

125. Two bombshells burst in the early 1970s: Charles Di Peso’s (1974) *Casas Grandes* and Emil Haury’s (1976) *Hohokam*—Fat Man and Little Boy (the books, not their authors). Di Peso’s eight-volume magnum opus described an astonishing cosmopolitan city just south of our border. Haury’s massive single-volume book described—for the first time, for many readers—the wonders of Hohokam, with startling canal systems, dense populations, ball courts, remarkable art. After those two explosions, the Southwest was no longer mainly or even principally Pueblo—but don’t tell that to Santa Fe! Despite the fact that neither of these fabulous ancient events were Pueblo, the Pueblo Mystique survived and even found application in those strange new worlds: Casas Grandes is called a “pueblo” and Hohokam struggles to be anything but egalitarian—because the Southwest (we know) was intermediate and egalitarian.

126. Prying anthropologists have been replaced by prying artists, prying linguists, prying New Agers, prying environmentalists, and prying tourists. No rest for the Zuni, no peace for the Pueblos. In an old *New Yorker* cartoon, taped to my file cabinet, a nicely dressed matron in hat and gloves and purse wanders into a Pueblo room where a
family sits at breakfast, over their Wheaties. “Oh I beg your pardon,” she apologizes, “I thought you were extinct.”

127. But here’s a sample, anyway:

Some...anthropologists have continued to suggest that the conservatism and tenacity of Pueblo social, political, and religious organizations were relatively unaffected by the pressures of the wider cultural milieu. I suggest exactly the opposite is true.... [I]t is undeniably naïve to suggest that the core of Puebloan organization has remained relatively unaffected by changes that occurred even during the “ethnographic present,” let alone during the periods prior to 1880.... The prominent role of religious ceremonial, the so-called “egalitarian” character of political and economic affairs, the absence of individual wealth, are all fundamentally related to changes induced during the contact period. (Upham 1987:267–268, 272)

128. Gregory Johnson (1989:373) acknowledged the shark-tankiness of Southwestern debates: “Commenting on Puebloan political organization seems to be one of life’s ‘no win’ pursuits. Opinions on the same cases expressed during the [1983] seminar ranged from Apollonian egalitarianism to centrally administered and socially stratified social formations. Whatever you say, some large number of folk will be in vigorous disagreement with you.”

129. See, for example, Cordell 1997, Reid and Whittlesey 1997, and (somewhat later) Kantner 2004; the better angels of ethnology hover over these fine textbooks.

130. One of several Great Divides, really. Just in the later pre-history, 1300, 1450, and 1600 mark the approximate dates of major historical deflections: 1300, the end of Chaco-Aztec; 1450, the end of Casas Grandes and Hohokam; 1600, the arrival of Spanish colonizers. Surely there were others before 1300. I focus here on 1300 but the other hinge points were just as important, historically.

131. There were earlier hints and allegations of un-Pueblo social difference and complexity at Chaco (Altschul 1978; Grebinger 1973; Martin and Plog 1973:270–271). By the early 1980s, your author had convinced himself (Lekson 1984) and maybe a few others that Chaco was a class-stratified society—based on the very obvious differences between contemporary Great Houses and Unit Pueblos (a difference noted with alarm in the 1930s; e.g., Hawley 1934). Similar conclusions were reached, independently, by John Schelberg (1984, 1992) and David Wilcox (1993). Boomers all, with the obvious exception of Paul S. Martin.

132. I arrived in 1950, but a couple of decades passed before I was functional. This section is notably un-footnoted and under-referenced, because I was there. This is my personal version of what happened in Southwestern archaeology during my so-called career. If you don’t like it, write your own.

133. It’s worth noting that these laws were not written by archaeologists; they were written by historians and architectural historians. The four criteria for inclusion in the National Register are: (1) associations with historic events; (2) associations with historic people; (3) archetype of a master architect; and, as a special dispensation for archaeology, (4) sites “that have yielded or may be likely to yield, information important in history or prehistory.” Kind of a catch-all, and it caught all. People nominated lithic scatters to the National Register, until cooler heads prevailed.

134. CRM began at museums and universities; but the magic of the marketplace soon moved CRM to stand-alone for-profit firms or into much larger engineering or environmental businesses. E.g., Doelle and Phillips 2005; Roberts, Ahlstrom, and Roth 2004.
135. That’s nationally, I’d guess the ratio is higher in the Southwest (with huge tracts of federal and tribal lands), perhaps as high as five to one. My data are rag-tag, from various sources with various dates and various degrees of certainty: AAA guides to departments; blogs like “Doug’s Archaeology” (Rocks-Macqueen 2014); dated SAA surveys (Zeder 1997; Association Research Inc. 2011); and correspondence with ACRA and several colleagues, nameless and blameless. I claim no particular precision for these numbers but, I think, some accuracy in the ratios. For what they’re worth, my back-of-the-envelope numbers (and dates of data): Academic, 1,630+ (2012); Federal agencies, 1,550 (2013); State agencies, 850 (2008); CRM, 7,000 (2008); for a total of about 11,030 archaeologists working in the USA. Except for the federal archaeologists and my personal count of academics (which excludes archaeologists working primarily outside the USA) these estimates all occurred pre-2008 crash, which of course impacted CRM.

136. The scale of the natural laboratory was, at most, a small river valley—the right-sized research for a dissertation. And it was generally assumed that everything was local: What happened in your natural laboratory stayed in your natural laboratory. Outside, uncontrolled inputs would scotch the experiment. Small scales—for a river valley is small—may have respected Pueblo Space (small, local, independent) or it may have reflected research budgets. The latter, more likely. The scale of natural laboratories was shattered first by “regional systems” such as Chaco and Hohokam (ably mapped as macroregions by David Wilcox, e.g., 1999; see also Crown and Judge 1991) and then by a renewed interest in migrations, Kayenta to southeastern Arizona and Mesa Verde to Rio Grande (e.g., Bernardini 2005; Cameron 1995; Clark 2001; Ortman 2012).

137. Garland Press, in the 1970s, published about two hundred volumes of Land Claims documents. Among many others, Florence Hawley Ellis wrote on behalf of several Rio Grande Pueblos; her student Alfred Dittert worked closely with Acoma (and is well remembered there).

138. Iowa’s burial protection law passed in 1976—a clear precursor to the later federal legislation. The Slack Farm fiasco came in 1987, and Indian protestors picketed Dickson Mounds around the same time. Archaeologists knew that things were going to change.

139. Other laws important to CRM and “discretionary” field research were also amended in the late twentieth century to include, prominently, consultations with Indian tribes. The key Section 106 of the National Historic Preservation Act requires federal agencies to consult with tribes for projects on and off tribal lands—that is, any project that is subject to Section 106 now involves Indians. The Archaeological Resources Protection Act—under which federal archaeology permits are issued—also requires consultation with tribes as part of the permitting process. NAGPRA, NHPA, and ARPA gave Indians a major role in the regulation of fieldwork on federal lands, or (in most cases) with federal money—in some cases amounting to veto power.

140. And of course all those many individuals may not agree. My university museum was part of a consultation on “Fremont,” jointly with a federal agency; when we arrived in the conference room, it became clear that the federal agency had invited one set of tribes, the university another. Whoops.

141. My well-thumbed copy of Elsie Clews Parsons’s (1996 [1939]) Pueblo Indian Religion is an edition of 1,200 pages in two hefty volumes.

142. Fowles persuasively argues that “doings” is not another word for religion, but may more accurately represents a pervasive system of politics or, rather, a tangle of both. This novel argument may be lost on readers who come to his work with an interest
in Pueblo “religion”—which in this ritually beglamoured age includes, perhaps, most Southwestern archaeologists. (All three back-cover blurbs situate his book as or in relation to the study of “religion.”) Fowles insists that “the analytical divide between religion and politics...[is] entirely untenable for the precolonial period” (Fowles 2013:240–241). I agree...and disagree. Operations and behaviors and institutions we, today, would call “religious” and “political” were almost certainly intertwined, then. But the analytical categories, now, are still useful for us—if we today study “religion” and/or “politics.”

143. I am struck by how archaeologists, who clearly respect Pueblos, insist on our right and ability to chart the course of Pueblo religious beliefs. These are precisely the things Indians tell us are NOT our business, and also things among the most difficult to see, with any certainty, in the archaeological record. But we press on, making remarkable pronouncements about the cosmology, ideology, and rituals of Pueblo ancestors.

144. The model was David Freidel’s (1981). Judge’s application was not precise; for example, Freidel’s model does not involve a central ritual place analogous to Chaco Canyon in Judge’s model (Judge 1989:242). The main parallel between the Maya and Chaco models is that both were ritually driven; that was something new for Chaco. Judge’s model has been dismissed (unfairly, I think); Freidel’s model is still widely if not universally accepted among Mayanists.

Judge was also strongly influenced by Kim Malville (my colleague at the University of Colorado), who studied pilgrimage in India, where pilgrimage centers attract staggering numbers of people (millions!); see Malville and Malville 2001. Few Chaco-pilgrimage adherents today realize that the first and most vocal proponent of Chaco-as-Pilgrimage-Center was a brilliant astrophysicist-cum-archaeoastronomer, importing models from the Subcontinent.

145. Nor in Native Mesoamerica. Joel Palka (2012:54–99) provides a review of Mesoamerican pilgrimage in chapter 2 of Maya Pilgrimage to Ritual Landscapes. Palka 2012:13: “Maya and Mesoamerican pilgrimage, in comparison to pilgrimage traditions elsewhere, does not focus on a few primary ritual sites. A potentially infinite number of sites exist in the Mesoamerican landscape where people communicate with spiritual beings”—much like the Pueblos, as I understand things. Mesoamerican pilgrimages were—like Pueblo pilgrimages—typically small groups going to landscape features (mountains, caves, springs) or to shrines. However, there are ethnographic accounts that “several hundred to several thousand people have participated in single pilgrimages” (Palka 2012:58; famously, the Huichol). A few cities—most famously Cholula—were ancient, authentic pilgrimage centers; again, mostly for small groups at varied times, not mass pilgrimages on the Chaco model. And, notably: Cholula was a city, a great big bustling settlement, not an empty ceremonial center. Freidel’s model of pilgrimage fairs (mentioned above, for Chaco) is central to Palka’s (2012:55ff) review. The Maya pilgrimage fair model does indeed move large numbers of people around on a calendar. Palka (and Freidel) note that such an organized regional system with urban pilgrimage centers presupposes a “universalizing religion” (Palka 2012:55), which unites individuals and attracts them to ceremonies and trade fairs. (As noted above, Judge nodded to this requirement by suggesting a “widely shared religion” in Chaco’s era.) Universalizing religions are generally associated with fairly high-level sociopolitical entities (state, empires, and so forth); NOT in the Pueblo Space, at least until the friars arrived. I’ll give the pro-pilgrimage people their pilgrimage center, if they give me a Chaco state, with a state religion!

An interesting and perhaps important aspect of pilgrimage: Palka’s (2012:60–61) review of the differences between the Old and New World includes Kubler’s (1984)
observation that pilgrimage in the Old World benefits the individual pilgrim (think: Hajj or Camino de Santiago); Kubler and others maintain that while the Mesoamerican individual benefits from pilgrimage, there is also a very strong communal function that is a benefit to the community. So too Pueblo pilgrimages—at least some I’ve been told about. At the same time, “Elites organized pilgrimages and processions, and they were undertaken to affirm community members’ different social ranks and status.” See also Kantner 1996; Wells and Nelson 2007.

146. Pilgrimage penetrated both popular and secondary scholarly literatures; see Fox 1994 for an early example and Brooks 2013 for a more recent example, among many others.

147. Most famously with the work of Anna Sofaer and her colleagues on the Solstice Project, summarized in Sofaer 2008. See also: http://www.solsticeproject.org/about

148. Certainly, it’s ritual. Although for farmers, having a calendar is also an economic necessity. The two—ritual and economy—were OF COURSE intertwined. Who would doubt that, when even in twenty-first-century America, our coinage tells us “In God We Trust”? (God may or may not trust American currency: Didn’t Jesus chase money changers from the temple?)

149. I do not agree, of course. There were earlier exceptions. Lynne Sebastian’s (1992) Chaco Anasazi: Sociopolitical Evolution in the Pueblo Southwest focused on power I would call political, but still respects Pueblo Space, coming down on the side of the angels: “very active leadership…by means of religious monopoly [and] the metaphor of ritual”—Chaco, in the end, was ritual (Sebastian 1992:121–123). John Kantner’s (1996) prescient “Political Competition among the Chaco Anasazi of the American Southwest” argued that political forces played a major role in the creation of Chaco. Kantner later merged politics with pilgrimage (Kantner and Vaughn 2012). Both Sebastian’s and Kantner’s early arguments seem to have been swept away by the rising tide of ritual. Jeffrey H. Altschul (1978) also edged towards the political.

150. Drennan 1999; and more famously in Yoffee, Fish, and Milner 1999:266:

To describe the supposedly non-townish and not really political nature of Chaco, Dick Drennan proposed the term “rituality” to refer to its “ritual boomtown” nature. This neologism is attractive to us, since it is not borrowed from the extensive Western literature on urban geography, and it must be unpacked and explained in its own terms.

What did Drennan have in mind? Something very much like Yoffee’s use—there was no distortion there—but it is worth examining the rather difficult context in which Drennan, as an outside arbiter, found himself. He contrasted Mississippian and Southwest—but two Southests, Chaco and Hohokam, were merged into one. I have argued elsewhere (Lekson 2009) that Hohokam is truly remarkable for its vast public infrastructure in the absence of any easily identified central authority; Chaco, in contrast, is a garden-variety polity, like Dorothy small and meek. But because, at the SW-SE conference, Hohokam was enthusiastically declared a nonstate community (or “comunidades”) and Mississippian Cahokia was happily truncated at the “chiefdom” level, so Chaco must be something similar or lesser:

Whether Hohokam or Anasazi groups can be classified as egalitarian or not, elite personages played a far less central social role than they did in Mississippian societies.... [On the regional scale] [i]t is easier to attach the label polity to this scale of integration for Mississippian, because politics seems
to be the arena in which the leaders who integrated these entities emerged. Politics, in the broadest sense at least, are probably not entirely absent from any human society, but they seem remarkably undeveloped in Hohokam and Anasazi societies. It is tempting to call Hohokam and Anasazi entities at the scale rituals or communalities (to exaggerate [Suzanne] Fish’s use of communities for Hohokam regional societies), thereby emphasizing the ritual, communal, group solidarity on which these entities were founded. (Drennan 1999:256–257, emphasis original)

151. The effort’s shortcomings certainly were not structural, but rather personnel or personal—proclivities participants brought to the conference table (all nice bright people and fine company!). Among the Southeasternists were several notable “minimizers” (as they called themselves in seminar conversations and later in print) while among the Southwesternists were a few upstart “maximizers” (including your author) who saw the Southwest as more complex than then currently acceptable.

Today there can be no reasonable doubt that many Mississippian polities of the Southeast were very-nearly-states and Cahokia clearly belongs in that exclusive Old Boys Club. But, in 1999, voting ran strong against that conclusion; and, if Cahokia was a simple chiefdom, what hope for the Southwest, with its few examples (like Chaco) of maybe-possibly-sorta-kinda-state-like? None: In the end Mississippian polities were planed off at the level of “chiefdom” (in the terms of the time) while Southwestern pretenders—so clearly inferior to Cahokia—were dismissed as something far-less-than-a-chiefdom: “communidades” and “ritualities” (Drennan 1999; Yoffee et al. 1999).

152. Yoffee disliked Neo-Evolutionary schemes. Revisiting “rituality” a few years later:

I used the term “rituality” (adopting the term from R. Drennan; see Yoffee, Fish and Milner 1999) because it seemed to me that Chacoan “complexity” cannot be fit tidily into neo-evolutionary social organization types, such as tribes, complex tribe, big-man systems, chiefdoms (simple and complex), segmentary states, or just plain states, all of which have been employed to characterize the same material evidence at Chaco…. The neologism “rituality” is attractive to me since it must be unpacked and explained in its own context, namely a series of constructions whose place and purpose were concerned with matters of belief and ceremony. (Yoffee 2001:64)


154. The Harmonic Convergence was a syzygy of celestial bodies, predicted to be transformative. Perhaps it was. That was the year of “the drive,” when John Elway took the Denver Broncos ninety-eight yards to beat the Cleveland Browns and go to the Super Bowl, so maybe the Harmonic Convergence worked. But the Broncos lost the Super Bowl, so maybe it didn’t. But in any event, you have learned a new Scrabble word: syzygy.

155. First in articles such as “Ritual Control and Transformation in Middle Range Societies: An Example from the Southwest” (Schachner 2001), about Pueblo I; “Ritual, Power, and Social Differentiation in Small-Scale Societies” (Potter 2000), about Pueblo IV; and—after interest and enthusiasm built up—in edited volumes such as Religion in the Prehispanic Southwest (VanPool et al. 2006) and Religious Transformation in the Late Pre-Hispanic Pueblo World (Glowacki and Van Keuren 2011).
156. Paul Reed summarized the consensus at about 2000:

In contrast to models that view Chacoan society as driven by competition and hierarchy, another new approach to social organization envisions a unified but cooperative endeavor. Chacoan society is seen as the result of an integrated, cooperative, and largely egalitarian effort without significant competition or social hierarchy. In this view, ritual and ceremony are paramount concerns, and ritual specialists or priests are the leaders.... Many Chacoan archaeologists have proposed hypotheses regarding social organization that fall into this general view. This view seems to represent a consensus or, at least, a majority opinion of the scholars gathered for the Chaco Synthesis Conferences. (Reed 2004:50–51 and note 17)

Chapter 2: Chaco in the Twenty-First Century

1. My original intent was to review everything Chaco since 2000. Boy! Is there a lot of Chaco since 2000! This chapter was originally much, much longer—far too long. So I selected only a few books and articles that illustrate particular points, and moved the rest online (https://stevelekson.com/), where my comments and appreciations will continue to appear. That division does not reflect on the quality and importance of publications absent from this chapter, but rather reflects the narrow structure and greedy needs of my argument.

2. Mid-twentieth century, Chaco Park archaeologist Gordon R. Vivian raised this awkward issue in a report on the excavation of Kin Kletso (Vivian and Mathews 1964). Vivian concluded that Chaco was escaping Pueblo Space: Chaco was "not in the direct line of the Northern [Rio Grande] Pueblo continuum...the continuation of the direction taken by the Chaco group would have carried it even farther out of the stream of development that culminated in the Rio Grande [Pueblos]" (Vivian and Mathews 1964:115). Vivian's remarkable insight, as far as I can tell, has been all but ignored in the subsequent Chacoan literature. Why? I know the answer: We want Chaco to stay in Pueblo Space.

3. The initial products of which were just beginning to appear in 2000, and rated a mention in Mills 2002. “Chaco Synthesis” was the uninspired name of a long, complicated project supported by the National Park Service in collaboration with the University of Colorado, which spanned the millennial divide. The goal was to pull together the work of the NPS Chaco Project (1969–1986)—a huge field project which did great things and published wonderful technical reports, but failed to conclude, failed to launch. The Chaco Synthesis (under the direction of yrs trly) staged a half-dozen working meetings at various universities and museums, involving over one hundred archaeologists from 1999 to 2004. Those meetings produced a series of books and thematic journal issues, culminating with a volume of twelve thematic chapters by twenty authors, The Archaeology of Chaco Canyon (Lekson, editor 2006), and complementing Joan Mathien's 2005 excellent summary of Chaco archaeology and Chaco Project fieldwork Culture and Ecology of Chaco Canyon.

4. The Chaco Research Archive was created in 2002 by Stephen Plog and Carrie Heitman, with the assistance of a stellar board of advisors. The Archive is “an online archive and analytical database that integrates much of the widely dispersed archaeological data collected from Chaco Canyon from the late 1890s through the first half of the 20th century”—but it now includes later materials, too, such as the “gray literature” reports of the Chaco Project, including a few by yrs trly. It’s a phenomenal resource. www.chacoarchive.org
5. The Salmon Ruins Initiative was an NSF-supported 2001–2014 project sponsored by Archaeology Southwest to complete analysis and reports on Salmon Ruins, a major Chaco Great House near Bloomfield, New Mexico—70 km north of Chaco Canyon. Salmon Ruins had been excavated from 1970–1978 by Cynthia Irwin-Williams (with a cast of hundreds, including yrs trly in a minor role). A preliminary report had been prepared in 1980 but never published. The Salmon Ruins Initiative, led by Paul Reed, compiled the old report and added much new analysis in a three-volume 2006 publication, *Thirty-Five Years of Archaeological Research at Salmon Ruins, New Mexico*, and a 2008 volume of papers edited by Reed, *Chaco's Northern Prodigies: Salmon, Aztec and the Ascendency of the Middle San Juan Region after AD 1100*, and a thematic issue of *Kiva* guest-edited by Reed.

6. The University of New Mexico's Chaco Stratigraphy Project, which began in 2006 with the re-excavations of Neil Judd's 1920s trenches at Pueblo Bonito. The Project is directed by Chip Wills and Patty Crown, and is ongoing. As part of this project, Dr. Crown documented cacao in Chaco cylinder jars—a jaw-dropping side-project. The first major publication from the larger project is Crown's 2016 volume on the artifacts, *The Mounds of Pueblo Bonito, Chaco Canyon: Material Culture and Fauna*. Crown and Wills's Chaco work has expanded to re-excavations in the central core rooms of "Old Bonito"—the ninth-century core of the building.

7. Among others: University of Colorado excavations at the Bluff Great House (Catherine Cameron and yrs trly) and Chimney Rock (Brenda Todd and yrs trly); Crow Canyon Archaeological Center's investigations at several northern San Juan Great Houses (most recently, led by Susan Ryan); and many others. See https://stevelekson.com/


9. This might remind readers of Ben Nelson's (1995) comparative study of Chaco and La Quemada. Measure after measure, Chaco came out ahead of La Quemada; but, ultimately, Pueblo Space kept it in its place. “Not if they are complex, but how they are complex”—opening the door for “alternative leadership” and that sort of nonhierarchical stuff. Which may be fine for Pueblos, but almost certainly not for Chaco.

10. Drennan and his colleagues (2010) in their own words: Regarding the burials of Pueblo Bonito, “In any context other than the US Southwest, the presence of such burials would immediately be taken to indicate substantial social inequality” (p. 48). And again, “Burials 13 and 14 at Pueblo Bonito would superficially, at least, suggest strongly individualizing organization, persistent efforts to label it ‘corporate’ notwithstanding” (p. 71).

   And regarding labor in monuments and public works: “While the nature and purposes of monumental construction in Chaco Canyon were rather different from either Moundville or La Venta, the magnitude of the labor invested clearly puts this case in the top ranks of this set of nine. The tax rate required for such construction in Chaco Canyon was probably also several times higher” (p. 68).

11. I considered quantifying this, taking a longitudinal random sample of Southwestern papers and coding the Pueblo Space buzz words and catch phrases and red flags. A chore too depressing to contemplate. And to what purpose? The results are obvious. If you doubt this, you are welcome to review the Southwestern literature for the past hundred-plus years, and good luck to you.
12. For example, Schelberg 1983:18. In my reading of Schelberg, John made as much sense as anyone on the subject:

Unfortunately classificatory labels frequently evoke passioned [sic] response rather than reasoned debate…. Suggestions of incipient urbanism, stratification or a complex chiefdom are not simple attempts to over-extol the virtues of Chaco or to make the development comparable in grandeur to other areas of the world. It is an attempt, however, to indicate that certain processes had occurred…. Arguments concerning classificatory labels such as "complex chiefdom" (which I believe the Chacoans to have been) or "simple state" obscure the more important questions concerning the conditions that select for stratification. Stratified societies have evolved in many areas of the world and there is no reason to assume a single causality. (Schelberg 1983:17–18)

13. Turner and Turner’s 1999 cannibalism study had been preceded by Tim White’s 1992 Prehistoric Cannibalism at Mancos 5MTUMR-2346—a solid argument in a weighty tome from Princeton University Press with an approving back-cover blurb from Binford himself; which was initially ignored and then speedily forgotten. Its ugly, unwanted message was emphatically expanded by Turner and Turner, and their work was picked up by the press, most notably by Douglas Preston’s (1998) provocative article “Cannibals of the Canyon” in New Yorker magazine. With the cascade of newspaper stories that followed, the fan was fully and roundly hit.

14. The then-head of Hopi denounced Man Corn from his office on TV, pointing to books on his shelf that proved the Hopi were peaceful people (which, of course, they are). Several months later, I was scheduled to meet with Hopi’s Cultural Preservation office. As I walked in, I was met (uncharacteristically) with a preemptive question before I could even say hello: If you’re here about that cannibalism business, we know all about it and it wasn’t us. Like a fool, I did not follow up; I said that my present business concerned proposed excavations at a Great House (as I recall; I remember their opening shot far better than the meeting that followed, which was about something other than cannibalism).

15. I’m not a big fan of Turner’s take on Chaco, but for those that reject his thuggish view or Mesoamerican interlopers: Aztec accounts of their arrival in the Basin of Mexico, and their initial attempts to win friends and influence people, were pretty darn thuggish. Real nasty. And those accounts were official court histories of the Aztec emperors.

16. Patricia Lambert, in a 2002 review of “The Archaeology of War: A North American Perspective,” notes that “the American Southwest has been a primary focus of archaeological research on warfare in the last decade, and several major synthetic works have recently appeared. However, despite abundant archaeological and ethnohistoric evidence of its presence, the identification and interpretation of warfare in this region remains a contentious subject, at least in part due to perceptions based on modern Puebloan culture” (Lambert 2002:219, internal citations omitted). People outside our field looking in have no difficulty perceiving our peculiar institution, Pueblo Space: “Perceptions based on modern Puebloan culture.”

17. Apparently I coined the faux-Latin “Pax Chaco” to reflect the evident absence of widespread warfare during Chaco’s time, in a conference paper in 1992—which means I must have been looking at evidence other than bones (Turners’ and LeBlanc’s books came out in 1999). Certainly settlement patterns before, during, and after Chaco.
Before and after, they circled the wagons and aggregated and eventually retreated up into cliff overhangs; during Chaco, farmsteads dotted the landscape, spaced at distances that suggests an absence of fear. I explored this topic in my one and only *American Antiquity* article (Lekson 2002). My interpretation has been challenged (Cole 2007; Kohler, Cole, and Ciupe 2009), but—right or wrong—it gave aid and comfort to the metaphorical enemy, because Pax Chaco pushed Chaco back into peaceful Pueblo Space. That was not my intent. Given the pervasiveness of warfare before and after Chaco, its absence during Chaco (if real) suggests a strong central authority keeping the peace. In my reading, by the threat of seldom-used force; by more popular readings, by ritual.

18. Anderson’s (1983, 1999) “imagined communities” had mainly to do with the printing press, literacy, and ultimately control or influence of the media by the national government. Anderson’s highly influential argument is, of course, far more complex than the precis. Tropes of patriotism and racism—and, interestingly, heritage through media and museums—run through Anderson’s “imagined communities.”

19. And, far less theoretical, nationalist agendas: In the second edition Anderson details his book’s many translations, and his fears that “imagined communities” became Bible for nationalism while at the same time being diluted from a top-down invention of tradition or imposition to a kind of cheery Gemeinschaft.

20. Which of course she acknowledges; Mills (2004a:241): “The definition of cultural patrimony as prime examples of collectively owned, inalienable objects.” That NAGPRA definition is:

Object of Cultural Patrimony: An object having ongoing historical, traditional, or cultural importance central to the Native American group or culture itself, rather than property owned by an individual Native American, and which, therefore, cannot be alienated, appropriated, or conveyed by any individual regardless of whether or not the individual is a member of the Indian tribe or Native Hawaiian organization and such object shall have been considered inalienable by such Native American group at the time the object was separated from such group. (25 USC 3001(3)(D))

21. I do not doubt that these objects were used in ritual, but what sort of ritual? Rather than ethnographic Zuni, perhaps we can look at Chaco’s contemporary world: Mimbres and Hohokam. Mimbres pottery images, contemporary with Chaco, show objects much like suggested Chaco “altars” (Vivian, Dodge, and Hartman 1978) in use, not as altars but as emblems or totems—birds, fish, other animals—mounted on poles carried by bearers, clearly for public display (e.g., Brody, Scott, and LeBlanc 1983:Figure 72). Elements of such a display, found in a Mimbres cave, are curated by the Northwest Museum of Arts and Culture, Spokane, Washington. (They’ve never been published.) Mimbres pottery also shows knobbed staffs similar to those from Chaco. The Mimbres images show staffs carried by individuals, in one case an elder on a stool/throne (similar to those made later at Paquimé; DiPeso 1974:Figure 367-2), and to my mind suggest that staffs were regalia of authority (Rodeck 1956; Brody 2004:Figures 23, 27). Staffs depicted in contemporary Hohokam art seem to indicate a role as a burden bearer, perhaps a trader (Haury 1976:237–239). Or, as suggested in note 31, political authority. Staffs may mean one thing in modern Pueblos, and perhaps something else in the eleventh century. Our job is to determine what they meant in the eleventh century, and then figure out how that shifted to its current meaning.

Giving. “The Defeat of Hierarchy” is the title of a chapter in that book. (See also Weiner 1985.) The book is about challenging the anthropological faith in *reciprocity*: “It is the tenacious anthropological belief in the inherent nature of the norm of reciprocity that impedes the examination of the particular cultural conditions that empower the owners of inalienable possessions with hegemonic dominance over others. It is, then, not the hoary idea of a return gift that generates the thrust of exchange, but the radiating power of keeping inalienable objects out of exchange” (Weiner 1992:149–150). Hoarding? That could be the case for Bonito, and it’s been suggested for Paquimé. Inalienable objects are indeed circulated but not through trade or exchange; and the most prestigious are retained within the noble “house.”

23. “The significance of [Weiner’s] work is that it points us in new directions, both within and outside of the sphere of exchangeability, for looking at the way that social valuables are produced and used. In this way, Weiner’s work recently has become an important influence on the anthropology of materiality” (Mills 2004a:239).

24. “Inalienable wealth takes on important priorities in societies where ranking occurs. Persons and groups need to demonstrate continually who they are in relation to others, and their identities must be attached to those ancestral connections that figure significantly in their statuses, ranks, or titles” (Weiner 1985:210).

25. Weiner (1992:103) does indeed say that European colonization often turns a possession of immense political power into an art object for a foreign museum, or even a tourist trinket. The other ways an “inalienable possession” loses power are physical: Destruction and deterioration.


The central themes of this chapter: first to locate the sources of authentication for inalienable possessions…second, to evaluate the limitations and potentials for keeping inalienable out of exchange, given the necessity to exchange; and third, to see how these problems and their solutions politically impinge on or are affected by the relationships of siblings and spouses. These are the elements that reveal under what circumstances hierarchy is established or defeated. Although gender and kinship are central to the dilemmas of keeping-while-giving, inalienable possessions are the hub around which social identities are displayed, fabricated, exaggerated, modified, or diminished. What is most essential about the trajectories of inalienable possessions, however, is not their individual ownerships but their authentication. The Polynesian cases reveal that shifting the ownership of an inalienable possession from one person to another does not reduce the possession’s power as long as beliefs in its sacred authentication continued. (Weiner 1992:99–100)

27. Attributed to Jean-Baptiste Alphonse Karr, nineteenth-century novelist and epigrammist. These are tough times for American graduate students: Taught Bourdieu and Foucault and French poststructuralists in all their annoying ambiguity, and now dragged back to Lévi-Strauss’s *ancien régime* of binary oppositions and rigid structures.

29. American Anthropological Archaeology jettisoned Claude's original notion of the noble house and honed in on Lévi-Strauss's more ethereal, conceptual baggage; in particular, a house as "moral being" or "moral person" (Personne Morale)—a Straussian notion as unlikely as a "corporate personhood" (Citizens United vs. Federal Election Commission, No. 08-205, 558 U.S. 310, 2010).

30. Susan Gillespie (2000) thinks Lévi-Strauss's house needs repairs:

Despite his intention to clarify studies of kinship practices, Lévi-Strauss instead garnered a great deal of criticism on virtually every point. His definition of the house has been ignored or rewritten. His characterization of the house as fetishization of marriage alliance has been considered inappropriate or simplistic, although the fetishistic or representational aspect of the house has been carried even further. He has been reprimanded for having ignored what should have been a major object of inquiry—the physical house itself—and rebuked for treating the house as a classificatory type with an outmoded evolutionary trajectory. Finally, has been criticized for failing to rise above the naïve conceptions of kinship that he himself argued against. [However] there is still much to be gained from the original version of the house of Lévi-Strauss. (Gillespie 2000:23–24)

Not much left standing. By 2000, kinship studies were very much on the wane in cultural anthropology; and all but invisible in archaeology (but see Ware 2014). Why does Lévi-Strauss's "house" appeal so strongly to archaeologists? The answer, of course, is that archaeologists leveled Lévi-Strauss's house and rebuilt their own, to a new design—while keeping the Lévi-Strauss brand. We de-constructed his house—blew it to pieces, really—and salvaged the ornamental bits we find attractive, to decorate a whole new edifice. Cultural anthropologists, too:

the real value of Lévi-Strauss's idea lies not so much in the creation of a new, unwieldy social type to complement or nuance already threadbare categories of traditional kinship theory but rather in providing a jumping-off point allowing a move beyond them towards a more holistic anthropology of architecture which might take its theoretical place alongside the anthropology of the body. (Carsten and Hugh-Jones 1995:2)

But l'ancien apparently was having none of this Anglophone meddling. In his last word on the subject (at least in English; Lévi-Strauss 1992) he stuck by his guns, repeating the arguments and analogies of "Nobles Sauvages." House Societies were ranked, hierarchical precursors to full-blown states.

31. The Lincoln canes were preceded by Spanish canes of authority. I suspect that whoever advised Old Abe that canes were just the thing (rather than Pendleton blankets, or Peace Medals, or other trinkets) knew of the historical importance of canes of authority in Pueblos. Many Pueblo governors (or rather, their offices) have three or four such canes, some dating back to early colonial times. Canes of this type were, in early colonial times, a symbol of Spanish gentility, a sort of peaceful sword; beyond their meaning in Spanish society, might the Governors' canes have tapped into a much older indigenous tradition of staffs/canes of authority? Both caballeros and caciques knew what canes meant. Bonito's staffs might well prefigure Pueblo governor's canes. Canes are symbols of authority and power from Spanish times on, and perhaps before. Could 300 staffs represent 150+ “outliers” plus the Canyon's noble families, retired when the capital shifted to Aztec? For an excellent description of Pueblo canes
and their history and possible pre-history, see Canes of Power (Silver Bullet Productions 2012).

32. EM seems peculiar to the West: Southwest, California, Northwest Coast. It began with Morgan in the Northeast, but is today conspicuous by its absence from (for example) the Mississippi Valley. If it were possible to EM Cahokia, which seems unlikely, could we push beyond that back to Hopewell and Poverty Point? No, of course not. There’s something about the Southwest that makes things seem reasonable which are patently absurd elsewhere. And that is Pueblo Space.

33. Chaco Revisited boasts three EM enthusiasts: Cultural anthropologist Peter Whiteley in one chapter and archaeologists Kelley Hays-Gilpin and John Ware in another.

Whiteley’s theme is “Chacoan kinship.” His reference, of course, is Pueblos: “The modern Pueblos are in important respects homologous continuations from Chaco, not analogous metaphors lacking in common derivation. Correlating historic and contemporary Pueblo models with prehistoric Pueblo societies should thus be conceived for what it is: not ethnographic analogy, but ethnological homology.” (Whiteley 2015:295, emphasis original) “There is every reason to assume—since they were not states—that Ancestral Pueblo social systems, including the most highly developed versions at Chaco Canyon, were articulated through principles and rules of kinship” (Whiteley 2015:298, emphasis added). But...what if they were states?

Whiteley’s chapter carries on at length, talking Pueblo to Chaco. I’m sure it’s a masterful analysis, but with several fundamentally false assumptions, I cannot read it with enthusiasm. Chaco was not a Pueblo (it was a state; chapter 3), Chaco’s kivas were not kivas (they were houses; Lekson 1988), and so on and so on. Projecting Pueblo analogies, much less homologies, back onto Chaco is a deeply doubtful enterprise.

But the times favor it, and so do Kelley Hayes-Gilpin and John Ware, whose chapter is titled “The View from Downstream”—a play on “up-streaming”: Moving interpretively from the present to the past. Hayes-Gilpin represents Hopi and Ware the Rio Grande. They are more temperate than Whiteley, acknowledging that “whatever Chaco was, it was different than the historic pueblos. Nevertheless, Chacoans were among the ancestors of contemporary Pueblo people, and we can productively use the historic pueblos as ‘end points’ in our reconstruction of past trajectories of culture change” (Hayes-Gilpin and Ware 2015:322). The rest of the chapter is a festival of homologies. But they end on what I see as a hopeful note: At Chaco’s end, “some [Chacoans] reorganized into smaller communities—some of which apparently kept some Chacoan legacies and some of which rejected all things Chaco” (Hayes-Gilpin and Ware 2015:344, emphasis added).

34. Ware denies this is teleological: “We do not look at ethnographic variation and deduce that the course of Pueblo social history had to be so-and-so,” but how can a ballistic reconstruction be anything but teleological? I absolutely agree with Ware (2014:11): “Historical reconstruction is the essential preliminary to all socio-cultural explanation. We need to know what happened and when, and often in considerable detail, before we can know why things happened the way they did.” He thinks EM will get him that history, but his method steamrolls the evidence—Chaco, most particularly—as discussed below. Not a steamroller, perhaps; rather, one of those car-crushing machines that compresses a Buick into a very, very heavy, breadbox-sized cube—sort of like Pueblo Space.

35. That history is not hidden; I wrote one version in History of the Ancient Southwest (Lekson 2009a). And see also Scott Rushforth and Steadman Upham’s (1992) A Hopi Social History. Rushforth is a sociologist, Upham an archaeologist; their book at-
tempted to bridge prehistory/history. Ware does not like this book, because Rushforth and Upham "argued that the 'horizontal' structures of the Hopis were not fully in place until the 1800s and were the direct result of historic depopulation and organizational simplification" (Ware 2014:58)—formation, that late, of Pueblo social structures is anathema in Ware's world, with Ware's method.

36. Ware believes that EM will move Southwestern archaeology forward. I'm doubtful: Ware "decided to 'revivify old research agendas' [citing Jerrold Levy] and take up questions and issues that fascinated an earlier generation of Pueblo ethnographers" (Ware 2014:xxii). It's a return to the 1920s, maybe even the 1880s; but like Marty McFly, Ware will send us back to the future.

37. Severin Fowles riffs on this theme:

There is a sense, then, in which Taos and the other [Eastern] pueblos conformed to a traditional model of a stratified society. One can legitimately speak of elites and non-elites. One can speak of hierarchy. One can speak of power. Status was not solely based on achievement; rather, ascription played a heavy hand. This much has been obvious to nearly all serious students of Southwestern ethnography, giving rise to the common assertion that the pueblos were theocracies.... The term "theocracy" invites comparison with much larger-scale phenomena: African polities governed by principles of divine kingship, royal monarchies of medieval Europe, pharaonic Egypt, and the like. Such comparisons are slippery exercises and, when taken too far, draw attention away from a key difference: however "elite" the priests were within the overall moral hierarchy of Pueblos, they nevertheless had no special—let alone kingly—privilege in any significant economic sense, or at least in any sense that we would immediately recognize as economic. Theirs was an elitism without grander residences, larger storehouses, or more wives. With few exceptions, the priests did not partake of privileged cuisine, nor did they wear fancy baubles signifying their status. Priests were even without exemption from hard labor. (Fowles 2013:60)

38. Ware (2014:22–23) dictates the question:

This book stands or falls on two arguments. The first is that the Pueblo ethnographies are more than a source of speculative analogies. The ethnographic Pueblos are end points on trajectories that preserve important information about the contingent histories of Pueblo social practices and institutions. Archaeologists and other historical scholars need to put aside their biases and become, again, serious students of historical ethnographies.

...My second argument is that if we are to understand social practices in Pueblo prehistory, we need to combine our study of the historical Pueblo ethnographies with a better grasp of kinship theory in general. Unfortunately, Southwestern archaeologists have gone the other way: they have convinced themselves that answers to kinship questions cannot be derived from material data of deep prehistory.

I'd say those Southwestern archaeologists are quite right: We cannot reconstruct kinship in deep prehistory. Moreover, I'd say that Ware's book stands or falls on quite a very different argument: His method. If his method is not sound, his "two arguments" are irrelevant.

39. Ware: "Skeptics will point out that we can never know precisely what kinship patterns existed long before written history.... Even with living groups, there is often
disagreement among both scholars and Native people about those patterns and how they came to be," [but] “it is important to infer what we can—especially in a book on the social history of tribal societies” (Ware 2014:23–24). This seems a tad circular: It’s damn near impossible to do it, but it’s OK to do it here, because that’s what the book is supposed to do.

40. There’s a reason cultural anthropologists abandoned kinship two decades ago; Ware notes this, with dismayed denial. And there’s a reason Southwestern archaeology gave up on kinship after a few attempts at “Archaeology as Anthropology” (e.g., Hill 1970; Longacre 1970; and not many thereafter): It’s nearly impossible to see kinship in the dirt. You can only project kinship through the calculus of EM, pushing what we think we know about an ethnographic present back into the past—where it does not belong.

41. Ware argues that the variation among and between Pueblos documented in ethnographies proves that Pueblo institutions survived colonization; that is, they were not entirely homogenized through assimilation. He concludes with a rallying cry for EM: “Ethnographic variation is not epiphenomenal! It is real, it is deep, and it matters!” (Ware 2014:183). Nice slogans, but the failure of colonial policies to totally assimilate Pueblos to Spanish/Mexican/American models does not mean that Spanish/Mexican/American policies and actions did not impact, deeply and profoundly, Pueblo histories.

42. Ware seems to think the demographic collapse didn’t happen, and has waved at the 2015 book Beyond Germs as a warranting text (Cameron et al. 2015). It’s not. Ware’s demographic quarrel is with Henry Dobyns (1966, 1983); his methodological gripe is with Robert Dunnell (1991).

In the Southwest, drastic demographic collapse, east and west, began as early as 1300–1400, long before European contact (e.g., Hill et al. 2004). This raises questions about Dobyns’s and others’ timing of the collapse; but does not question collapse. Beyond Germs does not argue that population loss was not “precipitous” or “devastating” (Kelton, Swedlund, and Cameron’s terms), rather that additional colonial effects exacerbated decline, and then prevented population rebound. “If epidemics had been the only factor involved, Native populations would have recovered within a few generations” but they didn’t (Kelton, Swedlund, and Cameron 2015:8). Dunnell’s argument, for me at least, remains intact.

Other studies, like Ann F. Ramenofsky and Jeremy Kulischeck (2013), refine the chronology and causes of the population collapse and question Dobyns’s and other’s high numbers; but no one denies that the loss was huge and devastating. Devastating to Pueblo societies and to the Ethnological Method.


44. Ware (2014:152) waves away the Great Divide at 1300: “In my judgement, Pueblo III and at least the early part of Pueblo IV are not primarily about social and political reorganization; they are about drought, arroyo entrenchment, and a scrambling of the monsoonal rainfall systems, resulting in range contraction, social balkanization, and conflict.” I suggest that several items on that litany of disasters would probably impact the validity of EM back beyond 1300—and of course there’s abundant evidence (noted in the text) for change in cosmology and religion, and to me very clear evidence of political collapse (again, noted in the text).

45. “If Chaco was organized on an eastern Pueblo model, why does Chaco not look more like an eastern Pueblo, and vice versa? Why do eastern pueblos not have great houses and roads? Where are the elites among the Eastern Pueblos, the modern equivalents of the individuals buried in Old Bonito?” (Ware 2014:126–127, original
capitalizations). Very good questions, all of 'em, which Ware (2014:127–131) does not answer in five pages of speculations on kinship versus bureaucracy which follow—a discussion which is actually pretty interesting! Ware devotes a section to “Kinship and Bureaucracy: A Fundamental Tension”; he argues that sodalities detached from kinship and became protobureaucracies (Ware 2014:126–130). Now we’re getting somewhere! Norman Yoffee, the arbiter of statehood: “I suggest...that the most important necessary and sufficient condition that separates states from non-states is the emergence of certain socioeconomic and governmental roles that are emancipated from real or fictive kinship” (Yoffee 1993:69). Sodalities, emancipated from kinship?

In a stunning display of ahistorical up-streaming, Ware argues that since modern Pueblos don’t have kings, Chaco could never have had kings: “Despite the fact that no modern Pueblo community has a political hierarchy capped by a powerful individual, some archaeologists have argued that self-aggrandizing individuals may have played a prominent role during the Puebloan past in places like Chaco Canyon. I understand the temptation; monumental architecture and exotic trade goods whisper suggestively of the rise of powerful political agents. Of course, if big men, chiefs, or similar self-aggrandizers figured prominently in early Pueblo communities such as Chaco, archaeologists are challenged to explain why such powerful individuals had disappeared by the time Spanish explorers showed up” (Ware 2014:40). The answer is simple: Chaco had kings and the Pueblos got rid of ‘em; Pueblos are a reaction AGAINST Chacoan political systems, not a development FROM Chaco (see chapter 3). That’s how history works: Shit happens.

46. Save Aztec, Chaco’s successor, which should be considered as a package: The Chaco-Aztec polity (Lekson 2009a, 2015a).

47. As I write, Eric Blinman (a well-known Santa Fe archaeologist) has hopped on the bandwagon and delivered a public lecture on “A Revisionist View of Chaco Canyon: Is It Simpler than We Think?” I wasn’t in the City Different to hear that lecture, alas.

48. That “someone” would be Cynthia Irwin-Williams, a remarkably astute, intelligent archaeologist. She came to Chaco from hunter-gatherer archaeology, and thus avoided much of the bias and baggage of Pueblo Space. We need more like her.

49. From Ware’s A Pueblo Social History:

Consider Upham’s (1982) suggestion that late prehistoric communities in the Little Colorado valley of northeastern Arizona were politically linked by intervillage mating networks among elites. It is true that alliances based on inter-marriage are found in every region of every inhabited continent, but all ethnographic Pueblos are endogamous. As Jerry Levy (1994a:239) points out in his critique of Upham’s alliance model, “if intervillage alliances were cemented by marriage among elite families, the decline of such a system would have resulted in agamous communities rather than the strict endogamy that we find even among bilateral Tanoans.” Levy’s critique reminds us that comparative research, no matter how systematic, complements but can never replace historical [i.e., ethnographic] analysis. (Ware 2014:10)

This argument is questionable on several counts, not the least that Levy’s insistence that marriage between elites contrary to Pueblo “rules” in the fifteenth century would result in a lack of marriage rules among Pueblo nonelites in the nineteenth century. That’s classic Ethnographic Method. Likewise Ware’s dismissal of an interpretation of a fifteenth-century situation with the trump card of ethnology: In the idealized ethnographic present, “pueblos are endogamous,” therefore no other situation was possible in the past.
50. Well, there are still a few people who want it both ways: Their outlier was vaguely connected to Chaco (“trade,” “ritual,” “social interactions”), but really it was autonomous and local. I summarize, harshly but fairly I think, a few arguments that are out there right now on the web and in conference papers: A particular outlier was built with local stone, therefore it’s local. A particular outlier does not look exactly like Pueblo Bonito, therefore it’s local. A particular outlier had local ceramics, therefore it’s local. I thought we had hashed all this out decades ago—in fact, we did hash this out, but some people apparently didn’t get the memo. A particular outlier (and each and every outlier) is different, therefore it’s local. This latter argument gets drawn like a gun to prove Chaco’s region was no region.

Of course buildings are built of local stone; no one is going to haul Cliff House sandstone 150 km to build a “real” Chaco building. Of course they don’t look like Pueblo Bonito; there are more than a few parts of Pueblo Bonito that don’t look like Pueblo Bonito (the range of variation in a single building at Chaco approximates the range of variation in outliers—controlling for local materials). Of course they used local ceramics; Chaco Canyon didn’t make much of its own pottery, they used “local” ceramics from everywhere else. And of course every outlier is different; every great house at Chaco Canyon is different. Still, outliers constitute an easily recognizable class or category. It’s a gift from the archaeology gods, which some of us still try to make go away.

Local is somehow seen as logically better, methodologically preferable, even when Chaco’s region is one of the best documented archaeological (big) facts in Southwestern archaeology. There’s 150 of these things and you want them all to be somehow autonomous and local? 150 cases of simultaneous equifinality? Gimme a break.

51. I cannot resist pointing out that the two decades of research on Black Mesa was almost entirely working in advance of massive coal-mining machines—great research, but contractually limited in scope. Black Mesa is enormous, and Mr. Peabody’s coal trains hauled away only a bit of it. Proportionately, a very small bit. The vast majority of Black Mesa remains unknown to (archaeological) science, and prime country for Great House-hunting. Cedar Mesa (north in Utah) provides a useful lesson: A well-designed, well-executed, systematic sampling of the vast top of Cedar Mesa (the whole thing, not limited to a mine area) missed its two Great Houses—both with Kayenta pottery (Matson and Lipe 2011). That’s the nature of rare or singular things: You have to look for them. We did, back in the 1980s and 1990s, and found them nearly everywhere.

52. Scores of very good archaeologists—perhaps hundreds—labored as individuals and as groups for several decades to discover and document the Chaco region. The regional structure is as close to a fact as we will ever get in Southwestern archaeology (at least, regarding anything interesting). It is not an interpretation, it is an empirical pattern, with a great big Chaco in the center and 150+ Great Houses in a repeated, robust contextual pattern scattered around it, over 60,000 sq mi. One may (and should) niggle about details. But at this point, we should be highly suspicious of arguments that wave it off or ignore it. Chaco’s regional structure is as much a fact as Chaco’s corn agriculture (but wait for chapter 3: There’s nothing sure save death and taxes—not even farming).

53. Fowles argues that Taos was beyond Chaco’s reach and so too the whole Rio Grande: “Perhaps because the Rio Grande populations wanted it that way.... We might reinterpret the absence of Chaconess as the presence of anti-Chaconess.... In the absence of Great Houses we might find evidence of a refuge from Chacoan orthodoxy” (Fowles 2013:86). But Taos history was not in “developmental isolation and provincialism”
They were entangled with Chaco at its northeasternmost outlier, Chimney Rock, if not elsewhere. He suggests that migrants into the Taos area—who eventually became part of Taos—were “refugees, dissidents who fled an expanding regional system…or more provocatively: to what extent might we view the Winter People [a moiety at Taos] as a counterculture that opted out of Chacoan orthodoxy” (Fowles 2013:92). He extends this to the whole Rio Grande Valley: “It is unlikely that all eleventh-century communities in the northern Southwest would-have-if-they-could-have become Chacoan. Many may have purposefully avoided such a fate, viewing social simplicity as more progressive, more enlightened, more evolved than complexity” (Fowles 2013:93). In a word, Taos and the Rio Grande Valley rejected Chaco. (Works for me!) There follows a rich history related by Fowles to arrive at modern Taos (of which the Winter People were but one important element). But we can leave Taos’s story here. Taos and the Rio Grande Valley rejected Chaco, broke with Chaco, charted their course away from Chaco and its customs.

“Let us assume that there was something to oppose and struggle against, a rotten seed in Chaco’s evolving gestalt that populations eventually left behind. The Pueblos themselves frequently imply as much”—Fowles alludes to Pueblo accounts of “past worlds (Chacoan and other) gone corrupt and abandoned in favor of newer worlds, truer center places” (Fowles 2013:83–84). He uses ethnography not so much as a catalogue for objects and conventions to project back into the past, but as a source for real history.

Scott Ortman, whose study analyzes migration from the Mesa Verde region to the Tewa Pueblo area around Santa Fe, takes a similar position. Tewa-speakers from the Mesa Verde area without question were impacted by Chaco (or Chaco/Aztec), but left all that behind when they moved to the Rio Grande. The merging of Tewa peoples into existing Rio Grande populations was marked by the renunciation or repudiation by the migrants of many characteristic Mesa Verde and Chaco features (see also Lipe 2006). Ortman sees these deliberate acts as “an overt, negative commentary on the recent past…[part of a] discourse of erasing, or undoing, the recent past” (Ortman 2012:348–349). And again “‘wiping out’ emblematic features of the recent past and returning to a previous [pre-Chaco] way of life—in this case an earlier period of Mesa Verde culture history” (Ortman 2012:350) transposed or preserved in the Rio Grande Valley. Which, as Fowles argued (above), largely avoided direct Chaco entanglements.

Intriguingly, Keres has been suggested as the probable “prestige language” or “prestige code” of Chaco (Shaul 2014:141–147): “The presence of an elite means an elite way of talking, a prestige code, which was used in rituals and by the elite in general, and by others communicating with the elite” (p. 142).

Pasiwvi, Hopi for “place of deliberations,” was focused on Wupatki and the San Francisco Peaks, but also encompassed a larger part of northeastern Arizona from the Grand Canyon to the Mogollon rim. Wupatki rose as Chaco fell, and represented something of a “rival” to Aztec (Lekson 2009a:156–158). Aztec was huge, a city; Wupatki was much smaller but remarkably eclectic: A Great House, a Great Kiva, and a Hohokam ball court! While Aztec is famous for the near-absence of exotics, Wupatki had more macaws (and other high-end baubles) than Pueblo Bonito. Aztec severed its southern ties; Wupatki kept the lines open. As pasiwvi, Wupatki may have been the place where Hopi leaders reinvented their societies as something new, something other than / different from Chaco-Aztec. The quote continues: “This new life plan would be distinct from the complex religious ways that had until then held sway. This Hopi life encompassed the principles of Hopi life today: Cooperation, sharing, respect, compassion, earth stewardship and, most of all, humility.” (Kuwanwisiwma
et al. 2012:9). Does this suggest that the “ways that had until then held sway” may NOT have included cooperation, sharing, humility? Only the Hopi know. In any event: Another potentially major historical discontinuity, pinned perhaps to twelfth-century Wupatki.

56. “If Chaco fit neatly into some straight-forward organizational ‘box’…we would have found that box by now. This does not mean it was some unique specimen we haven’t seen before or since in the world; that is theoretically possible but statistically unlikely. What is more likely…is that we haven’t looked at enough boxes yet” (Sebastian 2006: 411, quoting Sebastian 2004).

57. Call Chaco a “kingdom,” and you get a big smackdown or the silent treatment. Call the Comanches an “empire,” and you win the Bancroft Prize or (almost) the Pulitzer. I refer to Pekka Hämäläinen’s (2008) The Comanche Empire and S.C. Gwynne’s (2011) Empire of the Summer Moon. Gwynne’s “empire” was figurative and literary, but Hämäläinen’s “empire” was literal and strategic: Hämäläinen wanted to jolt historians and anthropologists out of their conventional view that Comanches were a “band-level” society. Hämäläinen argued that the facts strongly suggested otherwise (he’s right, read the book). In my words, not his: American Anthropology’s biases did injustice to Comanche history. What’s true on the Southern Plains is truer in the San Juan Basin. The facts strongly suggest that Chaco was a kingdom (or altepetl)—terms I, like Hämäläinen, used to jolt my colleagues out of Pueblo Space. That word-strategy worked for Comanche, but not for Chaco.

One big difference in the contrary receptions of these historical revisions is the flattening effects of the Pueblo Space versus the Plains’ Chiefs-and-Battles mythology. I will not go into the popular imagination of Plains warrior societies—Indian Wars via Hollywood. Suffice it to say that our image of the Plains accepts a horse empire. Pueblo Space does not admit a kingdom.

58. While writing these paragraphs, which took a few days, the Santa Fe newspaper published an oversize magazine-style article, “The Center Holds: Enigmas Endure at Chaco Canyon” (Weideman 2017); and Science News magazine displayed on its cover “Mysteries of Chaco Canyon” (Bower 2017; although that was not the title of the article). I don’t keep count, but I’d guess that articles and posts touting Chaco’s mysteries appear every two or three months, and have for as long as I’ve been paying attention, which is a long time. No kidding.

59. “No one really knows why this ancient site was built here or what the main purpose or function of the site was. The mystery and beauty of Chaco Canyon has enthralled visitors and researchers for many years. May it continue to do so.” An excerpt from a brief report of a field trip to Chaco by the School for Advanced Research in Santa Fe, and sent out to their constituency on “SAR November E-News” of November 1, 2016. This specimen fell into my lap (or rather, popped up on my screen) as I wrote this section; kismet dictates that I share it here. It is a perfectly fair example of all the Chaco-as-mystery out there in Pueblo Space.

60. Stephen Plog introduced the recent volume Chaco Revisited with an essay, “Understanding Chaco” (Plog 2015:3–29). Regarding understanding Chaco, Plog says: We don’t. Our lack of understanding is not for want of data (“copious amounts of data”). Then why? Plog suggests several “important reasons why key questions about Chaco remain unanswered.” First, “the remarkable spatial scale of the Chaco social network”; second, “the complexity of the archaeological record in the canyon”; and third, “there has been too little effort to intensively and rigorously evaluate those [competing] models” of Chaco. To reinforce this last point, he quotes and paraphrases Gwinn Vivian: “There are too ‘many “as it was” scenarios developed in attempting
to “explain” the archaeological record’ of Chaco and not enough studies that ‘use the record as a source of data to confirm or refute testable hypotheses, models, or propositions” (Plog 2015:12).

Let’s consider these dilemmas, which seem to derail our understanding of Chaco. Is the spatial scale of Chaco remarkable? Not really. Chaco’s about the same size as Hohokam in the Southwest and pales in comparison to Hopewell and Mississippian in the East. Yet there is not nearly the mystification and hoopla about those three archaeological entities—despite the fact that Hopewell really IS mysterious and weird. Chaco’s spatial scale is remarkable only when one’s frame of reference is Pueblo Space. While historic Pueblos used impressive expanses of land for wild resources, the Pueblos of Pueblo Space were tidy, self-contained farming villages. If you are expecting a Pueblo, then Chaco’s regional scale may come as a surprise. But…why would you expect a Pueblo?

Is the archaeology inside the canyon unusually or even prohibitively complex? Again, no. Chaco archaeology, in fact, is emphatically easy: Unsubtle sites, superb preservation, thousands of tree-ring dates, a century of (comparatively) generous funding, and a broad spectrum of excellent environmental studies—what’s to gripe about? Chaco certainly is less challenging than Hohokam, whose far less substantial remains of far more complicated settlements lie under Arizona’s capital city. Nobody built a city on top of Chaco. Hohokam archaeologists look at strip malls and wonder what’s underneath. Chaco archaeologists stroll through the standing walls of Pueblo Bonito. Chaco’s archaeology is more complicated than a hamlet on Black Mesa or a village on Mesa Verde, because Chaco was not those: Chaco was a small regional capital. Of course—of course!!!—its archaeology is more complicated than smaller sites in the northern Southwest—that complication is an index, “data” if you will, telling us Chaco was not farmsteads and Pueblos. But Chaco’s archaeology is not alarmingly complex compared to other small regional capitals (think Cahokia) or centers (think Phoenix). Unless one expects the simplicity of Pueblo Space: If so, then Chaco’s archaeology might seem a bit intimidating.

Too much speculation and not enough hypothesis testing? Here I agree and disagree. Certainly some of our speculations can hardly be evaluated much less tested. How to test for ceremonial center or rituality? Beats me. But this misses the point. The most useful models of Chaco are not trying to explain the archaeological record, they are trying to describe it. What profits us to “test hypotheses explaining” Chaco if we don’t first understand what Chaco actually was? What good to evaluate an explanation without first correctly defining the explicandum? When I say Chaco was a low-wattage state (or, to anticipate a bit, something like an altepetl), I have explained nothing. But I’m not trying to explain anything; I am describing what is to be explained. Plog, and Vivian (2001) before him, seem to miss that point; in large part, I think, because of residual scientism. They appear to want write history through science, narratives through hypothesis testing. That won’t work (see chapter 6). It should be possible, of course, to evaluate (NOT test) competing interpretations of Chaco-as-explicandum—but probably not within the framework of science. I’ll return to this problem, below and at length in chapter 6.

The single most important reason why key questions about Chaco remain unanswered is simply this: Pueblo Space. That bias makes us expect small scales and simple archaeologies of Pueblos when, at Chaco, we are in fact confronting a small regional capital. Raise your sights! And learn about the archaeology of small regional capitals; most are far more complicated than Chaco, but archaeologists around the
globe, working with far less copious and less precise data than Chaco’s, somehow make sense of them.

61. Ortman (2016a) review of Chaco Revisited. “Enigma” was simply a nicely turned phrase, of course; but it’s also the latest of many archaeological allusions to Chaco’s mystery.

62. A sampling from the ’teens: Historian Daniel Richter, trying to make sense of Chaco, complained that “the surviving physical evidence leads archaeologists to wildly different conclusions,” most particularly “about the degree to which Chaco Canyon was politically stratified” (Richter 2011:17). (Happily, Richter chooses wisely, and gets Chaco right.)

Archaeologist Severin Fowles (2010:195) concurs: “There is little agreement on the nature of Chacoan leadership. Chaco polarizes contemporary scholarship.” And a few years later, despairing of Chaco scholarship: “a thing of cults and religious fanaticism”—referring to the current state of its archaeology, not Chaco’s spiritual life in the past” (Fowles 2013:75).

In 2010, Stephen Plog’s “Reflections on the State of Chacoan Research” concludes: “Despite the profusion of publications we have not made significant progress in understanding key aspects of Chaco” (p. 378). And again: “To paraphrase [David] Phillips (2002:12), we lack a conceptual framework for Chaco that attempts to explain all of the variation we observe, that carefully examines the dynamic nature of social groups and accounts for relationships among those groups” (p. 390). Plog and Phillips might not have a conceptual framework that “explains” Chaco to their liking but I do, we do. Plog and Phillips just don’t like where that conceptual framework takes us: Away from Pueblo Space into dangerous territory.

Chapter 3: A History of the Ancient Southwest

1. Great Houses are real, and really different. The stark differences between Pueblo II Great Houses and normal Pueblo II architecture (e.g., the Bc sites) were noted early and often (e.g., Hawley 1934; Kluckhohn 1939). The owners/residents built and maintained the Bc sites; They were domestic, vernacular buildings. Great Houses required labor forces far larger than the number of residents (below) and can quite properly be called monumental architecture. Even Architecture with a capital A.

The differences between Great Houses and “small sites” has been a central question in Chaco archaeology, from Florence Hawley’s realization that Great Houses and Bc sites were in fact contemporary (Hawley 1934); to Gordon Vivian’s tricultural systematics (Vivian and Mathews 1965); and the Kluckhohn-Gwinn-Vivian biethnic Chaco (“as an egalitarian enterprise”; Vivian 1989, 1990). “Towns” versus “villages”: Those differences have troubled archaeologists for almost a century. The solution seems obvious—nobles and commoners—but that’s forbidden in Pueblo Space.

2. Great Houses were houses, but not many people lived in them. Great Houses were houses, not temples or empty monuments or machines for ritual. Three independent analyses have reached the same conclusion: People lived in Great Houses, but far fewer people than would be suggested by their hundreds of storage rooms (see note 18; see Lekson 1984; Windes 1984; Bernardini 1999).

3. People living in Great Houses were...special. Pueblo Bonito was the only Chaco Canyon Great House where numerous burials were found. Nancy Akins reported that the Bonito burials were bigger and healthier than their contemporaries (Akins 2001, 2003; contra Palkovich 1984); that they were a closely related group (confirmed by Kennett et al. 2017); and that the burials suggest “Chaco Canyon’s residents were hierarchically
organized” and “status in the upper levels was ascribed” (Akins 2003:105). Recent analysis of the two main male burials in Bonito’s central tomb note that they were “among the most remarkable in the prehispanic Southwest” presenting clear evidence of “hierarchy and social inequality” (Plog and Heitman 2010). In an earlier report, Akins (1986) noted that women of Bonito generally lacked the “squatting facets” that mark the bones of young women presumably tasked with corn grinding, which requires extended daily periods of squatting. Recent genetic research concludes that the Bonito burials in Room 33 (a high-status crypt with 300+ years of burials; Plog and Heitman 2010) were a “matrilineal dynasty” (Kennett et al. 2017).

4. Chaco was a class-stratified society. The differences between Great Houses and Bc sites once was interpreted as two different ethnicities simultaneously sharing Chaco Canyon (e.g., Vivian 1990). Given other lines of evidence—the monumentality of Great Houses (above), bioarchaeology (above), disparities in “high-end” material culture (below), Chaco’s regional system (below)—it seems far more likely that, as a pre-PhD punk once suggested, “stratification in housing presumably reflected social distinctions in the population” (Lekson 1984:271). Great Houses were palaces—monumental residences for elite, presumably ruling classes, which incorporated a great deal of nondomestic storage space, specialized ceremonial structures, and probably “offices” for an evolving protobureaucracy (Lekson 2006). Claims that Chaco’s Great Houses were purely (or even mostly) ritual or simply farming villages require us to ignore a great deal of robust, fairly unambiguous evidence. Wishful thinking, sending Great House back into Pueblo Space.

5. Outlier Great Houses define Chaco’s region. The geographic extent of Great Houses was established by several independent, collaborating projects (Fowler et al. 1987; Kincaid 1983; Marshall et al. 1979; Nials et al. 1987; Powers et al. 1983). Several “outlier” inventories listed the same sites, consistently. Among those who visited “outlier” Great Houses in the four quarters of the Chacoan world, there was little doubt that small “outlier” Great House sites in the north represented the same architectural expressions as small Great Houses in the south. Beyond the massive reality of the Great House itself there was a bundle or package of more subtle landscape features that were consistently but not ubiquitous at “outliers.” Southwestern archaeology in the 1980s had not yet come to grips with landscapes; our observational language was insufficient to the task of describing outlier Great Houses while recognizing their variability. The observational language developed or perhaps devolved to “Big Bump” surrounded by a “community” of smaller sites, berms, depressions, etc. (Lekson 1991). For a less enthusiastic view, see Kantner and Kintigh (2006).

6. Road networks: Chaco’s region was a system. Road segments have been unequivocally defined in every direction from Chaco except northeast—an area largely devoid of Great Houses. But only a small fraction of the projected road network has received adequate attention. A half-dozen major roads radiate out from Chaco (Kincaid 1983; Nials et al. 1987); local networks of roads appear to link outlier Great Houses and significant natural features (e.g., Hurst and Till 2009).

Roads unquestionably carried foot traffic (they have elaborate stairs and ramps at topographic breaks) but they also carried symbolism as earthen monuments connecting places in Chaco’s region. One valence of symbolism was historical: Later point A was connected to earlier point B, memorialized in a road-monument between non-contemporary places (Fowler et al. 1987; Stein and Fowler 1992; Lekson 1999)—or between a Great House and an important natural feature. Roads surely had meanings beyond the pedestrian. In the rush to ritual, the North Road connecting Chaco to
Salmon and Aztec Ruins was declared a 100 percent symbolic road to nowhere (Marshall 1997), but that interpretation falls before the evidence (Lekson 2015a; Carlson 1966). The roads we know are the ones that are (comparatively) easy to see. We probably know most (nearly all?) outlier Great Houses; we know only a small fraction of roads.

The design, construction and long-term maintenance of roads required as-yet-uncalculated but surely substantial amounts of organized labor. While it is possible that road features have a deeper history in the Southwest, the association of roads with Chacoan Great Houses is clear throughout Chaco’s region. Roads are not a series of repeated independent inventions: They reflect Chacoan engineering, transportation, and ideological canons; and in fact, major roads lead ultimately into and out of Chaco Canyon. And their extent appears to correspond to the region defined by outlier Great Houses.

7. Line-of-sight communication: Chaco’s region was a system. The existence of line-of-sight communications from Chaco to outliers and among outliers is certain, but its extent is even less documented than roads (Hayes and Windes 1975; Heilen and Leckman 2014; Van Dyke et al. 2016). This system demonstrably spanned 130 mi (200 km) from Chimney Rock to the southern Chuska Valley, and it seems highly likely that it continued out to the edges of Chaco’s region. It in part paralleled and in part operated separately from the road network.

Recent studies highlight the importance of “repeater stations”—isolated “shrines” away from residential sites that were positioned to relay information from one Great House to another. The classic case for “repeater stations” was Katherine Freeman’s demonstration that Huerfano Butte served as “repeater” between Chimney Rock Great House in southwestern Colorado and Pueblo Alto at Chaco Canyon—a total distance of about 80 mi (130 km) (Freeman et al. 1996). There are no Great Houses or, indeed, residential sites on or near Huerfano Butte. Someone would have to be stationed there to operate the line-of-site system, relaying information to and from Chaco and Chimney Rock. Perhaps constantly, perhaps on some prearranged schedule; but the persons assigned that task were in fact personnel in a true technological “system” of communication.

8. Chaco’s population. My unimpeachable source: Lekson 1984:272. I like my early estimate because it was based not on hearths (which may be missed on now-vanished upper floors) but on features that are almost impossible to miss: “kivas.” My estimate combined Alden Hayes’s numbers for small-site populations, based on his survey of the canyon (Hayes 1981), which seemed to me solid and technically standard for Southwestern archaeology at that time. The problem was Great Houses. Pueblo Bonito had 500 rooms (or whatever the number was; I forget). What did those rooms mean for population? For Great Houses—the thorny question, then and now—I developed a method based on the number of “kivas,” with one kiva equaling one household. And then multiplied the number of “kivas” by a notional family size. This assumed, of course, that “kivas” were domestic, not ritual (Lekson 1988).

The round rooms reverently presented to the public as “kivas” at Chaco and Mesa Verde were not the ceremonial structures we see today in Pueblos, they were gussied-up pit houses. That was true of so-called “kivas” at Unit Pueblos (five rooms and a “kiva”) before, during, and after Chaco, right through Pueblo III. (Note that I’m not including Great Kivas in this discussion; they may have been more like the Pueblo kivas; more on Great Kivas, below.) When first proposed (Lekson 1984:50–51, 1988), this interpretation met stiff resistance (e.g., Plog 1989:147–148); today it’s SOP in Mesa
Verde archaeology. “Today, archaeologists typically count households by counting pit structures [pit houses and kivas]...in pre-Pueblo IV sites” (Kohler and Higgins 2016:692). But not in the public imagination, or in NPS interpretation, or in archaeology outside the Four Corners (including Chaco, alas) where every Pueblo II or III pit structure remains a kiva (e.g., Crown and Wills 2003). You just can’t make Chaco “kivas” go away, can’t chase them out of Pueblo Space: Ritual über alles.

Recognizing that “kivas” were actually pit houses, and equating both with a household is fairly straightforward. But...how big was a household? Based on archaeological conventions of the time, I fixed on 6.4 people—somewhere at the lower end of the continuum between a “nuclear” family (not atomic: parents, a few kids, and a stray aunt or uncle) and an “extended” family (not racked and stretched: founder parents, a couple of nuclear families, and a few other odds and sods). 6.4 was a number I took from archaeological studies of the time—conventions based on nothing much beyond a firm faith that simple people had simple (“nuclear”) families. Extended families are, of course, larger, and (as it turns out) far more common: From many societies across the globe, extended families range from 10 to 20 people—about two or three times larger than my 6.4 convention. So my 1984 estimate of 2,100 to 2,700 people at Chaco may be a minimum; Chaco’s population was probably more than that, with even more nobles. If I live long enough and can recover from Chaco-fatigue, I may revisit this problem—and the number will surely go up.

9. Chaco’s region...60,000 to as many as 100,000 people. The population of the San Juan Basin has been estimated, at Chaco’s height, to be around 35,000 (Dean et al. 1994)—an estimate supported by more recent work (Heilen and Leckman 2014:380).

There was a lot more to Chaco’s region outside the San Juan Basin. Consider the northern San Juan, which overlaps only slightly with the San Juan Basin. For the Village Ecodynamics Project (VEP) area—a limited portion of the “Central Mesa Verde” subdistrict—a recent estimate of momentary population at 1100 was about 1,000 households; at 5–9 people, that’s another 5,000 to 9,000 people (Varien and Potter 2008). It seems safe (to me) to double VEP for the rest of the Central Mesa Verde area, in size much larger than VEP: Mesa Verde itself, southeastern Utah, etc. Say: 10,000 to 20,000 people total (maybe more) in the northern San Juan. That takes us to 60,000 plus. As far as I know, no one has ever attempted population estimates for Chaco’s considerable region in northeast Arizona or, to the south, the densely settled Zuni/Cibola region. Archaeology is thick on the ground in both. If only 10,000 each (a modest guess), that gets us to 80,000; but I think Zuni/Cibola was almost certainly more densely occupied. And Chaco’s region incorporates other, less densely settled areas. Another 20,000?

Sixty thousand seems solid and certain; 100,000 is an educated guess, but not unlikely.

10. Chaco had bulk and prestige economies. By “economy” I mean: The organization or structure of production and distribution of goods and services. For the first analysis in two decades of a major collection of Chacoan bulk goods, see chapters in Crown (2016).

Economy is one of several aspects of Chaco that has been swept under the prayer rug of ritual. Faith might move mountains, but a quarter-million pine beams came to Chaco from distant forests on peoples’ backs (Betancourt et al. 1986; Guiterman et al. 2014). Not to mention tons of crockery (Toll and McKenna 1997; but see Arakawa et al. 2016), rocks (Cameron 1997; Duff et al. 2012), meat (Grimstead et al. 2016), and quite possibly vegetables (Benson et al. 2003; Benson et al. 2008; Benson et al. 2009; Benson 2010; but see cautions in Benson et al. 2010). Tumpline economies op-
erate fairly efficiently over distances up to 150 km (Malville 2001; discussed in Lekson 2009a:132–133), a radius around Chaco which encompasses the sources of these bulk goods. And of course there is no requirement that Chaco’s economy was efficient; were that a consideration, they would have built Chaco in the Chuska Valley or on the Animas River. The economy answered to Chaco, not Chaco to the economy.

A brief consideration of prestige goods: Chaco had lots of bling (Matson 2016), and the honored dead of Pueblo Bonito had way more than most. Much was shell, much was shale, much was other materials, but the biggest baddest bling was turquoise. Turquoise came into Chaco from central Nevada, southern Nevada, southern Colorado, northern New Mexico, and southern New Mexico (Hull et al. 2016). At Chaco, commoners in Unit Pueblos crafted beads and tesseræ—workshops were found in “small sites” and Great Houses (my interpretation of data in Mathien 1997, Matson 2016; biased by my recollections of excavating a large storage pit at a small site, with hundreds of thousands of bits of turquoise bead–production debris). A lot of that turquoise dandified Chaco’s nobles.

We have known for over a century that many Mesoamerican (sensu Kirchhoff) high-status objects found their way to Chaco: Scarlet macaws, copper bells, and so forth (Nelson 2006). Recently we learn that Chaco had cacao from southern Mesoamerica and Black Drink from the Southeast Gulf Coast (Crown and Hurst 2009; Crown et al. 2015).

Chaco may not have been a total black hole, sucking in prestige goods and emitting nothing. I argue elsewhere that Chaco crafted regalia of office from macaw feathers and sent these, and probably turquoise, out to elites at outlier Great Houses (Lekson 2015a:30–34; see also Borson et al. 1998). This can only be a suggestion: While it is easy to see bulk and prestige goods imported into Chaco, it is much harder to demonstrate goods emerging from Chaco—a source for nothing but sandstone—propping up the local gentry and oiling the prestige economy.

11. Chaco was not a great place to grow corn. Currently, one group of archaeologists claims Chaco was an agricultural oasis (Vivian 1990; Vivian et al. 2006; Wills 2017; Wills and Dorshow 2012; Dorshow 2012; Tankersley et al. 2016). Another group, led by USGS geologist Larry Benson, insist Chaco was a lousy place to farm (Benson 2011a, 2011b; Benson et al. 2006; Benson 2016)—which was also the opinion of a number of earlier analyses by both archaeologists and soil scientists (for example, Loose 1979; other early, uniformly pessimistic studies are summarized by Mathien 2005:36–37).

What’s a practical prehistorian to do? These disagreements involve experts and expertise beyond the tool kit of most of us (certainly of your author), which appear to disagree on a rather fundamental issue: Chaco’s prehistoric productivity. This does not appear to be a compromise situation; these two camps are polemically opposed: One says yes, one says no. It’s not that we can average everything out and say that Chaco was 50 percent farmable.

Maybe we could look at a third party: Navajos, who actually tried to raise crops in Chaco Canyon. Navajos were canny farmers; and Chaco’s only edible resource, pinyon nuts, was chancy at best. How many Navajos lived at Chaco?

The Chaco Project survey of Chaco Canyon located 43 hogans (Navajo houses) which could be dated to 1750–1820, and 53 which could be dated to 1880–1945 (Brugge 1981:98). A few other hogans could not be assigned to these intervals. That averages out to about 6 to 8 hogans per decade—a crude statistic, but indicative of fairly low population levels during Navajo times. David Brugge (1980) notes that hogans were conspicuous by their absence in early Spanish accounts—accounts which detailed
hogan encountered elsewhere. When I asked David Brugge the maximum number of Navajos who lived in and near Chaco Canyon at any one time, he guessed no more than a few hundred people total—and many of those families had farms elsewhere (Brugge 1980; Brugge personal communication 1985). A few hundred Navajos were probably more than Chaco could feed. At its height in the eleventh century, Chaco had 2,500+ mouths to feed.

Given the well-attested bulk importation of wood, pottery, meat, etc., and the very low numbers of actual Navajo farmers which, historically, the canyon could support, I am very much inclined to accept the naysayers and to suggest that corn was a bulk import into Chaco Canyon—a conclusion supported by an initial "sourcing" study of a small sample of Chaco corn cobs, all of which were grown elsewhere (Benson 2010). Chaco might have been better for farming than its immediate surroundings, but that is faint praise indeed. Chaco paled in comparison to the Chuska Valley, the San Juan River valley, and the other agriculturally favorable areas around the outer edges of the San Juan Basin.

12. Chaco was cosmopolitan. Given the size of Chaco’s region and the size of its population, it seems likely that more than one language was involved, and more than one ethnicity— insofar as such distinctions were recognized in the eleventh century. (It has been suggested that Keres was Chaco’s lingua franca; Shaul 2014.)

Note 10 hinted at the exotic items that reached Chaco from distant lands, Mesoamerica and the Mississippi Valley/Gulf Coast. From a little closer to home, Chaco had a Hohokam shirt: "one of the more important archaeological textiles at the Maxwell Museum" (https://hands.unm.edu/63-50-123.html). The webpage goes on to remind us: "This surviving shirt fragment is a reminder that exchanges between the two cultural centers may have involved a few specific things (including rarities) that usually do not survive in archaeological sites.”

At risk of repeating some material from an earlier note, we’ve known for a century that Chaco had items from and interests in various parts of Mesoamerica (Nelson 2006): Copper objects from Michoacán maybe, macaws and cacao from Veracruz or Mexico’s West Coast (Aztatlan or even Oaxaca), Black Drink from the Mississippian region (perhaps from Cahokia itself), and other rare and precious objects from far-away places with strange-sounding names. And Chaco things moved out over equally long distances. Turquoise, for example: John Pohl (2001) recounts how Southwestern turquoise reached Tututepec in coastal Oaxaca by boat, and from thence to Mixtec artisans who created the turquoise-encrusted objects we marvel at in Mexican museums.

"Cosmopolitan,” in my dictionary: “Composed of people, constituents, or elements from many different places or levels.” Chaco was that. Yes: Chaco was a worldly, cosmopolitan place—the dictionary definition of “cosmopolitan,” not the one-world aspirations of Cosmopolitan Archaeologists.

13. Under threat of legal action, Papa substituted “Julian” for his on-again off-again pal, Scott Fitzgerald.

14. Gini coefficients—the current tool of choice for inequality in American Anthropological Archaeology (Kohler and Smith 2018)—may not get the job done because they measure wealth, not class. That distinction is important! The wealth difference between a petty king and a prosperous commoner might not be great. But a king is still a king and a commoner is still a commoner.

15. We would do well to learn about Mesoamerican elites and kings, alongside the Classic Pueblo Ethnographies—a little extra reading. For example, Chase and Chase 1992 and Kurnick and Baron 2016.
16. I won’t apologize for saying this in different places in this book, it’s really important: Bedrock, bottom-up populism colors our attitudes toward the idea of social differentiation in the Southwest. The default assumption is that agrarian Southwestern (and North American) societies were stateless, classless, and egalitarian. Deviations from that happy state are ascribed to grasping aggrandizers (like everyone else, but with diseased ambition); or minimized by appeals to the holy: Would-be leaders are cloaked in sanctity and (from Voltaire) “live without working, at the expense of rascals who work to live.” Not a different class but a different role, rising (up?) from the masses. But in a world of kings and commoners, nobility is a status, not a scam.

17. The last 10–15 years have seen a remarkable explosion of Chaco studies. I note only a few of these intriguing studies here, and many of the rest at https://stevelekson.com/. Most of this new information, considered from beyond Pueblo Space, supports (or does not contradict) the model of Chaco presented in this chapter and in Lekson 2009a and 2015a. Of course these remarkable new data change the narrative: History must accommodate new information. The reviled post hoc accommodative argument: That’s exactly how history works (as discussed in chapter 6). It turns out, for example, that Chaco starts earlier than I/we thought (e.g., Plog and Heitman 2010). As I say in chapter 6, my History of the Ancient Southwest should have a new edition every five years or so. I’m not going to do that, but I hope some younger, more energetic person(s) will take on that challenge, perhaps in nonpaper media (also discussed in chapter 6).

18. Save, perhaps, one: Great Houses were accommodations or hostels, temporary housing for the influx of thousands of pilgrims. That’s a very interesting idea, surely worth consideration.

I think “temporary housing” may be true for elites/nobles visiting the canyon from the countryside, who settled into their not-a-kivas as temporary/seasonal/occasional residences—like a European king’s summer palace, but humbler. But what about all those hundreds of other empty rooms? Rooms to let? Probably not: All those famously empty rooms at Pueblo Bonito and Chetro Ketl are securely identified as storage rooms (that is, not residences/living space) by their distinctive small doors with secondary jambs. This type of door—for it is a type—is familiar from small, stand-alone granaries (which can be nothing but storage) throughout the Four Corners. The same type of doors penetrate the walls of Chaco’s troublesome empty rooms (Lekson 1984:25–28, 266). From the evidence, all those empty rooms at Chaco Great Houses were built to store stuff, not to house people. Or, moving a step away from the evidence, perhaps to appear to store stuff: Conspicuous warehouses proclaiming their proprietors’ (potential) wealth. From outside, you can’t tell if the storerooms are full or empty.

19. Moreover, “there are no monuments to rulers”—holding Chaco up to Maya standards, or to definitions of “state” taken from the half-dozen primary states (China, Mesopotamia, and the rest of that Old Boys Club). Do we need stelae? Archaeology knows of many stratified societies and states that did not celebrate its rulers Maya-fashion. Indeed, the Maya were anomalous in Mesoamerica for making such a big deal of their kings. Aztecs had emperors (who were buried with pomp and circumstance) but few monumental images remain, no towering statues, no stelae.

20. The latter, in particular, annoyed Southwesternists, many of whom visibly wince; to quote me, talking to myself: “King is so loaded, it’s so European, ugh. I appeal to authority: Graeber and Sahlins’s (2017) encyclopedic On Kings. Should I use another word, perhaps a Native or near-Native word? Cacique? Almehnob? Cazonci? Gobernantes (the latter hardly Native)? I think not—those terms imply a cut-and-paste
transfer of office, and I'd rather let Southwestern kings define themselves” (Lekson 2009a:303n199). I use “king” because I speak English, and you are reading English, and “king” is a very flexible term. It does not mean only Louis XIV, but also Irish legend King Cormac, who, if he ever really ruled, ruled a kingdom of surpassing smallness. In American Anthropological Archaeology’s old neo-evolutionary terms, a king like Cormac would be lucky to be a chief.

21. Bonito may get pride of place, but we haven’t excavated Peñasco Blanco, as old and for much of Chaco’s history as big as Bonito; Chetro Ketl was as large, if not as old; and the Navajos say the king actually lived in Pueblo Alto.

22. Dr. Gutierrez has my sincere thanks for directing me away from Tarascans and towards Nahuas; but he is in no way responsible for my use or misuse of Nahua ethnographies.

23. And other Mesoamerican peoples, too. The basic model of multiple noble families sharing governance, commoners obliged to particular nobles, and so forth has been applied to non-Nahuas. Some archaeologists project the altepetl back into the Epiclassic and Early Postclassic (Hirth 2000, 2003, 2008); others say tut-tut, not so fast (Smith 2008a). Models of shared or distributed governance have recently been suggested for late Olmec (by Christopher Pool, reported in Wade 2017a) and Postclassic Tlaxcala (by Lane Fargher and Richard Blanson, reported in Wade 2017b). For Olmec, apparently, that form followed centuries of conventional kingly hierarchy. Chaco’s political structure may derive from a Postclassic Mesoamerican model; or Chaco may have coevolved with Mesoamerican developments; or Chaco itself may have developed key features later seen in the Nahua altepetl. These alternatives are not problems for the application of the model to Chaco; they are research questions which might actually be important.

24. Please: Before you blow this off, get your head out of Pueblo Space and read the longer, extensively referenced version in Lekson 2015a:Appendix A. For the record here, my principal sources: Bernal García and García Zambrano (2006), Gutiérrez (2003, 2011), Hirth (2000, 2003, 2008), Hodge (1997), Lockhart (1992), Ouweneel (1999), and Smith (2008a)—Smith is not fond of my (or others’) use of altepetl, but his works are invaluable references on scale of Nahua city-states, providing many of the numbers offered here (and in chapter 4).

25. Technically, “modulus of elasticity,” but “modulus of rupture” allows me to use, in a nasty way, one of the most annoying Social Theory words, “rupture.” Oh rapture: rupture!

Chapter 4: A Science of the Ancient Southwest

1. Much of this chapter was written in 2010–2011 at the School for Advanced Research in Santa Fe, and posted in various versions on the page “Southwest in the World” (https://steveleekson.com/). My interest in several of these topics is even more ancient: Parts of this chapter revisit bits of my unpublished, unlamented 1988 dissertation! For this book, I have brushed up the SAR essays but I cannot claim to have brought them comprehensively up-to-date. Happily there is much new Southwestern work on some of these topics (e.g., scalar matters and networks/diffusion); unhappily, not much on others (e.g., secondary states).

2. A few of Arizona State University’s many intriguing projects, from their website: Complexities of Ecological and Social Diversity: A Long-Term Perspective; Change Is Hard: The Challenges of Path Dependence; Archaeology of the Human Experience;
and Long-Term Coupled Socioecological Change in the American Southwest and Northern Mexico.

3. Dr. Venkman DID say that. And, to paraphrase Capt. Hector Barbossa: You’d best start believing in science books, missy; you’re in one. But if you’re a kid and don’t know what a slide rule is for: Look at the pictures and just turn the pages. Did Sam Cooke sing that?

4. “Cross-cultural analysis” was the foundation of nineteenth-century anthropology and a staple of twentieth-century anthropology. Today, not so much. A pity. There were (and are) vast databases of cultural information on thousands of societies around the world. (See, for example: http://hraf.yale.edu/.)


6. Not everyone disdains “magic numbers.” Robert Kelly in his recent compendium of hunter-gatherer ethnoarchaeology devoted several pages to group sizes (Kelly 2013: 168ff), in particular the “magic numbers” of 500 for “bands” and 25 for “local groups” from Man the Hunter (Lee and Devore 1968). Martin Wobst (1974) said 475 was likely “minimum breeding population”—a band? And the local group size of 25: Kelly notes that most hunter-gatherer groups were smaller, but

Marcus Hamilton and his colleagues (2007a) lend some support to both “magic” numbers.... They found a remarkable cross-cultural regularity: starting with a single individual, groups seem to increase by a factor of about 4: families consist of 4–5 people, a residential group of about 14–17 people; social aggregations (e.g., at winter camps) of 50–60 people; periodic aggregations of 150–80 people, and an entire ethnic population of 730–950 people. This regularity holds true even for different environments. (Kelly 2013:172)

Severin Fowles devotes a gratifying number of pages (2013:142–148) to “magic numbers” in his excellent Archaeology of Doings. He does not much care for them: “A strange species of Malthusian argument results, one in which scale becomes a kind of agent or invisible hand that makes its own demands upon society” (Fowles 2013:142). Yup, that pretty much sums up my argument—but Fowles is not buying it: "Rarely is it acknowledged that the causal arrow might just as well be reversed. Rarely is it seriously considered that aspiring leaders, prophets, or even new ideologies of human and cosmic order may have themselves been demanding larger communities” (Fowles 2013:142). That works for me too! Once things get going (aspiring leaders with new ideologies) then they take on a life of their own. Like rust or crab grass.

See also the short but excellent discussion of scales and numbers in Dark 1998: 127–133.

7. I use the terms "settlement" and “community” interchangeably. Note that I do NOT define “community” as the minimum unit for social and biological reproduction (Kolb and Snead 1997). For useful reviews, see Kolb and Snead 1997, and N. Mahoney 2000. Also, please note that my use of "scale" and “scalar” differs significantly from recent, exciting research on “urban scaling”—which is commendably cross-cultural (e.g., Ortman and Coffey 2017).

8. Kosse’s and my analyses converged and agreed, independently. We did this work at the same time and place: The University of New Mexico in the mid-1980s. Thanks to Lewis Binford, UNM was a hotbed for this kind of research. Kosse and I were both peripheral to Binford’s department, she in Maxwell Museum, me in the National
Park Service. We were ignorant of each other’s research until a third party introduced us. Thus, our work was independent, and—happily—reached the same conclusions, which makes me think it might be real.

I published first, but Kosse published better. My work appeared in a student journal (Lekson 1985), in an unpublished dissertation (Lekson 1988), and in a chapter in an obscure volume on vernacular architecture (Lekson 1990—material developed in the early 1980s). Kosse’s work was more broadly published (Kosse 1990, 1994, 1996, 2000)—appropriately, I think, because her methods were more rigorous and her goals were broader than mine. Sadly, Kosse died in 1995 and we lost a very talented, very smart archaeologist.

9. In many parts of the world, 2,500 isn’t a city or a government; it’s just a big village. It is quite possible for a peasant village to exceed 2,500 and lack an obvious mayor or king; but in that case, there’s almost always a ruler somewhere else, a king over the hill. That is, the peasant village is part of a larger polity. So the rule holds: 2,500 indicates governance, but governance can be internal or external: A mayor running the town itself, or a king in another city providing rules and controls. That’s handy, because in archaeology we can’t always tell if a settlement is independent or part of a polity. For the 2,500 threshold as a diagnostic, it apparently does not matter.

Are there real-world examples of communities bypassing the K-rule? A key question, which will resurface in the discussion of Roland Fletcher’s (1995) C- and T-limits in urbanism, and critical to Fletcher’s analysis of low-density urbanism (Fletcher 2009). I discuss possible southwestern K-rule scofflaws below, and there are anecdotal exceptions to the K-rule: Permanent, (presumably) independent settlements of more than 2,500 that “bypass” the K-rule by internal subdivision—fissioning in place, as it were. Bypass is usually accomplished by segmentation into clearly defined neighborhoods, barrios, or wards (Smith 2010b).

10. Kosse and I were far from the first to explore the relationship of community/settlement size and sociopolitical complexity. In various versions, that correlation is a (minor) theme in social sciences almost from the beginnings; in its current thread, at least as far back as Naroll (1956), Ember (1963), and Carneiro (1967). Interest within Processual archaeology can be traced from Flannery (1972:423) through Wright (1977), Johnson (1978, 1982; Wright and Johnson 1975), Feinman (1998), Adler (1990), and Kintigh (1994), among others, and of course Kosse and Lekson. And more recently: Binford (2001) and Feinman (2011).

It’s not exclusively on this side of the pond. Clive Gamble (1998:436) noted that “interestingly, Bernard and Killworth’s sociometric study (1973:183) produced a figure of 2,460 as the maximum group size which has some stability without a formalized hierarchy governing interaction.” Bernard and Killworth (1973) reached that figure (2,460) by beginning with the Rule of Six—or, in their case, seven—and building a complex network analysis. Their article, “On the Social Structure of an Ocean-Going Research Vessel and Other Important Things,” is an amusing read (at least until they get statistical) and probably useful for current network analyses in the Southwest.

11. Southwestern archaeology is violently allergic to rulers or kings, and will dose down almost any nostrum which soothes the irritation. One of the best-selling palliatives is “sequential” (versus “simultaneous”) hierarchies: Gregory A. Johnson (1982:396), thinking about the Southwest, suggested that “simultaneous” hierarchies (that is, conventional hierarchy) might not be the only solution to threshold problems, and he offered “sequential hierarchies: an egalitarian alternative.” Sequential hierarchy (aka “heterarchy”) is, he admitted, “difficult...to characterize” (Johnson 1982:403), but
they appear to involve consensus decisions on several levels (nuclear family, extended family, group). “Sequential hierarchy is unlikely to be the only social mechanism allowing large aggregations among egalitarian groups. Ceremony, ritual, or what might be called ‘generalized feather-waving’ is probably another” (Johnson 1982:405). For an attempt to find “sequential hierarchies” in Southwestern prehistory, see Bernardini (1996).

Please note that sequential hierarchies are rare. Johnson notes (1989:386): “We have garden-variety ‘chiefdoms’ and ‘early states’ stacked ten deep under the lab table, but elaborate sequential hierarchies may have been a rare phenomenon.” For this and other rara avis “corporate hierarchy” and “heterarchy” and a posse of “alternative leadership” models, the Southwest’s remarkable (and remarkably stereotyped) Pueblos are the constant, overworked reference—a tribute to Southwestern exceptionalism and Pueblo Space. The rarity of sequential hierarchies seems, to me, a very good reason for considering more common conventional hierarchies first; if those fit, fine: Mission accomplished, as one of our less successful commanders-in-chief said.

Bruce Trigger, in an essay on “Cross-Cultural Comparison and Archaeological Theory” (following his seminal 2003 Understanding Early Civilizations), is unenthusiastic about “sequential hierarchies” and “heterarchies” and “acephalous states”:

Some relativists believe that social inequality and states are optional, rather than inevitable, consequences of increasing social scale, leaving open the possibility that these are only accidental features of modern industrial societies…. Yet so little is known about the archaeological cultures that are cited as possible examples of stateless early civilizations that these conclusions are no more objective than are interpretations of ink blots. (Trigger 2004:53)

Çatalhöyük comes to mind: Declared elite-free (and therefore Pueblo-like) based on a remarkably tiny sample of the site (Hodder 2011).

12. Hard-wiring leading to emergent properties across different social histories is not as bizarre as you may think, gentle reader. I was gladdened to run across the following quote from Bruce Trigger, an archaeologist I have always greatly admired; and saddened because he is no longer with us. In his essay “Retrospection” (Trigger 2006:252), he refers to his last major work, Understanding Early Civilization (2003:252):

I concluded that many of these similarities resulted from specific aspects of human intelligence operating under similar conditions and hence were constrained by modes of thinking and emotional reactions hard-wired into human beings over long periods of biological evolution…. This is an idea I would have been unwilling to entertain twenty years previously…. While these findings have cast doubt on beliefs I have held all my life, I regard this not as an intellectual defeat but as a welcome challenge. Cherished ideas should not be abandoned lightly, but they also should not go unexamined.


14. Kosse (2000) “Some Regularities in Human Group Formation and the Evolution of Societal Complexity” in the Santa Fe Institute’s journal Complexity. The article was published posthumously (edited and introduced by Linda Cordell). For anyone intrigued by the subject of this section, Kosse’s article is well worth reading—it hints at directions more rigorous than my simplified treatment here.
15. Dunbar (2010:27): “For the twenty-odd tribal societies where census data are available, these clan groups turn out to have a mean size of 153. The sizes of all but one of the villages—and clan-like groupings for these societies—fall between one hundred and 230, which is within the range of variation that, statistically, we would expect from the prediction of 150” (Dunbar 2010:25–26). “In traditional societies, village size seems to approximate this, too. Neolithic villages from the Middle East around 6000 BC typically seem to have contained 120 to 190 people, judging from the number of dwellings. And the estimated size of English villages recorded by William the Conqueror’s henchmen in the Domesday Book in 1086 also seems to have been about 150. Similarly, during the eighteenth century the average number of people in a village in every English county except Kent was around 160. (In Kent, it was a hundred).”

16. “Classifications and social conventions allow us to broaden the network of social relationships by making networks of networks, and this in turn allows us to create very large groups indeed. Of course, the level of the relationship is necessarily rather crude but at least it allows us to avoid major social faux pas at the more superficial levels of interaction when we first meet someone we don’t know personally” (Dunbar 2010:80).

17. Van Dyke (2007b:119) estimates a lower number: “A more realistic estimate is derived by imagining a row of spectators shoulder to shoulder every 75 cm around the circumference. In this scenario, approximately 75 people could stand around the 56 m circumference of an 18 m great kiva.” I’m a big guy, and my shoulders are 45–50 cm across (depending on the kind of day I’m having). The ancient people were smaller than us; and Pueblo proxemics are not ours. Van Dyke’s estimate may be overly generous with space: Her movie theater to my bleachers? But her assumptions are more humane and more pleasant to contemplate.

18. The Rule of Six (or Seven) began as an industrial observation. Frederick Taylor (1911) noted that the ratio of producers to nonproducers (i.e., managers) in his factory studies was 6 or 7 to 1. Vytautas A. Graičiūnas (1937 [1933]) first modeled this observation mathematically, demonstrating that while size of an organization increases arithmetically, the potential interactions increase geometrically; Graičiūnas (1937:186) concluded that “no supervisor can supervise directly the work of more than five or at most six subordinates.” That is, there exists an observable threshold at 6 (or 7) beyond which management efficiencies rapidly decay.

19. Gregory Johnson (1978) cited sources as far back as midcentury, Stanley Udy (1958, 1970): “The maximum number of items to which an individual can give simultaneous attention ranges between three and seven with a mode at five.... Udy’s (1970:50) own data suggest that in activity coordination, this number is probably four” (Johnson 1978:105). Johnson considered cost-benefit analysis of information sources and hierarchies, and concluded: “Across the whole organizational range considered, however, the mean number of organizational units integrated by an immediate superior unit in administrative hierarchy generated on an assumption of efficiency maximization is 3.66, with a range of 2.33 to 6.00. This mean of 3.66 is a reasonable approximation of Udy’s figure of 4.0, and the range of 2.33 to 6.00 is remarkably close to that of 3.0 to 7.0 reported in the psychological literature” (1978:105).

In a later paper looking at small-group studies, Gregory Johnson (1982:392–393) applied this “rule of thumb” to specific cases, and noted an evident “organizational threshold” at six or seven individuals. Johnson cites studies of “capacity of an individual to monitor and process information” that suggest that “span of absolute judgment of unidimensional stimuli” and “span of immediate memory...simultaneously retained”
are “fairly narrow, and average about 7” (1982:393–394). “If hierarchy development is related to some kind of scalar stress, why should it occur at around group size 6? Unfortunately this question is much more easily asked than answered” (1982:393).

“There appear, then, to be rather severe limits on the maximum size of task-oriented groups that are organized horizontally (nonhierarchically), and these limits may be related to individual information-processing capacity” (Johnson 1982:394; see also Johnson 1982:410–413). These limits caused “scalar stress” (Johnson 1982)—cognitive limits on human information processing. Johnson defined “scalar stress” as cognitive limits on human information processing. “‘Scalar stress’ measures the number of potential or real face-to-face interactions among decision makers in group of n people, expressed mathematically by the formula (n^2 – n)/2” (Johnson 1982:394).

20. Cross-cultural organizational regularities were argued to be grounded in cognitive constraints, shared by all humans, that limit the number of pieces or channels of information that can be simultaneously processed by the human brain” (Bernardini 1996:372). Citing the usual suspects, Bernardini accepted that “the maximum information processing workload for an individual is exceeded at group sizes of greater than about six people. That is, for consensual group decisions, each person can maximally consider the views of about five other people, plus his own, to arrive at a choice” (Bernardini 1996:374–375).

21. Bernard and Killworth’s (1973) analysis began with a factor of seven and reached a maximum hierarchy-free size of 2,460. How ’bout that?

22. Carniero, Yoffee, and Trigger may seem an unlike series of citations but I see them as theoretically sequential: Carniero 1981 → Trigger 2003 → Yoffee 2005. All theory is local.

23. Hewett’s identification of Rio Grande Pueblos with Greek city-state democracies was a scam for the Pueblo Mystique, I think. Hewett had to know better—some key administrative institutions in eastern Pueblos can be downright dictatorial. A fact John Ware (2014) makes, but sort of which misses the point: The dictating is hidden, behind the scenes, and not through institutions of rulership and noble-commoner class structures.

24. We have remarkable geographic information—atlas, really—for all of the Pecos periods: Pithouse/Pueblo I (Young and Herr 2012; Reed 2000; Wilshusen et al. 2012); Pueblo II (sensu Chaco: Marshall et al. 1979; Powers et al. 1983; Fowler and Stein 1992; and most recently, Heitman et al. 2016), Pueblo III (Adler, ed., 1996), Pueblo IV (Adams and Duff 2004); and wonderful longitudinal GIS projects: The Coalescent Communities project (Hill et al. 2004; Wilcox 1999; Wilcox et al. 2007) which evolved into the Southwest Social Networks Project (Mills, Clark, et al. 2013; Mills, Roberts, et al. 2013; Mills et al. 2015); and State CRM GIS databases; and more, of course.

Since this is officially my Last Book, I cannot refrain from noting that I (and Bill Lipe) designed the conference that produced the Pueblo III volume (ably brought to print by Michael Adler in 1996), a format followed by subsequent Pithouse/Pueblo I and Pueblo IV volumes—all published by the University of Arizona Press. And of course I had something to do with the Pueblo II work. Making maps is fun!

25. Nancy Mahoney’s (2000:Table 2.1) estimates of “maximum momentary population” for several Chacoan “outlier” communities ranged from about 200 to about 970 people. Beyond Chaco Canyon, the largest eleventh-century Pueblo towns were in the Mimbres area. Mimbres towns were at most only a few hundred people: Anyon and LeBlanc (1984:192) estimated “a maximal Galaz village size of 300 people.” That
figure agrees reasonably well with the scale proposed by Shafer (2003:133) for NAN Ranch Ruin: “24-plus” “extended family households” or perhaps 250–300 people (my estimate, not Shafer’s).

For Pueblo III (twelfth–thirteenth centuries), Michael Adler (1996:97) says the largest Mesa Verde settlements were about 1,500 people. Adler (1996:105), using “momentary population” estimates, notes “empirical support for a demographic size limit of between 1,000 and 1,500 people in Anasazi communities of the Mesa Verde region.” Thus, “we are stuck in that grey area described by Kosse (1990) and Lekson (1988), in which community size can be used to argue for either emergent sociopolitical complexity or the lack of complexity” (Adler 1996:105).

Yellow Jacket was the very largest Mesa Verde town in the thirteenth century. Kristin Kuckelman estimates Yellow Jacket’s maximum population at between 850 to 1,360 people. See: http://www.crowcanyon.org/publications/yellow_jacket_pueblo.asp

Scott Ortman (2009:Appendix A) studied the population of the largest northern Rio Grande towns in detail; for Sapawe (the largest northern Rio Grande town), Ortman estimates a maximum population of just over 2,300. Very close! But no cigar: Sapawe was occupied for a maximum of 150 years, probably less; apparently, after a few generations, fragmenting into multiple daughter communities. See also Duwe and others 2016.

26. To quote a sometimes reliable source: “The ten most populous Rio Grande Pueblos averaged about 400 residents each during the eighteenth and nineteenth centuries; the western New Mexico Pueblos averaged about 1000 residents during the same period (Simmons 1979:Table 1; Zubrow 1974:Table 2). After the Revolt of 1680, no Pueblo was ever larger than about 1500, except Zuni which occasionally peaked at about 2500 (but which averaged about 1500)” (Lekson 1984:272).

Schroeder (1979:246) suggests that Acoma had 6,000 people at the time of Coronado, but surely this is incorrect. The estimate is based on a Spanish estimate of 500 houses atop Acoma’s mesa; currently there are fewer than 100 houses (mostly unoccupied, or occupied only on special occasions) and the mesa top is pretty crowded. The Spanish account was probably...enthusiastic.

Pecos was reputed to be the largest Rio Grande Pueblo when the Spanish arrived; their accounts say 2,000 people. The archaeological data support only half that size. See: http://www.cr.nps.gov/history/online_books/pecos/cris/chap7.htm.

27. Whalen and others 2010. As had David Wilcox before them (in a conference paper), provoking a response from yours truly (Lekson 1999b). All of us are working with precisely the same data—early accounts, etc. My argument was that Di Peso knew the site better than anyone else, having actually worked at Paquimé—which none of us have. David Phillips and David Wilcox walked over the supposed “east wing” and found only limited evidence for archaeological architecture; both of these gentlemen are excellent field archaeologists, so I suspect Whalen and colleagues (2010) are probably correct about the east wing; that is, there isn’t an east wing, an area in which Di Peso was not able to excavate. I am disturbed, however, at the seemingly gleeful reduction of number of stories, numbers of rooms, and so forth in west and north wings and other areas where Di Peso actually excavated. And although Whalen and Minnis 2001 (and others) seem intent on whittling it down, the Casas Grandes region was pretty darn big; all this is discussed at even more length in Lekson 2009a:212–214.

28. Kosse (1996:90), looking at a “nonrandom sample of 103 societies” notes that 500 is a threshold for “CAN be” complex; 2,500 is a threshold for “MUST BE” complex. Note that she is referring to the total population of a single settlement polity. “With
so much ambiguity and variability of behavior [between 500 and 2,500] it is not surprising that the material evidence for middle-range societies is less than clear-cut” (Kosse 1996:90). John Bodley, in “The Power of Scale,” is big on 500 as a complexity threshold (Bodley 2003:87–89) but it’s not quite clear where he gets this figure.

29. Yet curiously absent or muted in early textbooks. Kidder’s (1924) Introduction to the Study of Southwestern Archaeology mentions the introduction of maize, but that’s it. Hewett’s 1930 Ancient Life even seems to discount maize: “No one carried agriculture to other people and taught it to them” (p. 35), after which Mesoamerica is absent—although Hewett later in the book suggests that Casas Grandes was Aztlan (p. 373)! The first edition of McGregor’s Southwestern Archaeology (1941) had four index entries for “Mexican influence” and “Mexican traits.” All were for Hohokam. His second edition (1965) upped the ante to nine, again all for Hohokam. Gladwin’s (1957) A History of the Ancient Southwest has nothing; nor does Martin and Plog (1973) Archaeology of Arizona—beyond the obligatory nod to maize. It is only with Cordell’s several editions of Prehistory of the Southwest (which appeared in 1984, 1997, and 2016) that textbooks took Mesoamerica seriously.

30. V. Gordon Childe—probably the most significant mid-twentieth-century archaeologist—published What Happened in History (1942), a very influential book about diffusion of early civilizations from the Near East into western Europe.

31. Carl Sauer and his crew more or less invented “cultural landscapes,” but they were big on diffusion too; two New World examples: Sauer 1932, 1952. Kroeber 1940 defined Stimulus Diffusion: “the birth of a pattern new to the culture in which it develops, though not completely new in human culture. There is historical connection and dependence, but there is also originality…. Stimulus diffusion might be defined as new pattern growth initiated by precedent in a foreign culture” (Kroeber 1940:20). Stimulus diffusion is the idea of the complex or system which is accepted, but it remains for the receiving culture to develop a new content…. Obviously this process is one which will ordinarily leave a minimum of historical evidence…. [M]uch diffusion takes place below the surface of historical record…. [W]hile systems or complexes in two or more cultures may correspond in functional effect, the specific items of cultural content, upon which historians ordinarily rely in proving connection, are likely to be few or even wholly absent. Positive proofs of the operation of idea-diffusion are therefore, in the nature of the case, difficult to secure long after the act, or wherever the historical record is not quite full. (Kroeber 1940:1–2)

Thinking like an anthropologist, and not like a historian, Kroeber looks at parallels between Greek and Hindu theater: “One inference may be drawn from this example: that contacts did occur and that they did have influence far beyond what we could directly infer from the preserved documentary literature. In other words the absence of direct historical records as to connections between Greece and India is no proof that there was no connection” (Kroeber 1940:12). Historians might have a problem with that assertion. Archaeologists should not.

32. Ford died before his remarkable work was published, and his book was seldom cited; but see Clark and Knoll 2005. Di Peso lived to promote his ideas—which resonated with some Southwesterners—and inspired a whole shelf of Di Peso–inspired collected papers: Woosley and Ravesloot 1993; Reyman 1995; Schaafsma and Riley 1999; most recently Minnis and Whalen 2015; and others. But Di Peso’s notions were out of
temper with the times: New and Processual archaeology strongly favored local over distant at Paquimé (e.g., Whalen and Minnis 2003).

33. Kristian Kristiansen and Thomas B. Larsson, 2005, *The Rise of Bronze Age Society: Travels, Transmissions and Transformations*. As noted, Kristiansen and Larsson (2005) avoided the word “diffusion”; but it’s not the word so much as the legitimacy and importance of long-distance interactions that matter. Kristiansen elsewhere notes that in Europe

we have witnessed the silent collapse of the dominant post-processual framework, as it did not account for the kinds of evidence we have seen emerge during the last ten years. And neither did the processual framework. In short: we are in a period of theoretical and methodological experimentation and re-orientations, where everything that was “forbidden” research 10–15 years ago are now among the hottest themes: mobility, migration, warfare, comparative analysis, evolution, and the return of grand narratives. (Kristiansen 2014:14)

34. Migration never really vanished in Arizona, thanks to Emil Haury, Lex Lindsay, and Jeff Reid; but interest was muted during the heyday of New Archaeology; e.g., Haury 1958, Lindsay 1987, Reid 1997.

35. And an explosion of new migration studies: Cameron 1995; Clark 2001; Kohler et al. 2010; Lekson et al. 2002; Ortman 2012; among many others. See also: https://www.archaeologysouthwest.org/what-we-do/investigations/salado/.

36. The editors of a recent volume on trade and exchange lament that “archaeological interest in trade and exchange...has declined in recent decades” (Agbe-Davies and Bauer 2010:13) as archaeological attention turned away from global or metanarratives to microhistories.

37. World History overlaps with and could be confused with “Big History,” a less-useful current enthusiasm. Big History tries to write global histories starting with the Big Bang. I’m not sure I see the need, or the point.

38. Some notable New World exceptions: For example, Chase-Dunn 2011; Chase-Dunn and Hall 1998; Peregrine 1996; Smith and Berdan 2003. For a recent review of World Systems’ sometimes rocky relationship with archaeology, see Hall et al. 2011.


40. “Mound” is a strange, belittling word for the largest pyramid north of Mexico, larger in base area than the pyramids of Giza. North of Mexico, every boat is a canoe, every pyramid a mound, and every king a chief.

41. For an excellent recent account of Mesoamerica in the Eastern US, see Pauketat 2009a:136–145. Discussion of the Mississippi Valley/Southeast and Mesoamerica was not uncommon up to midcentury, but was almost completely foreclosed thereafter. See also chapters in White 2005.

In a 2012 book-length review of Recent Developments in Southeastern Archaeology, David Anderson and Kenneth Sassaman (forward thinkers, both!) devote one paragraph to the topic. Their enthusiasm is guarded at best, concluding,

Some scholars see these interregional connections [between the Southeast and Mesoamerica] as more significant and enduring throughout prehistory than currently assumed, and argue that the comparative study of the large-scale processes common to and possibly shared between these regions is worthy of far more research than they receive at present (e.g., Lekson and
42. Engraved and carved shell pectorals that indicate interaction between the Huasteca and Mississippian are, in fact, earlier in the north. “Engraved shells in the southeastern United States date to a later era [than Classic-period Maya examples], predating those from the Huasteca. The majority of engraved shells were found at the archaeological site of Spiro, Oklahoma. Whereas the apogee of Spiro dates to the Harlan Phase (AD 950–1200), those from the Huasteca emerged between the thirteenth and fifteenth centuries” (Dávila Cabrera 2015:146).

43. Cahokia’s map provides powerful data: The place looks like a major Mesoamerican city, while Chaco does not. It is not clear to me why trinkets trump monuments.

44. It would be interesting to do the metallurgy on Huastec copper artifacts; where did the metal come from? From West Mexico or the Upper Peninsula?

45. We might also consult with Political Scientists of the International Relations bent, who have whole typologies for cultural connections on the state level—which is the level we are on; see for example Dark 1998:133–137.

46. This would be something like the “comparative archaeology” I (and others: Cordell et al. 1994:175–178 for my bit) envisioned twenty-five years ago.

47. I’ve had a few successes, lifting the Glass Ceiling: I was asked to contribute a chapter on Chaco to a volume on palaces (Lekson 2006a in Christie and Sarro 2006); another to a volume in the Cambridge History of the World on “A World with States, Empires, and Networks” (Lekson 2015b in Benjamin 2015); and a third to a volume on “Ritual in Archaic States” (Lekson 2016 in Murphy 2016). Those editors sought me out, not vice versa. I explained to them that most Southwesternists would shudder to see Chaco in such exalted company, crashing those toney parties. They insisted. I’m sure some readers are quoting to themselves A. Lincoln (attrib.) on fooling some of the people all of the time. I’d prefer to think that you can’t fool all of the people all of the time. That is, right-thinking archaeologists unencumbered by Southwestern biases see Chaco for what it was. Anyway, it’s a start.

48. I’ve read Societies against the State (Clastres 1989) and Hierarchy in the Forest (Boehm 2001). I found the latter unconvincing because Boehm seems to assume that all hunter-gatherers were egalitarian; that’s not so. Yes, there are ways to live that avoid “states” as I define that term here; Pueblos do that. But so many complicated agricultural societies go the route of state/king (maybe all? Trigger 2003) that we should question the special pleading of Pueblo Space which keeps Chaco out of that very, very common condition.

49. So did David Wilcox (1993). Neither of us had much luck.

50. Actually, Chaco was recognized by the UN (UNESCO) as a World Heritage site, and they got it right: “Chaco Canyon, a major centre of ancestral Pueblo culture between 850 and 1250, was a focus for ceremonials, trade and political activity for the prehistoric Four Corners area. Chaco is remarkable for its monumental public and ceremonial buildings and its distinctive architecture—it has an ancient urban ceremonial centre that is unlike anything constructed before or since.”—this from UNESCO’s World Heritage website. Ritual, mysteries, and spirituality dialed down, not a hint of pilgrimage, and the political, economic, and ceremonial dialed up. Of course it’s dated, but I like it. (And, no, I didn’t write it.)

51. A formulation that owes much to Severin Fowles’s careful reading of an early version of this chapter—but for which he bears no blame, only my thanks for extracting
and clarifying my embedded arguments. Still, this may seem a low bar for archaeologists accustomed to old checklist definitions of states: “Writing,” “Weights and measures,” “standing armies,” “craft guilds,” and so forth. Those lists reflect Victorian-era thinking, with the British Empire as the obvious peak of progress. Britain had those things, plus pomp and glory. Ancient state candidates were required to satisfy the list. “State” became a sort of Old Boy’s Club of approved civilizations—those meeting the Committee’s criteria. Many archaeologists research statelike polities, and there is growing interest in comparisons, contrasts, and continuities rather than exclusionary categories. Ben Nelson said it well: “not if they were complex, but how they were complex”—although elsewhere I quibble with this quote, a bit. If our field of view is confined by Pueblo Space, we can’t address that challenge effectively. Can’t say it too often: We have to recognize that Chaco was a something like a state just to ask the right questions.

52. “Secondary state” has other meanings. In International Relations, “secondary state” means a minor state which chooses to be unaligned with the great powers. It’s like “primate center,” archaeology jargon sometimes encountered in the state business. “Primate center,” for most civilians, means “monkey house.”

53. The word “state” appears only twice in Recent Developments in Southeastern Archaeology, and then only as counterexamples; for example, “Whether most tributary economies [in Mississippian chiefdoms] were anywhere near as formal as those in state-level societies is highly debatable” (Anderson and Sassaman 2012:168).


55. This comes from a 1998 Smithsonian Institution Press book, The Cahokia Chiefdom, by George R. Milner (republished by the University of Florida Press in 2006). The Cahokia Chiefdom was hailed in an American Antiquity review as “a tonic against earlier hasty overestimations of Cahokia’s size and complexity.” What they saw as wisdom I see as bias, Glass Ceiling, and misplaced parsimony:

> Only when the simpler sociopolitical model can be shown to be inconsistent with the archaeological data can scholars convincingly argue that Cahokia was something other than an unusually large version of the Mississippian chiefdoms that developed elsewhere in eastern North America. (Milner 1998:176)

56. Rereading Chiefdoms and Other Archaeological Delusions for writing this book, I am struck by the parallel themes and thinking between Pauketat’s and my work—not to drag Tim down into my swamp. We know each other, we cite each other, and we have participated in several seminars and field conferences. But we came to our views independently, probably by very different paths. Much like the parallel but independent research by Kris Kosse and me on population thresholds, I am greatly heartened by similarities between Pauketat’s work and mine. As Tim might agree, it’s lonely work and not much fun. Pauketat may or may not welcome my claim for similarities, but I have found his work both inspirational and comforting—in a confirmatory way. But don’t hold that against him, please.

57. The equation of Pauketat’s “civilization” and my notion of “governance” seems, to me, appropriate; see Pauketat 2007:17. But he, of course, means much more than political hierarchy and structures—those are already allowed (as chiefdoms) in the
Mississippi Valley. "I need a word, and civilization is awaiting reclamation from the ethnocentrism and racism of an earlier anthropology. Civilization as I use it isn't a qualitative transformation, a dawning of high culture in some ancient world. And it isn't an advanced type of social system. It didn't just happen once on each island or continental land mass. No. Civilization is an ongoing historical process, not an evolutionary phenomenon" (Pauketat 2007:17–18; original emphasis).

58. There were consequences. It’s one thing to damn the torpedoes, another to take one amidships. There’s a dismissive passage in Flannery and Marcus’s (2012:157) Creation of Inequality, which seems to be aimed at Chaco and at me (perhaps I just flatter myself): “To some archaeologists, familiar mainly with sites in the Southwest, Pueblo Bonito seems as spectacular as the ancient cities of Mexico.” If that was a shot across my bow, I must note that I’m reasonably familiar with the ancient cities of Mexico.

I am aware that some people think my fussing over Chaco is parvenu pretension, crowing on the dunghill. I don’t even like the place: It’s a black hole for archaeology, a drain on cheer and energy, and I’m really tired of writing about it. (So too may you be, dear reader.) But it’s important to get Chaco right; just doing my job, trying to get it right.

59. The contributors to the landmark Archaic States asked two questions:

How do third- and fourth-generation states differ from first- and second-generation states?…and what to call the polities on the periphery of states when they acquire some of the trappings of that state but are never really incorporated into it? (Marcus and Feinman 1998:6)

Norman Yoffee’s 2005 response, Myths of the Archaic State, was largely critical; but he too was much intrigued by secondary states:

The rise of secondary states is not separable from regional trends and the impingement of prior states…. While archaeological research must perforce be focused on sites and in local regions, explanations for social and political change must often seek larger historical contexts, as comparisons with other ancient states suggest. (Yoffee 2005:192)


The typological distinction [between primary and secondary states] should be rethought in terms of processes of state formation based on how emerging state institutions were financed either internally [primary] or externally [secondary]. Institutions that constitute states “are not exportable” (Price 1987:182). States should not be considered as a trait that can diffuse; they developed both in isolation and equally in association with other states. All chiefly leaders wish to be kings, and the origin of states depended on whether would-be sovereigns could develop systems of finance to support state institutions and to exclude rights of the common horde.” (Earle 2016:302)
He concludes: “I question whether the distinction between primary and secondary states has real analytical value…. Processes of finance were the drivers here, not the borrowing of ideas (Earle 2016:306).

I wonder: Why not borrow ideas? Chiefs want to become kings; they do not invent kingship. Earle puts the economic cart before the aggrandizing horse. His point, which seems reasonable, is that if someone wants to be king, they have to pay for it. But that’s another problem—and an American question.

61. The Aztecs established an empire over many squabbling altepetl, in the “fifth [and final] generation state.”

62. In too many conversations about “kings” in the Southwest, my colleagues dropped the “D” word: Despot. A strong theme in American bias against royalty is the notion that all kings are nasty. Read Plato, read Voltaire: Kings can be benign despots or enlightened absolutists or philosopher kings. Theoretically, anyway.

63. The full quote from Carl Sauer’s (1954) comments on Paul Kirchhoff’s “Gatherers and Farmers in the Greater Southwest” in American Anthropologist:

The notion of Apartheid of a Southwest Culture would not have arisen, it seems to me, if Mexico had been the center from which anthropological studies spread through North America. Kirchhoff has approached the matter from the south; our students, coming into the Southwest from the north, saw that they were getting into something strongly different, but stopped at the international border and failed to see how much of the complex lay beyond. Moreover, the notions about the Southwest Culture originated in the years when it was considered proper to infer endemism as dominant in culture, to maximize development in situ and to minimize the significance of dispersal and diffusion. A familiar example is the postulated succession of stages from Basketmaker I to Pueblo V, construed mainly as autochthonous “evolution.” (Sauer 1954:553, italics original)

In the past I used a version of this quote from Sauer’s Selected Essays 1963–1975, edited by Robert Callahan (1981). I underestimated Callahan’s note that he had “edited and abridged” Sauer’s original, for example replacing “Apartheid” with “independence and isolation” (Callahan 1981:159). Apartheid literally means “separateness”; it became South African policy in 1948, shortly before Sauer’s essay was written—but probably not the word we’d use today. Callahan’s editing did no violence to Sauer’s original essay, but I apologize for presenting an altered anthologized version and not the original text from American Anthropologist.

64. Early kings presumably had divine right, or a mandate of heaven, or some other celestial blessing and backing. Many were themselves divine (Trigger 2003). Divine kingship worked until it ran smack into the Enlightenment, which was sour on religion and skeptical about divinity of kings—but not initially antiroyal. It’s not necessarily bad to have a king. The English king gave up his divine right as late as 1689, but not (ultimately) his office. Divine or not, it’s important to realize that kings and royalty remained qualitatively different: They did not cease to be kings in exile, in defeat, or in a basement in the Urals.

65. I use the word “populist” in its original, benign meaning; in my desk dictionary “a believer in or advocate of the rights, wisdom, or virtues of the common people.” During review and revision of this book, “populist” got hijacked by crazies.

66. And a Canadian, Bruce Trigger (2003:142): “In early civilizations and complex preindustrial societies, inequality was regarded as a normal condition…rather than a social evil…. If egalitarian social organization was known to people in early civiliza-
tions, it was as a feature of small-scale and usually despised societies beyond the pale.” Of course, Trigger was a Marxist.

67. The American view underwrites Collective Action accounts, such as Blanton and Fargher’s (2008) remarkable Collective Action in the Formation of Pre-Modern States (see also Fargher 2016). What’s in it for the working stiff? Eric Wolf (1999) shows us a state can be an ideology in Envisioning Power: Ideologies of Dominance and Crisis. We do it this way because we do it this way, not because of cost-benefit ratios.

68. How much agency does an individual in a small, traditional society have? Some, to be sure; but exercise too much agency in a Pueblo (for example) and you’ll get stern instructions on your proper limits, how to behave. Exercise a little more agency and they’ll escort you to the edge of town, which is sometimes coterminous with the edge of a mesa.

69. It’s a lot cheaper to dig households than pyramids. Just pointing that out. I’m sure that had no bearing on the shift in anthropological interests from kings to commoners.

70. Kent Flannery (1999) examined this process in an article titled “Process and Agency in Early State Formation.” (See also “How to Create a Kingdom” in Flannery and Marcus 2012:341–366—less useful for my argument here.) Since all his examples are historic and well documented, they are all secondary states—“early” for their various areas, but still secondary. And all the examples involved aggressive “alpha males” who used military conquest and expansion to consolidate their power—“the subjugation of rival polities” (Flannery 1999:16). Since Chaco lacked rival polities (with the possible exception of Hohokam, which Chaco clearly did NOT subjugate) I doubt that was the process at Chaco—although David Wilcox (1993) long ago suggested Chaco was empire-like within the Four Corners region (his later interpretations were less martial, e.g., Wilcox 1999). Some of Flannery’s processes may apply: His early states “each made use of role models” (Flannery 1999:14; that is, they copied and modified familiar sociopolitical structures); after taking charge, “all state founders, however autocratic, need support; they get it by making a least a pretense of power-sharing” with councils and similar bodies (p. 15); and they all change the existing ideologies “to one more appropriate for a state…this is usually not through revolutionary moves to promote wholesale overthrow of the system, but through self-interested change in the meaning of existing relations” (p. 15), “this transformation of ideology is a universal in state formation” (p. 17). Those things probably happened at Chaco: Role models from Mesoamerica, power sharing through Great Kiva assemblies, ideological change suggested by things like the dramatic stylistic shifts from Basketmaker III–Pueblo I to Pueblo II–III rock art and the sudden and broad adoption of Dogoszhi-style pottery design (Neitzel et al. 2002).

71. Political Science does; but Political Science generally only goes back as far as Aristotle. For most of human experience, it’s up to archaeologists. And we are on it: Comparing “complex societies” has become a genre in our field and a productive one indeed; e.g., chapters in Feinman and Marcus 1998; chapters in Smith 2012.

72. Tim Pauketat (2007) used Morton Fried’s (1967, 1978) ideas about secondary states to explain the proliferation of post-Cahokia chiefdoms in the Southeast (citing Anderson’s map [1999:Figure 15.5]; Pauketat’s Figure 4.2). “Secondary polities” broke the rules because they skipped evolutionary steps, and required external drivers—which didn’t sit well with neo-evolutionists:

However, a lot of water has passed under the definitional bridge since Wright and Johnson codified their thoughts on hierarchy. And while there still exists a shoot-from-the-hip I-know-a-state-when-I-see-one attitude among some
[Yoffee’s Rule], there is also a growing recognition that many presumed states in many places can’t be defined using a single standard or checklist of traits. (Pauketat 2007:144)

73. An excellent short summary by Marcus and Sabloff (2008:4–12). Comparative studies are not fashionable in an era of “social construction” of this and that. As Roland Fletcher notes, “Sociality must properly be understood in local terms.... But very soon we will have to confront cross-comparability because it actually matters” (Fletcher 2010). And he argues for the physical, material aspects of urbanism (population, area, etc.).

74. Price (1978:170–175) offers, for example, Teotihuacan’s expansion into Guatemala. She notes that Teotihuacan was highly urbanized (that is, big and dense), but Maya cities were not (Price 1978:175). This does not bother her. “This lack of true cities may be taken as a strong suggestion of secondary status...because known pristine sequences tend strongly to develop cities, and to do so in a homotaxially early phase of their evolution, the lack of urban settlements may be taken with some confidence to indicate secondary status” (Price 1978:175–176). That is, secondary states should NOT have big and dense, Western-style cities.

75. Cities without the state: Norman Yoffee, in Myths of the Archaic State, suggests that the city defines the state: “Whereas neo-evolutionists seem to have regarded cities as place-holders at the top of settlement hierarchies they called states, I argue that cities were the transformative social environments in which states were themselves created” (Yoffee: 2005:45).

Several people whose work I admire argue that it’s cities that create civilization, not the reverse. Norman Yoffee (2005:42–90; see also Yoffee and Terrenato 2015) and Geoffrey West (2017:295) put cities first: West’s discussion is subtitled “Cities as the Great Social Incubator.”

76. Amos Rapoport’s (1993:32) definition of city: “The culturally neutral, cross-culturally valid definition of a city is also as an instrument for the organization of surrounding territory, making it dependent, integrating regions, and generating effective space.”

77. Rapoport (1969, 1976, 1982), in my opinion, was to vernacular architecture whatBinford was to hunter-gatherers. His work, though old, weathered well: It is systematic, logical, and factually based—and the facts haven’t changed (much). And his books are still cited, particularly by those working comparative approaches (e.g., Blanton 1994; Smith 2008a; Fletcher 1995). There has been much useful scholarship along the same lines since Rapoport’s. To find it, the field of “vernacular architecture” is a very good place to start (e.g., Oliver 1998; Vellinga et al. 2008).

78. Alas, there is no succinct quote where Rapoport says: Chaco was a capital. If there was, when I am dead and chested you would find it written on my heart. He simply lists Chaco among his early or preindustrial capitals—which, in a way, is even cooler. For example, in discussions of centrality and roads: “Centrality is reinforced by an emphasis on meaning and symbolism, which can be achieved in various ways. One is by assemblies at the capital of chiefs and subordinate kings, another through the residence there of conquered kings or through pilgrimage” (Rapoport 1993:34). These capitals were the center of road networks, and again he cites Chaco among his examples.

And on labor investment: “Capitals, and especially their cores, are front regions par excellence, that is, they communicate the desired meanings. This is why so many resources are devoted to them. The construction of monumental and ceremonial
complexes demands much effort and planning, and astonishing amounts of labour," and here he cites Chaco and Cahokia, alongside Peru and Maya (Rapoport 1993: 35–36).

79. Mesoamerican capitals were also reinforcement of political power. Mesoamerican capitals were designed as much for polity as ritual:

For Mesoamerican archaeologists a focus on political authority resonates well with indigenous views known from pre-Hispanic and early Colonial-period documents. As noted by many scholars, indigenous terms that accord most closely with our notion of cities refer to the seats of power of ruling dynasties that extend beyond particular settlements to the broader territory claimed by the ruler...consistent with the trend in archaeological theory towards a more relational view of urbanism.” (Joyce 2010:189–190)

George Cowgill (2004:535) noted that many early cities were, in fact, creations: “Many of the first cities (some would say all) may have been intentionally created in their entirety to serve the interests of powerful individuals or groups. In theoretical terms, the idea of cities as inventions is appealing, but much remains to be done to develop this notion.” The cities of the Chaco Meridian may well have been inventions of the elites.

80. Much of this may sound familiar today; but recall that Rapoport published this in 1993. Wendy Ashmore’s “Site Planning Principles and Concepts of Directionality among the Ancient Maya” was published in 1991. “Anasazi ritual landscapes” hit the limelight one year later (Stein and Lekson 1992). The British invasion of “phenomenological” landscape studies mostly postdates 1993 (reviewed in Lekson 1996) and did not really penetrate American Archaeology for several (many?) years thereafter. Rapo was pomo before mo went po.

81. His data include more than 300 “hunter-gatherer communities, small scale agricultural settlements, agrarian-based urban communities, and industrial urban communities” (Fletcher 1997:Fig. 4.3). One could, of course, quibble with the size and nature of the sample—“a world-wide grab sample” (Fletcher 1995:75; see also his note 6 on page 235). But I am not aware of any comparable data set that ranges from mobile hunter-gatherers to modern cities.

82. There are compendia of cities, databases and so forth, but they are (to my knowledge) largely limited to historic periods. Combining pre-history with history is a challenge. Constantinos Doxiadis took a stab at it, with (for example) his interesting 1968 book Ekistics. It’s my impression that Ekistics faded; Googling the Athens Center of Ekistics brings no recent returns.

83. To fully explicate how this matters to Fletcher’s theories is beyond the scope of this short section. Easier (and better) to read Fletcher’s book (paying particular attention to his Figure 4.16).

84. Marcus and Sabloff (2008:13) listed seven “elements often invoked in definitions of the city”:

1. Heterogeneous people, occupations, crafts, classes, and statuses.
2. Diverse political, social, religious, economic, and administrative buildings, institutions, wards, neighborhoods and associated personnel.
3. Dense packing or crowding of residential and nonresidential structures.
4. A monumental core of unique buildings (for example, a cathedral or temple, a library, a palace, a central market, a courthouse, or a set of administrative buildings).
5. A skyline or “city profile” that shows maximum building height at the center of the city and less and less height as one moves away from the city center.

6. A central focus—sometimes a sacred center, whose access was restricted and where temples predominated, and sometimes an administrative center where governmental buildings were concentrated.

7. Special organizational features, such as grid-like modules like city blocks, streets, city walls, ward or barrio walls, canals, sewers, aqueducts, parks, and public squares.

85. “In Mesoamerica alone, most ancient cities fall into the [i.e., Fletcher’s] low-density group. Many of the early cities in South America, Africa, and Southeast Asia can also be characterized as low-density cities. Archaeologists and historians working in these areas have generated considerable information on this ancient urban form, but comparative analysis has only recently begun” (Isendahl and Smith 2013:133).

86. A huge literature on Teotihuacan, of course; I rely on Cowgill 2008. The Aztecs jammed their capital onto a small island, the only land they could get (or build). Perhaps that’s why Tenochtitlan had European urban densities.

87. But Pueblo IV villages were not cities. They did not service or transform regions. And they were not capitals.

88. Kenneth Hirth (2008) suggests that the centers Smith calls cities were at most “incidental urbanism”—the altepetl itself was the operative social and political unit, and the cluster of palaces and temples near its conceptual center (which the Spanish called capitals) were simply clusters of palaces and temples. You say paTAYto, I say pohTAHto; let’s call the whole thing off.

89. “It appears that farming in urban contexts was quite widespread around the world through history and prehistory (although systematic comparative research has yet to be done)” (Isendahl and Smith 2013:142).

90. Much like the problem we saw with “states”—because Pueblo Space denies states, the referents for Southwestern archaeology are often out of date or inappropriate. So, too, for “city.”


In the poster, he applied the regional definition of city to Southwest cases, noted that urbanism is possible absent the state, and evaluated my claims (Lekson et al. 2006) that Chaco was a city. Working with the regional definition, he concluded that Chaco was not a city, but Phoenix Basin Hohokam might have been. It was a good poster and I liked it, although of course I differed in the assessment of Chaco. After a series of emails arguing that Chaco was indeed the center of and central to a region, Dr. Smith revised his view in a gracious email with the subject line: “OK, you win. City.” (Michael Smith, personal communication, January 12, 2011). He was, however, entirely unconvinced by my subsequent suggestion that Chaco was an altepetl.


93. The words “city” and “urban” are all but absent from Whalen and Minnis 2001 and 2009; and Whalen et al. 2010. Whalen and Minnis (2001, 2009) were convinced that Casas Grandes was an “intermediate society,” and those don’t have cities. Not allowed.
94. Subsequent work by Crow Canyon Archaeological Center reduced that figure to a range from 850 to 1,360 people (Kuckelman 2003).

95. “Ceremonial center” was a mistake typical of its times. Lots of kivas do not indicate phenomenal religiosity. “Kivas” were, instead, indicative of homes and households. See note 8 in chapter 3.

96. Those canals are really important; most places in the world, canals running through a string of separate settlements demand central administration, to avoid water wars; Hohokam archaeologists have scoured the earth to find a comparable system that apparently does not have a Chief of Canals or a Bureau of Canals (Hunt et al. 2005).

97. Suggestions for marketplaces forming a truly regional economy are made almost apologetically; markets smack of cities and states (Abbott et al. 2007).

98. I unkindly called these “fungus” (rainfall) and “flushing” (stream flow) models; Lekson 2009a:143–150. Blame the climate: Benson and Berry 2009; Benson et al. 2009; Weiss and Bradley 2001, among many, many others—see for example reviews in Tainter 2008. Climate is king in Southwestern archaeology, of course, but there was more to life than variance around mean precipitation. Around the turn of the Millennium, a role for “social strife” rose among Four Corners archaeologists (reported by George Johnson in the New York Times, August 20, 1996). In part, to accommodate the apparent fact that people bailed out of the Four Corners long before the droughts hit. They started leaving during a time of good climate and bad vibes.

99. Linda Cordell and Maxine McBrinn, in the standard textbook: “This term is inappropriate for two reasons. First, it is offensive to the descendants of the people who once lived in these places.... Second, the term abandonment was used in ways that conflated a variety of processes that are very different from one another” (Cordell and McBrinn 2012:223).

I have no quarrel with reason #1. If Pueblos find “abandonment” offensive, it should go. Reason #2 tells us “it’s complicated” and of course it is, but the end result was the same—several dramatic depopulations—worth searching for regularities.

100. Resilience theory seems not so much a theory but rather a sophisticated observational language, like Annaliste history. (That’s not necessarily a bad thing.) An intriguing article by Charles Redman and Ann Kinzig (2003) suggests that resilience theory is pretty resilient: Whatever you throw at it, the terms can handle it. That seems, to me, more a (useful) set of terms rather than an actual theory about how the world works, not really capital-“T” Theory as I define that in chapter 6.

101. Cycles have a history within history, and it’s choppy. Historians rejected the cycles of Oswald Spengler’s (1922–1923) Decline of the West, published between the Wars, as ill-informed mysticism. Arnold Toynbee’s (1934–1961) Study of History—also cyclic—published after World War II, fared no better. Yet both were influential bestsellers: Themes of cyclic rises and falls fit their times.

102. Archaeologist K. R. Dark’s remarkable The Waves of Time (1998) has had more influence in Political Science than in its home discipline. Dark had this to say about cycles in Mesoamerica, for example:

It is interesting to note that Mesoamerica, unlike the “Old World” and China, does not exhibit what seems to be a cyclic pattern of growth and decline.... Consequently, as we can see, a successive sequence of states is evidenced in Mesoamerica. [But Mesoamerica was not] unified under a single polity, even if the Toltec and Aztec kingdoms were both expansive and hegemonic. (Dark 1998:210)
Dark’s “Waves of Time” end up with modern nation-states and links them back to their ancient precursors. A Political Scientist I used to pal around with was flabbergasted that I (and, I think, many other archaeologists) did not know Dark’s work. Dark is an archaeologist, after all.

Joyce Marcus’s (1998) “dynamic model” of Mesoamerican (and other) early states, proposes a cycle of “consolidation, expansion, and dissolution.” States began as a large territorial state—“consolidation” in Marcus’s words—which arcs through a dynamic history of militaristic “expansion,” eventually falling, followed by “dissolution” into many smaller secondary and successor states (Marcus 1998:60).

103. Cycles formed a minor but important theme in early Sociology, through the work of founding fathers Vilfredo Pareto and Pitirim Sorokin.

104. Nikolai Kondratieff was a Soviet economist who was influential in the West until he was eclipsed by Keynes, and in the East until he was shot by Stalin. Years later, after the collapse of the Soviet Union, his work was rehabilitated by the Russian Academy of Sciences: A cycle of sorts.

105. Khaldun’s career was even more eventful than Kondratieff’s, with intrigues and exiles and adventures that could fill a book—and have: Fromherz (2010).

106. Upham’s “adaptive diversity” got some play at the time, but is seldom cited today. David Stuart’s ideas surfaced first in Stuart and Gauthier 1981, recycled in Stuart’s (2014) Anasazi America: Seventeen Centuries on the Road from Center Place. That’s the second edition; it was first published in 2000. To my mind, sadly overlooked, and seldom cited in the current Chaco literature. Stuart, when recently (2014) asked, “Has your power and efficiency model caught on with other archaeologists as a research paradigm?” replied:

Not really, most archaeologists are still invested in very particularistic explanations focusing on material culture, architecture and regional dynamics. Though many are extraordinarily talented at such analyses, few have studied ecology, agronomy, demography and climate in enough detail to put their better regional studies into modern analytical perspectives. So, as wonderful to read as many archaeological works are, few practitioners are interested in the level of analysis I champion. It’s another story, though, among a number of general anthropologists/ethnologists (some of whom refer to themselves as specializing in “cultural energetics”) and those from other fields in the natural sciences. Thus, the first edition of Anasazi America was reviewed in the journal Science before it attracted much attention in archaeology.

See more at: http://newmexicomercury.com/blog/comments/five_questions_with_new_mexico_authors_david_e_stuart#sthash.PY410Mal.dpuf.

107. Turchin attributes the cyclic successes (the rises and upticks) of polities to a quality he calls asabiyah (following Ibn Khaldun): The cohesiveness and solidarity of a group, and hence its ability to succeed in collective actions. Asabiyah is a positive property of metaethnic identity (kind of like “imagined communities” in their original sense; see chapter 2), which often develops on frontiers:

It is hard to imagine how large groups arise and maintain themselves in a homogeneous environment populated by many small groups, with an ethnic distance separating each pair of groups of roughly the same order of magnitude. But what if there is a major ethnic boundary?…A small group near such a boundary will be confronted with very different others, dwarfing in their
“otherness” neighboring groups that are on the same side of the meta-ethnic line.... This should lead to enhanced alliance formation among groups on the same side of the boundary. (Turchin 2003:53)

Thus formed, political units (polities) thereafter follow formal, predictable, secular cycles. Turchin’s penchant for neologisms and borrowed argot—*asabiyah*?—make it possible to dismiss his work as pop–political science. But his ideas, logic, and data, to me seem sound.


109. Among the many places where Tainter’s *Collapse of Complex Societies* does not appear are Vivian and Hilpert’s (2012) *Chaco Handbook: An Encyclopedic Guide*; the many chapters in *The Archaeology of Chaco Canyon* (Lekson, ed. 2006; although it appears in Lekson 2009a); the several chapters in *Chaco Revisited* (Heitman and Plog 2015); as far as I can tell, in any of the slew of recent articles about Chacoan agriculture; and many other recent articles and chapters and theses. I am happy to report that “Tainter 1988” is listed in Joan Mathien’s Chaco Bibliography, on the online Chaco Research Archive http://www.chacoarchive.org/cra/chaco-resources/bibliography/#t.

110. Diamond obviously is not an archaeologist; he’s a scientist but hard to pin down, bit of a polymath. His website says: “Professor of geography...he began his scientific career in physiology and expanded into evolutionary biology and biogeography.” He’s in the National Academy of Sciences, he’s won the National Medal of Science, and a MacArthur genius grant—among other laurels. So he’s the real deal, not a popular science writer/journalist. Nothing wrong with popular sciences writers/journalists (some of my best friends are popular science writers), I’m just establishing Diamond’s credentials.

111. A few Anthropological criticisms painted Diamond’s arguments as racist. I think those criticisms were wrong and unhelpful.

112. In *Third Chimpanzee* (1992), Diamond explores archaeological cases and concludes,

> Archaeology is often viewed as a socially irrelevant academic discipline that becomes a prime target for budget cuts whenever money gets tight. In fact, archaeological research is one of the best bargains available to government planners. All over the world, we’re launching developments that have great potential for doing irreversible damage, and that are really just more powerful versions of ideas put into operation by past societies. We can’t afford the experiment of developing five countries in five different ways and seeing which four countries get ruined. Instead, it will cost us less in the long run if we hire archaeologists to find what happened the last time than if we go making the same mistakes again. (Diamond 1992:336)

113. My best (and only) contact inside the Beltway (a brother who, after a State Department career, became a senior VP at the Institute for Peace) thinks *Guns, Germs and Steel* was a fine—but not flawless—book; and he assures me that most of his policy-wonk pals shared that assessment.

114. Not everyone joined the lynch mob. George Cowgill, as always, exhibited wisdom:

> Jared Diamond (2005) has provoked very mixed reactions among archaeologists but the issues he raises must be addressed—especially the extent to
which a research emphasis on interactions between humans and physical environments neglects or oversimplifies relations of humans with one another. (Cowgill 2008:971)

For a range of views, some positive, see deMenocal et al. 2005 and Faulseit 2016. And for a thoughtful analysis, Fowles 2016b.


116. Times were tough all over. In the Mississippi Valley, everyone left Cahokia and created the famous “Vacant Quarter.” Archaeology in the eastern Woodlands must confront pre columbian collapse.

117. As with most things Di Peso, later analyses challenged his interpretation. David Phillips and Eduardo Gamboa (2015:166), on “the fall of Paquimé”— their analysis is mostly chronological and supports an end date of 1450. Gamboa suggests “internal violence and collapse, without an external cause.” See also Whalen and Minnis 2012, who contend that relic populations persisted after 1450 in the old Casas Grandes region; but still, the great city was gone.

118. Actually, he (or his fate) stood on Zeus’s golden scale. Achilles on one side, Hector on the other. Hector’s pan dipped toward Hades, where that hero shortly followed. The Homer of this quote, of course, is Bart’s father and not the Bard. And to anticipate the next line: The poet came not from Ionia, but from Texas.

119. The idea and ideal of the city-state — the polis — come from Aristotle’s Politics. The basic form—an autonomous and largely self-sufficient small state with a single city-center/capital—was not unique to or invented by the Greeks, whose poleis were preceded by city-states of Mesopotamia, and the more proximate Phoenicians, among others. Two modern, influential archaeological definitions of city-states:

First, Mogens Herman Hansen (2006:9), distilling many years of cross-cultural city-state research with his Polis Centre: “City formation and state formation go hand in hand; but the relations between them vary…. There is a set of examples…in which each city is the centre of a small state consisting of town plus hinterland and, looked at the other way round, each state is relatively small and has, typically, one single city as the centre of society. And that is what we call a city-state.”

Second, Deborah Nichols and Thomas Charlton (1997:1), addressing mainly Mesoamerican cases: “In general we understand city-states to be small, territorially based, politically independent state systems, characterized by a capital city or town, with an economically and socially adjacent hinterland. The whole unit, city plus hinterlands, is relatively self-sufficient economically and perceived as being ethnically distinct from other similar state systems. City-states frequently, but not inevitably, occur in groups of fairly evenly spaced units of approximately equal size…. These small polities and small states in the Basin [of Mexico] and elsewhere in Mesoamerica have been referred to variously as altepetl, señorios, cacicazgos, kingdoms, petty kingdoms, principalities, and city-states, terms often used interchangeably.”

120. I have found particularly useful: Griffeth and Thomas 1981; Hansen 2000, 2002, 2006; Hansen and Nielsen 2005; Nichols and Charlton 1997; Martines 1988; Parker 2004; Scott 2012; Smith 2008a; Trigger 2003:Chapter 6; Yoffee 1997. Martines 1988 was one of the sources I enjoyed reading when I was looking at northern Italian Renaissance cities for ideas about Chaco.

121. Their argument is a bit of bait-and-switch, insisting that “city-state” must equal Aristotle’s polis and then gigging people for using “city-state” to describe things that were
not exactly *polies*. No non-Greek city-state enthusiast I’ve read was suggesting their “city-states” were exactly Aristotle’s *polis*, which was a philosophical ideal. This seems a bit (but not entirely) circular:

A term which many participants would like to see phased out is “city-state.” This term came into widespread use as a kind of English synonym for the Greek *polis*. There are two problems with its use: (1) many Aegean specialists do not believe the *polis* was a state at all, and (2) many of the polities all over the world to which the term has been applied do not resemble the Greek *polis*. The *polis* has been defined as a democratic and self-sufficient polity in which the majority of towns and villages had a high degree of autonomy and very little economic control over their citizens. Almost no society to which this term has been applied in Mesoamerica (for example) fits this definition. (Marcus and Feinman 1998:8)

122. Trigger’s take: “City-states were relatively small polities, consisting of an urban core surrounded by farmland containing smaller units of settlement. In territorial states a ruler governed a larger region through a multilevel hierarchy of provincial and local administrators in a corresponding hierarchy of administrative centers” (Trigger 2003:92). The question is: Are they a distinct class or an evolutionary step? He favors the former, but guardedly.

123. Of course, with Yoffee, it’s…complicated:

The term *city-state* is inherently and intentionally flexible, allowing for, even expecting, important differences in major political and socioeconomic institutions, and it requires that variability be delineated and explained. In some cases, however, the variability itself is illusory since *narratives of uniqueness can also result from failures of cross-cultural imagination*. Use of the comparative method (which is mandated by the term *city-state*) is thus, paradoxically, the only way in which attributions of uniqueness can gain plausibility. (Yoffee 1997:263; emphasis added)

124. For example, Marcus (1998; in Feinman and Marcus 1998, “Peaks and Valleys”) basically argues that city-states were remnants of older, larger territorial states which fragmented. That might account for some city-states but not for Cahokia or Chaco; which were in their regions sui generis, more or less. Which is why Cahokia and Chaco should have seats at the State Dinner.

125. “How to Make a Polity (in the Central Mesa Verde Region),” a recent *American Antiquity* article by Stefani Crabtree, Kyle Bocinsky, Paul Hooper, Susan Ryan, and Tim Kohler (2017), lifts my spirits even more than the Notable Archaeologist’s “Yes, but it’s complicated…” This, my friends, is how to do it.

Chapter 5: An Appraisal of the Ancient Southwest

1. “Indian” is of course false and wrong, the first of many European errors. Columbus wanted the Subcontinent, he got a New World. It’s all marketing, so Columbus called them “Indians.” Roger Echo-Hawk (2010) makes a convincing argument against the brand. He insists he is Pawnee, not Indian. Navajos are not Hopis; Zuni is not Taos. Each tribe stands alone, each a sovereign nation. But modern “tribes”—today, sovereign nations—were, to varying degrees, colonial constructs. A score of separate Hopi villages, for example, became a unified “tribe” only when Washington drew a rectangle around all those villages and announced: You’re the Hopi Tribe now. Apache
and Navajo local groups were forced into unwanted aggregates for the convenience of American administrators. In Indian law, heritage, and to a very great extent anthropology we practice what Tom Sheridan (2005) calls ‘strategic essentialism’: We know tribes are constructs but we treat them as entities. Because they are.

2. I do not refer here to the deeper depths of archaeological theory; those are a problem too, reserved for chapter 6. By “Postmodernity” I mean the general erosion of reason in the latter part of the twentieth century which gave us constructivism in public education and undermined rationalism across a wide range of authorities.

3. A brief recapitulation: Beginning way back in the 1970s, for a half-dozen years I worked side by side with Indians (work = picks and shovels, wheelbarrows, stadia rods, etc.). Then for four or five years I collaborated with Indian artists, writers, intellectuals, and political leaders first at the Museum of Indian Arts and Culture (a thematic precursor to NMAI) and at Crow Canyon (recruiting, with Rich Wilshusen and other Crow Canyon staff, their initial Native American Advisory Board). Then several years teaching (and doing) Indian and Indigenous issues in a Museum Studies graduate program at a university museum. And finally, over six years, I consulted with 80+ tribes (mostly face to face, home and away) for NAGPRA. We got it done: The University of Colorado Museum of Natural History is one of the few big museums to have completed repatriation (and assist with reburial) of our entire holdings, 600+ human remains and associated funerary objects. Very much a team effort with Debbie Confer, Christie Cain, and Jan Bernstein. Those were the big-ticket items; along the way, scores of collaborations with individual Indians (tours, panels, symposia, exhibits, and so forth); a dozen appearances before NPS and other Native American advisory groups; on-site consultations with tribes at various excavations; and more than a few lasting friendships.

4. From Aristotle to Žižek, Bacon to (E. O.) Wilson, I’ve read widely, and without apology—as Joyce Carol Oates supposedly advised aspiring writers.

5. Europeans are easy with the idea of “heritage” as the past used socially and politically in the present (see chapters in Fairclough and others 2008), but many American archaeologists seem to think that heritage and history mean essentially the same thing—at least as far as Native American history/heritage. The National Park Service muddied these waters when, with NAGPRA hovering in the wings, they changed the names and aims of CRM to “heritage management”—a shift that took place, I think, throughout federal agencies. Park Service journals such as CRM: Journal of Heritage Stewardship (2003–2011) in parallel with the hopefully titled Common Ground (1994–2011) replaced the earlier, prosaic CRM Bulletin (1978–1990) and CRM (1991–2002). At least this is my impression: I decline to research the issue further for this book (this madness must stop somewhere). I mention it as a possibly fruitful topic for others: When did history become heritage in our national archaeological program? And why?


7. Heritage can also be a tool for control, can cut off discussion. Steven Conn (also following Lowenthal) notes that with heritage narratives “there is little room for the argument, debate and polyphony that is the very essence of history” (Conn 2010:48). Speaking of European situations, Geoffrey Scarre and Robin Coningham: ‘Accusations of ‘cultural appropriation’ and claims that others have taken what is rightfully ours are often employed more with the aim of guillotining reasonable debate than of advancing it. Once you identify something of your heritage, then anyone else’s
claims to or concern with it can be rejected as irrelevant and intrusive, a threat to your own rightful possession or even an assault on your identity” (Scarre and Coningham 2012:2). Although it’s hovering overhead, that exclusionary use of heritage has not been a major problem in the Southwest—so far.

8. Chaco—under US administration—began as a National Monument, “an object of historic or scientific interest”; graduated to National Historical Park “for the enjoyment of future generations”; and later was elevated to a World Heritage Site, for its “outstanding universal value” under Criterion iii: “A unique or at least exceptional testimony to a cultural tradition or to a civilization which is living or which has disappeared.”

9. The Natives are not shy about making claims. For example: Hopi heritage tells of the Red City, Palatkwapi, to the south, but accounts are vague on its actual location. With the CRM explosion in Hohokam, Palatkwapi was said to be in southern Arizona. And with expanding knowledge of Paquimé, that became a favored locus. “It is tempting to view the successively southern claims for Palatkwapi’s location as an ever-widening land claim fueled by Hopi access to archaeological accounts. This is undoubtedly the case, but there is more to it. The stories and their recent elaborations are plausible if only on a small scale, and should not be dismissed out of hand” (Hays-Gilpin 2008).

10. For lowercase-“h” “history” I considered awkward replacement terms like “actual-ancient-events,” or “history-as-it-happened” (versus “History-as-written”), but in the end, I could find no solution that wasn’t cumbersome and ugly. So for stylistic reasons I accept a degree of sloppiness. Mea culpa.

11. For example, Jeremy Sabloff’s (2008) Archaeology Matters; Randall McGuire’s (2008) Archaeology as Political Action; and Scott Ortman’s (2016b) “What Difference Does Archaeology Make?” among many others. Calls for “action archaeology” (Sabloff) or for “emancipatory praxis” (McGuire) or for archaeology to be more about the present “and less about the past” (Ortman) are, of course, all tied up with théorie (chapter 6) and Indigenous Archaeology, but they are broader and more programmatic in calling for archaeology as an agent or promoter of social change across a wide range of issues. I wonder: If you want to cure the world’s ills (a fine and proper thing to do!), is archaeology an efficient platform? It seems an odd pulpit from which to preach. If changing the world is your goal, would it not make more sense to work for an NGO or a political party? Which, indeed, some of my colleagues have done, God bless ‘em.

In pondering this matter, I considered revisionist historians: Their products might change society. I write revisionist pre-history and I’m not sure my work would have the desired effect. That could reflect the local politics of my region, the Southwest.

12. That slap upside the head got Anthropology’s attention and launched hand-wringing conference sessions and edited volumes such as Indians and Anthropologists: Vine Deloria Jr. and the Critique of Anthropology (Biolsi and Zimmerman 1997). It was at least partly responsible for the end of cultural anthropology/ethnology in the Native Southwest.

Joe Watkins (2000:3) dates the open, active, confrontational opposition of Indians to anthropology and archaeology, after many decades of “uneasy truce,” to around 1969—the year, he points out, Playboy magazine published excerpts from Deloria’s Custer Died for Your Sins. Deloria’s books were required reading for Indian intellectuals, but Playboy brought it to the masses—or at least to horny guys, of which there were many, some of whom apparently were policy-makers.

13. They quote cultural anthropologist Elizabeth Brandt: “Throughout the late 1960s and early 1970s most researchers of my generation were influenced by the concerns
and critiques of anthropology by Indian people,’ continues Brandt, citing Deloria’s work in particular. ‘There has been little ethnographic work since…. Many anthropologists discouraged with the prospects for research and publication in the Southwest in light of the new circumstances [that is, the new politics of representation] changed their emphases and began to work in other areas of the world’’ (Brandt 2002:116, 118). Fowles and Mills note: “There are notable exceptions…. [B]y and large, however, ethnographers packed their bags and left, effectively leaving archaeology as the sole face of anthropology in the Southwest” (Fowles and Mills 2017:41).

14. Remarkable exceptions of great importance to Chaco are recent 14C, DNA, and other sourcing studies carried out on human remains from Chaco and Aztec (e.g., Plog and Heitman 2010; Price et al. 2017), mainly using AMNH collections. These studies seem to be flying under the regulatory or tribal radars; they show how much archaeology is lost with the diminishment of bioarchaeology in the Southwest.

15. Indians ask: Who gave you the right to tell other peoples’ histories? If we are only concerned with local history or local heritage, that’s a very good question. But if we globalize matters, I have an answer: That would be our mutual African grand-grand-grand-grandmother, who encouraged her child’s inquisitiveness to know the world—an australopithecine with a bump of curiosity. Ever since, humans wanted to know about and to make sense of their world; different cultures handle that curiosity differently. And of course all cultures set limits: Curiosity can be bridled.

But maybe that’s not the right answer to give, or the right question to ask. I happen to make sense of my world through the natural, material, scientific notions of the European Enlightenment—and so does archaeology and archaeology’s main audiences. Maybe turn the question on its head? Here’s what Enlightenment thinkers asked of priests and bishops, nobles and kings: We want to know how our world works; who gave you the right to stop us? Priests and princes had powerful answers to that impertinence, and things sometimes got nasty; but thus began what we might euphemistically call the conversation between reason and religion—within Europe.

I do not suggest a parallel with Indians and archaeology in the New World; those relations are complicated by colonialism, conquest, and genocide. I suspect that Indians would find my answer about our australopithecine grandmothers unsatisfactory too, on several counts. But that’s my answer, to myself. I do archaeology because archaeology gives us an account of the human past we can use to understand how the world works. To anticipate chapter 6: Capital-“T” Theory, scientific theory. Globally, archaeology needs no apology; locally, it needs respectful diplomacy with tribes.

16. I’ve been caught in the middle (literally) of that argument. See, if you wish, the account in Lekson 2010a. At one meeting addressing these issues, a tribal representative angrily complained that NAGPRA was just another White law aimed at dividing Indians, setting tribe against tribe. I had never thought of NAGPRA that way...

17. Loris Ann Taylor (Hopi). It’s an excellent video, by the way, and she’s an eloquent voice in it, representing her views; the video includes several archaeological voices saying archaeological things.

18. It seems slightly ironic that Chaco today is managed for “wilderness values” when, in the eleventh century, Chaco was the opposite of wilderness. And until mid-twentieth century, Chaco was home to many Navajo families, not wilderness. Simon Schama (1995) reminds us that “wilderness values” have, like “heritage,” European backstories. I know and you know what we mean by “wilderness”: we value the loneliness and emptiness of Chaco, compared to our noisy, cluttered urban homelands. Archaeology tells us that Chaco was not wilderness, long ago; and that’s a strong demonstration
that archaeology is NOT heritage. Archaeology tells us what happened then, not what we want to happen now.

19. Hewett also reclaimed Chaco for New Mexico from New York’s American Museum of Natural History by foreclosing on AMNH’s operations at Pueblo Bonito. Now that’s a heritage claim!

20. Demonstrated, as I write, by the range of opponents to proposed fracking around Chaco: Historic preservationists, wilderness advocates, Dark Sky enthusiasts, archaeologists, tribes, and more. A sad subject we will revisit in chapter 7.

21. The Great Kiva of Casa Rinconada is today closed to the public because New Age shenanigans offended the Pueblos (and most everyone else). I was in Chaco at the time, and know most of the subsequent story, and it’s bizarre. Buy me a beer sometime and I’ll tell ya all ’bout it.

22. The aesthetics of ruins are seldom considered by archaeologists, but for a significant element of our constituency, those values are paramount. Lowenthal (1999) riffs on this, and the literature is large: classics like Rose Macaulay’s (1953) *Pleasure of Ruins* and J. B. Jackson’s (1980) *Necessity for Ruins*; more recently a dense academic analysis by philosopher Robert Ginsberg (2004); breezier art criticism in Brian Dillon’s (2014) *Ruin Lust*; and Classicist Christopher Woodward’s (2003) *In Ruins*. And of course Michael Shanks’s oeuvre. Ancient ruins in the Southwest are seldom managed for aesthetics, witness the remarkable tin roof over Casa Grande. That roof is a personal favorite; hard to be more hegemonic than that: Casa Grande wants to melt away, but we won’t let it.

23. As Kelley Hays-Gilpin points out: “Ultimately, ‘preserving’ Pueblo religion is the responsibility of the Pueblo communities and not of anthropologists who, at best, can offer assistance if invited” (Hays-Gilpin 2011:613).

24. There are of course non-Western historiographic traditions which are not heritage. A useful survey (written, of course, in the Western analytical tradition): George Iggers and Edward Wang’s (2008) *A Global History of Modern Historiography*.

25. Maybe not truth; that’s too much to ask. David Lowenthal, no fan of Post-modernity, notes that we painted ourselves into a post-truth era (long before Trump): “Historians still strive for unbiased consensual understanding…. But ‘truth’ in the old sense—a veridical account of the past based on consensually agreed evidence—has become passé” (Lowenthal 2015:14).

26. “Myth” is considered pejorative when applied to Native or Indigenous accounts, but it needn’t be: We honor myths as the foundation of Western literature and thought. Where would we be without Zeus and Athena, or the equally mythical Honest Broker and Scientific Certainty?

27. This essay was greatly expanded into a book, *A Forest of Time: American Indian Ways of History* (Nabokov 2002). Nabokov is not currently a name to conjure with in Indian Country, as discussed elsewhere in this chapter. But that does not diminish the value of his considerable scholarship.

28. Hunt had a checkered career (Nabokov 2015): He left (or was expelled from) Acoma and traveled as a demonstrator of American Indian culture—a “show Indian” with the circus. He was paid by the Bureau of American Ethnology to tell his version of the Acoma origin story, the edition originally published by Matthew Stirling in 1946.

29. According to one news account in *Indian Country Today*:

   Theresa Pasqual as director of Acoma’s Historic Preservation Office, said that the Pueblo has its own protocol of how information is passed down generationally and, “Once that information becomes widely available the Pueblo
loses that ability. The traditional religious leadership has always expressed that this information gives us the basis for who we are." (Jacobs 2016)

And another report, in the *Santa Fe Reporter*:

Brian Vallo, the director of the Indian Arts Research Center at the School for Advanced Research, talked about the unique set of stories that explain how each clan came to be part of Acoma. "Those stories are life guides: They are very sacred. Different clan groups and societies have their own version, so the stories differ as a result. So, you know...you don't share that information with anyone else. Even internally some things are secret until you reach a certain age." (Iberico Lozada 2016)

30. Barbara Mills, in her influential chapter on "Remembering While Forgetting," drives home the depth and reliability of Pueblo histories, while noting their selective remembrance and selective forgetting:

The memory of Chaco also resides in the historical narratives of contemporary Native Americans in the Southwest. As Leigh Kuwanwisiwma (2004) recounts, Chaco Canyon is the place called Yupköyvi to the Hopi. Not all clans trace their migration pathways from or through Chaco. Of those that do, there is a history of clan order and particularly memorable places where ceremonies were performed. This history, recounted over 800 years later, demonstrates the efficacy of Pueblo ways of remembering while forgetting, and how the Chacoan past became the present. (Mills 2008:107–108)

31. "Shaping a field that is fundamentally geared toward establishing more inclusive, democratic, and reciprocal relationships with descendant communities" (Colwell-Chanthaphonh and Ferguson 2008:3). Full collaboration requires "goals develop[ed] jointly," "full voice for [all] stakeholders," and "needs of all parties realized"—consensus which seems unlikely if we are after historical facts. Facts all come with points of view, as the poet said.

32. Very, very few archaeologists are Native/Indigenous. Dorothy Lippert (2008) said, in 2006, only about a dozen. Today, happily there are more, including excellent, articulate advocates such as Joe Watkins, Michael Wilcox, Joseph "Woody" Aguilar, and Sonya Atalay, among others. More young Native people are engaging in CRM archaeology, mostly through tribal heritage programs. All this is very good indeed; but, as explained below, I doubt that archaeology as pre-history is an Indigenous interest. Rather, archaeology can be an instrument of heritage. (Please be clear: heritage is not bad or inferior, but heritage is not pre-history.) I once heard a very smart, well-educated Indian announce that she would become an archaeologist and "subvert it from within." She said it with a smile, but...

33. See, for an example of collaborative history, Benally and Iverson 2005. But Miller and Riding In argue for a purely Indigenous History that privileges and upholds works by other Indigenous scholars, relegating non-Indigenous works to a secondary status. Some readers may be put off by that tactic, but it is common in the development of disciplinary literatures, and historians use it habitually, referring primarily to the works of other historians instead of otherwise relevant works by writers in other disciplines. (Miller and Riding In 2011:4)
34. I read broadly, and I chatted up a few of the central figures. To reassure Indigenous Archaeologists that I’ve done my homework: In addition to those cited, others from which my extracts and notes did not make the cut for this chapter include books and edited volumes such as Bruchac et al. 2010; Colwell-Chanthaphonh 2012; Colwell-Chanthaphonh and Ferguson 2008; Liebmann and Rizvi 2008; Lydon and Rizvi 2010; McGuire 2008; McNiven and Russell 2005; McNiven 2016; Nicholas 2010; Silliman 2008; Smith and Wobst 2005; and Stottman 2010, among others. I gave them a fair read because it seemed to me with so much smoke, there must be fire. And fire there is: Ardent and commendable enthusiasm for social and political programs. For slightly less polemical perspectives, I very much appreciate the thoughtful analyses of Joe Watkins (e.g., 2000, 2003), and Sonya Atalay (2006, 2008, 2012). I hope I haven’t damned them with my (sincere) praise.

35. Colwell and others (2010:Table 1) list eight salient but rather disparate characteristics, seven of which are unabashed social or political agendas, while the eighth is a general appeal to “theory.” See also Nicholas (2008:1660).

36. “Reversing scientific colonialism may well be the primary ethical challenge facing archaeology” (Nicholas and Hollowell 2007:62). Indigenous Archaeology is a movement: Madonna Moss correctly notes that in “feminist, Marxist, and postcolonial [Indigenous] archaeologies…their practitioners aspire to contribute to social change beyond the realm of archaeology itself” (Moss 2005:581). And, presented as moral imperatives, all but impervious to analytical criticism.

It’s actually dangerous to question Indigenous Archaeology. For example, George McGhee’s (2008) critical American Antiquity essay “Aboriginalism and the Problems of Indigenous Archaeology” provoked a flurry (almost a frenzy) of articles by ten authors—a who’s who of Indigenous Archaeology—in a later American Antiquity issue (Colwell-Chanthaphonh et al. 2010; Croes 2010; Silliman 2010; and Wilcox 2010). McGhee, in my opinion, made a reasonable case, both historically and logically; his critics countered largely with moral claims. Not, for me, persuasive; as McGhee (2010) replied: Strawmen, red herrings, and frustrated expectations.

While the substantive value of Indigenous Archaeology is not the central theme of this section, I may as well go on record: I agree with George McGhee (2008:239): “Indigenous archaeology is a social project without a demonstrated intellectual foundation.” By siding with McGhee I forfeit, of course, what little credibility I might have with the Committee on Archaeological Morality. But…McGhee was mostly right: Indigenous Archaeology requires the sort of essentialism we work hard to avoid. Much the same point was made, eloquently, by Roger Echo-Hawk (2010), who questioned as essentialist (and racial) the terms “Indian” and “Indigenous.” Echo-Hawk’s argument shook or at least stirred proponents of Indigenous Archaeology (see papers in SAA Archaeological Record vol. 10, no. 3 [2010]).

37. “Acceptance of alternative worldviews and histories as valid forms of meaning-making is one of the greatest challenges faced by archaeology and by sciences in general” (Nicholas and Hollowell 2007:63). Indeed. How would we do that, and still have archaeology? Not to mention science? Their claim presages problems of the Ontological Turn in archaeological theory, discussed in chapter 6.

Take, for example, Ian McNiven’s 2016 “Theoretical Challenges of Indigenous Archaeology.” He notes, correctly, that Indigenous Archaeology has been mostly a call for collaboration, with theory “minimally articulated beyond praxis” (McNiven 2016:27). He begins by “challenging ontological and epistemological divides and dualisms within mainstream Western archaeology.” (Nobody likes nasty dualisms!)
He notes two (but not dual!) theoretical agendas for his brand of Indigenous Archaeology: First, “challenge objectivist tangibility.” Second, “challenge secularist archaeologies of a detached past with archaeologies...linked to identity and diachronic explorations of ontology and spiritualism” (McNiven 2016:1). I’ll pass on both.

38. Wylie conflates Indigenous Archaeology with the “community archaeology” of her title, but her essay is all about Indigenous Archaeology (see also Atalay 2012).

In my original manuscript for this book, I riffed at length on the curious provenance of Wylie’s essay, which begins with Paul Boghossian’s (2006) seminal critique of NAGPRA, follows his thread back to George Johnson’s (1996) “Indian creationists,” relies on Larry Zimmerman and Roger Anyon quotes in Johnson’s article, to speak hopefully for Indigenous Archaeology—while ignoring Vine Deloria, Dorothy Lippert, Susan Harjo, and other Indians with very dark views on archaeology of any stripe. Wylie’s essay was thoughtful and logical, of course, but it concludes with two (to me, odd) defenses for replacing Western rationalism in archaeology: (1) archaeology might get new ideas from Indians; and (2) diversity is good. Thin justifications for rejecting the Enlightenment.

39. It’s hard to see how that’s so: Is Indigenous Archaeology relevant to Ur, or Lascaux, or Olduvai? For Caral? For Göbekli Tepe? No, probably not. Wylie asks: “How can an openness to exploring ‘a different kind of science’ enrich, rather than fatally compromise, a social science like archaeology?” (Wylie 2014:73). I’d say: It can’t; it will indeed fatally compromise archaeology, much less social science. In an article published long after Johnson’s New York Times article, Zimmerman states that comingleing Indian thinking and science is “not possible,” and offers this solution: “Ethnocritical archaeology, in which archaeologists and indigenous people share construction of the past” (Zimmerman 2001:178). Parallel narratives, already a workable solution. Zimmerman might not hold those views today; nothing wrong there, we all change our minds.

40. What do I mean by “heritage technicians”? It’s not necessarily a bad thing: I mean professionally trained archaeologists who work primarily in the service of tribal heritage. You could, if you wish, say “heritage professionals.” Kelley Hays-Gilpin asks: “Can methods and theories developed by archaeologists from outside the Pueblo world assist Pueblo people with writing detailed long-term histories, documenting land claims and use-rights to shrines, plant-gathering areas, and other sacred places, and establishing cultural affiliation for repatriation of human remains, funerary objects, and sacred objects in museums and private collections? Or must Pueblo scholars invent new ways to meet these goals?” (Hays-Gilpin 2011:613).

More pointedly, Chuck Gibbs in the online journal Sapiens: “Over the years, I have reached the conclusion that American archaeology has more to offer Native Americans than it does Euro-Americans. What was once an instrument of oppression now seems to me an instrument of self-determination.... The Hopi call their ancestral sites ‘footprints.’ Such places document Native American history more effectively than any textbook. I view it as my obligation to make these footprints more accessible and relevant to Native American students” (Riggs 2017:Sapiens online).

If that’s what some people want to do, OK by me. Just don’t insist that we all do it, or that it’s the only way to do archaeology. I see Indigenous Archaeology as local responses to local situations, local politics. But we should not claim that working in the service of tribes changes archaeology fundamentally as a discipline: Archaeology is still the study of what happened in history. Archaeology is history, not heritage.
41. Here’s what Zimmerman (2012:116) wants archaeology to let go; his quotes, abbreviated by me to bullet points, with my exegesis in brackets. Here’s what archaeology should lose:

1. Past as public heritage. [Tribal heritage overrules History.]
2. Independent agency. [All work must be collaborative.]
3. Academic freedom.
4. An idea that there is a single past…. [T]he past is multivocal and multi-threaded; positivist views that there is single, knowable past do not work for Indigenous archaeology.
5. Western views of science. [Like Vine Deloria 1995:15, “much of Western science must go.”]
6. Traditional field and laboratory methods.

That does not leave very much. Lose all that, and archaeology is no longer archaeology. Which may be what Zimmerman and other enthusiasts want, but I don’t. Nor should you. Why would American Anthropological Archaeology fold up shop when archaeologies everywhere else in the world press on with their business, the business of learning about the past?

42. Indian initiatives are present, to be sure! For example, Joe Watkins, a Native American archaeologist: “The development of a truly indigenous archaeology will never happen until indigenous populations control the quality and quantity of archaeology performed within their homelands” (Watkins 2000:177). That’s a basis for policy.


44. Pueblos of course have their own ideas of what’s proper and useful. Leigh Kuwanwisiwma (2004:43), of the Hopi Cultural Preservation Office, tells us how Hopi uses archaeological data: “Indeed, [Hopi] seriously considers scientific findings and extracts information that corroborates Hopi traditional knowledge or is credible in terms of that knowledge. Although this may seem overselective, Hopis are not surprised that scientific conclusions complement their knowledge and verify cultural continuity between themselves and cultures thousands of years old.” Whatever works.

45. These phrases come from several sources; for an overview, see Cajete 2016, or the Worldwide Indigenous Science Network at http://wisn.org/. Indigenous Science includes superb engineering, keenly observed natural history, and thick layerings of spirituality. Not saying that spirituality is a bad thing, just not something we need in archaeology.

46. The rest of the quote, attributed to Aldous Huxley: “What is science? Science is angling in the mud—angling for immortality and for anything else that may happen to turn up.”

47. We are lucky to have archaeologists who push archaeology into the marketplace of ideas: Books with broader impacts, beyond local interest. From the Southwest, Steven LeBlanc’s (1999; LeBlanc and Register 2004) books on ancient warfare are widely read. Anthropological archaeologists Kent Flannery and Joyce Marcus (2012) and classicist Ian Morris (2010, 2014, 2015) and Bryan Fagan (too many to list! http://www.brianfagan.com/) and more than a few others write thoughtful “general audience” books. Most recently, I was thrilled by Robert Kelly’s (2016) The Fifth Beginning: What Six Million Years of Human History Can Tell Us about Our Future. The content,
to be sure; but even more the concept, the bold use of American Anthropological Archaeology to address fundamental human questions. It’s great when an archaeologist writes a book like that, because not many of us can. I can’t.

We may disagree with what they say, but we should laud them to the skies for saying it. As Voltaire is said to have said, I may disapprove of what you say, but I will defend to the death your right to say it. (He didn’t say that; those words were put in his mouth by a friendly biographer.)

48. No individual could read all the output of CRM archaeology in the Southwest. Moreover, the distribution of CRM gray literature is often intentionally limited; you can’t read it even if you wanted to. Happily, the bigger projects are often published in multi-volume reports. Big CRM budgets can do archaeology beyond NSF’s wildest dreams; and CRM has the talent to use that money wisely.

49. And sometimes CRM writes for larger audiences, with great success. I’m thinking, in the Southwest, of Archaeology Southwest’s long-term commitment to public audiences, which the many titles in their *Archaeology Southwest Magazine* do exceedingly well. And several other CRM organizations do equally great work along these lines. SAR Press (not a CRM firm!) does outstanding work with its “popular archaeology” series, originally “curated” by David Grant Noble.

50. In chapter 1, I estimated about 11,000 archaeologists working in the USA. My guess of 1,500 in the Southwest, assembled from several informal sources, is almost certainly too low. More like 2,000? Who knows? In any event, there are a LOT of Southwestern archaeologists, one or more under every wet rock.

51. A political “constituency” elects its representative. We can be fairly certain that Indians would not elect archaeologists to represent their past. "Constituency" is imprecise, but more melodic than “stakeholder”—which sounds like Professor Van Helsing’s assistant.

52. One cause of the happy accident was SFI’s Nobel laureate physicist founder Murray Gell-Mann’s polymathematical interest in prehistory; later cemented by several years of an outstanding archaeologist, Jerry Sabloff, as SFI’s president. The Santa Fe Institute’s location in the City Different is another happy accident, of a sort: Its founding owed much to Los Alamos, over the river and through the woods. Most of SFI’s founders were nuclear Los Alamos physicists who wanted to do something more elevating than building bombs.

53. The Department of Anthropology at ASU reinvented itself as the School of Human Evolution and Social Change and proudly announced “ground-breaking collaborative social science”—and they made good on that promise, with cross-disciplinary projects escaping the confines of conventional Anthropology. I’ve listed a few of its many projects elsewhere in this volume. All engage other disciplines with notable results. ASU is not alone, of course, in exporting our products; but they may be the most prolific.

54. It’s great today to see many American Archaeologists working to reach the nonspecialist audiences. It wasn’t always so. When I got into this business about fifty years ago, archaeology shunned popular audiences and media. This was especially true in the academy, and remains true: Universities actively discourage “popular” writing for a professor’s first six or seven years—that is, before tenure; and those rules and habits become ingrained.

It took me a while to figure all this out. My first decade as an archaeologist, I learned my craft through a string of shovel-bum jobs across the Southwest, ending up with the National Park Service’s Chaco Project, which kept me busy for ten years. Our offices were at the University of New Mexico, where I got a close look at academic
life. With Lew Binford and several other colorful characters, it was not dull. When they got tired of beating up each other, UNM’s Department of Anthropology beat up NPS and public archaeology—despite UNM’s thriving CRM operation (in a separate building) and Maxwell Museum of Anthropology (right next door). (I’ll spare you the details.)

Halfway into my NPS stint, I realized that we nonacademic archaeologists weren’t doing much for the public, either. In NPS, that job was done by “interpreters.” But NPS (and other) “interpreters” were typically decades behind the times, archaeologically; and (this is important) they never, ever, ever ventured out of Pueblo Space—still a big problem. So in 1987 I wrote my first article for Archaeology magazine, and the next year an article in Scientific American, followed by more articles in Archaeology and New Mexico magazine and El Palacio and so forth. I was well aware that appearing in those outlets would compromise my scholarship, my academic integrity—and it did. (Again, I’ll spare you details.) But I wasn’t then (or now, really) interested in being a professor. It needed doing and no one else was doing it—with the single, notable exception of David E. Stuart. So I did it. And thereafter, throughout my career, I attended to nonspecialist audiences with trade books, exhibits, lectures (we all do lectures; I do a LOT of lectures). I worked in museums and I tried to run Crow Canyon. Public archaeology was my career, although along the way I managed a bit of research, too.

That was annoying because at heart I’m a research archaeologist; I did the public stuff because it needed to be done. A few years before the Millennium, I realized that the two spheres need not be separate. Indeed, as I argue in the text, research would be better if it answered the interests of the public—history—and not American Anthropological theory. I started writing “crossover” books, like Chaco Meridian (1999a, 2015a) and A History of the Ancient Southwest (2009a): Technical books intended for professional audiences but accessible to public readers willing to work a bit. Did that work? Yes and no: The public read those books and got something new, something outside Pueblo Space. But the professional audience for whom those books (and this book) were intended blew ‘em off—in part because they didn’t care for my conclusions, but also in part because they were accessible—public, popular. (Once again, I’ll spare you details.)

I hope this extended note established that I know about this stuff: I’ve done it and thought about it for decades. Please judge my opinions in that context; thanks. My knowing about this stuff does not mean that my opinions are correct, of course; only that my opinions are informed.

Chapter 6: A Future for the Ancient Southwest

1. For an interesting if ultimately wrong-headed alternative categorization: Gabriel Abend’s 2008 article on “The Meaning of ‘Theory’” in sociology.

2. Lewis Binford—I paraphrase a personal communication, from sometime in the 1980s. In print: “Theory is a causal argument about patterning in nature” (Binford 2001:243). In a footnote to this sentence, “It cannot be stated too often or with too much emphasis that it is not possible to see a cause. A cause is a relational statement developed through argument” (Binford 2001:482n2).

3. Some of my thinking could fall at various places on the critical theory spectrum. But I am only trying to clarify our knowledge of the past, not to advance a political agenda in the present. I have searched my soul and I do not see how identifying state-level societies in the ancient Southwest (the core of my argument) should affect current or future social and political situations—except perhaps to drive a tiny tack into Marxism's
well-nailed coffin, an act which is at this point truly and only academic. Whether this argument has any impacts on Indian people is, as far as I can see, entirely up to Indian people.

4. But not entirely! I originally planned to focus on Slavoj Žižek as my example of theoretical excess—"I am a Hegelian. If you have a good theory, forget about reality!" Žižek has been variously described as the Most Dangerous Philosopher in the West, the Elvis of Culture Theory, and the Ken Dowd of Post-Lacanian Hegelianism. A big target—but not exactly a familiar face at the Southwest Symposium. In the end, I chose as my exemple par excellence Laurent Olivier, a French archaeologist we will meet later in this chapter. The trouble, after all, was mostly French; the Brits did some damage, as did Austrian-born American Paul Feyerabend. And Slovenians (like Žižek), none at all, unless perhaps yrs trly.

5. Francois Cusset, professor of American Studies at École normale supérieure de Saint-Cloud, describes théorie in America thus:

   A deep mystery, skillfully maintained, surrounds the term ‘theory,’ this new transdisciplinary object fashioned by [American] literary scholars from French poststructuralism. This mystery distinguishes it, in any case, from the previous uses of the term, all more or less linked to science [including social science, e.g., Marxism]. Moving against the grain of these more precise definitions, the new theory of which it is everywhere a question for the last thirty years in [American] literature departments, whether it is designated as French or simply as literary, remains mysteriously intransitive, with no other object than its own enigma: It is above all a discourse on itself, and on the conditions of its production—and therefore on the university. For it had to remain itself, without object, since to aim at a more particular or transitive utility it would quickly lose ground in comparison to approaches that were less innovative but otherwise useful outside the campus. (Cusset 2008:99)

   We return to Cusset’s amusing book—written by a French intellectual for other French intellectuals, prochainement.

6. This, from a TAG paper, archived on Shanks’s webpage http://humanitieslab.stanford.edu/23/Home.

7. Severn Fowles correctly points out that the Southwest has been largely impervious to the wilder flights of théorie. He notes that things that look like théorie in the Southwest may have alternate histories. Contrasting our standard Processual-Plus and an emerging, more humanistic Southwestern practice hovering around Native American concerns (my words, not his), Fowles continues,

   Superficially, the contrast between these two intellectual moments may look like the old opposition between processual and post-processual archaeology.... By my reading, however, this characterization is quite incorrect, not only because there never was a British Invasion in the Southwest archaeological theory (which goes without saying in the Southwest but is news to some British colleagues) but because such a characterization obscures the profound influence of the Native American Graves Protection and Repatriation Act—and the new world of decolonization it stands for—on the source of American archaeology, particularly in the Southwest. (Fowles 2016a:178)

   I agree. But perhaps Fowles gives the Southwest too much/too little credit: Théorie may only now be reaching our region. In my experience, Southwestern ar-
chaeology was consistently a decade or so behind developments on our Coasts—which themselves were a decade or so behind developments on the Continent. (Most of “my experience” was pre-web, pre-social media; so these lags may no longer apply.) I review NSF dissertation improvement proposals and I of course work with graduate students at my university and elsewhere. It is (almost) charming to see the labored logics that invoke the philosophies of “old post-processualism.” It’s like the kids don’t really believe it, but they feel compelled to name the names. And as a museum pro and a relentless popularizer of Southwestern archaeology, I see the rot of epistemological relativism, science studies, and the whole baggage of théorie influencing—probably at third or fourth hand—constructivist and antiscience representations of our field, and what we have learned. (I sat on a committee advising a major Southwestern museum on a new archaeology exhibit; the convener opened the proceedings with “Now we all know that science is just another belief system…” and it went downhill from there.) And as a somewhat battered veteran of what is sometimes termed “the consultation community,” I see increasingly the language of théorie used to underwrite, philosophically, what is at heart human rights laws and regulations—which need no such justification.

8. For younger readers who may have arrived after the heyday of French Poststructuralism, I recommend Laurent Binet’s (2015) The Seventh Function of Language—a mystery/police procedural about the demise of Roland Barthes, run down by a laundry van on February 25, 1980 (this is a fact). An accident? Maybe not. Seventh Function—which features Foucault, Derrida, Deleuze, Althusser, Eco, and a great deal of literary theory—is not entirely respectful of Binet’s elders, but offers an easy, raunchy entrée into their odd world. It includes an epic dust-up at a (fictional) 1980 conference at Cornell with most of the major French linguistic philosophers of that era, the continentals slugging it out with the American analytics including John Searle, Noam Chomsky, and Jonathan Culler; in the end, Derrida is killed by a gigantic hound.

9. Théorie became an end to itself, and undeniably faddish. “Theory has become its own industry, merely trading an old canon for a new one, and retaining the same hierarchies and worshipful groupthink. There is little subversion to putting Judith Butler or Slavoj Žižek on a T-shirt, or to liking them on Facebook” (Balzer 2014:11). Some (how much?) théorie is simply careerist: “Postmodernism may not help us understand the past, but it was a wonderful career advancement tool for its first and loudest advocates” (Shott 2005:3). As was the strident scientism of some New Archaeology.

10. Cusset is intrigued but bemused at French théorie in America, because in the 1980s, “right when the works of Foucault, Deleuze, Lyotard, and Derrida were being put to work on American campuses…those very names were being demonized in France” (Cusset 2008:xviii). In the late 1980s, an SAA panel on “Theory in Archaeology” included one working French archaeologist. She listened to American panelists one-up each other with Foucaultisms and other French zingers, and observed thus: I am impressed that Americans are so comfortable with Foucault, because in France most of us have no idea what he’s talking about.

Bruno Latour agreed: “The French, having sold postmodernism to the whole world, are proud of never having partaken of it, a little like cynical pushers who would sell coke, but only drink Coke” (Latour 2007:16). He changes the subject: “The postmodern is an interesting symptom of transition, let’s accept it as such, use it to bring about the end of modernism more quickly, and, for goodness sake…let’s talk about something else” (Latour 2007:16–17).
In Britain, we are told, enthusiasm for French Poststructuralism peaked in the early 1990s and then waned ("the Death of Theory")—almost before it reached American shores (Pluciennik 2011:35).

11. In an essay titled "Pierre Bourdieu's Argument against Ordinary Language," Michael Billig recounts Pierre's demand that the language of social studies be ornate and obtuse. "If what literary theorists wrote was easy, then it would be common sense. And if it were common sense, then it would be conservative and nonintellectual. So, literary theory has to be difficult" (Billig 2013:90). I can't prevent the French from being French, lamented Charles de Gaulle; impenetrability comes with the territory. It's not limited to French Poststructural prose, of course: as Martin Heidegger (neither French nor Poststructural) is said to have said, "Making itself intelligible is suicide for philosophy."

12. Bruno Latour, one of the last men standing of French théorists of that early group, now laments his eager deconstructions which today underwrite agendas he finds distasteful. He didn't really mean it, he protests; of his partners in "science studies," he insists "several of them, at least, pride themselves on extending the scientific outlook to science itself" (Latour 1999:2, emphasis original). Several of that pack, perhaps; but one could question if their notion of "scientific outlook" conforms to something a real scientist might recognize. I find Latour’s apologia disingenuous; having overseen the havoc, he now distances himself from the wreckage. But the damage is done:

While we spent years trying to detect the real prejudices hidden behind the appearance of objective statements, do we now have to reveal the real objective and incontrovertible facts hidden behind the illusion of prejudices? And yet entire PhD programs are still running to make sure that good American kids are learning the hard way that facts are made up, that there is no such thing as natural, unmediated, unbiased access to truth, that we are always prisoners of language, that we always speak from a particular standpoint, and so on, while dangerous extremists are using the very same argument of social construction to destroy hard-won evidence that could save our lives. Was I wrong to participate in the invention of this field known as science studies? Is it enough to say that we did not really mean what we said? Why does it burn my tongue to say that global warming is a fact whether you like it or not? Why can’t I simply say that the argument is closed for good? (Latour 2004:227)

This is one of the least attractive aspects of théorie: Young Turks staked bold—even outré—claims; held them long enough to benefit professionally; and then backed away: We didn't really mean it, at least not to that extreme. I should note that I found equally distasteful the rhetorical excesses of the Young Scientists of (very old) New Archaeology.

13. Theory-of-the-month club? Would that be Speculative Realism, or the Ontological Turn, or New Materialism? Like Dorothy, head spinning in Munchkinland: “People come and go so quickly here!” New Materialism, for example, surfaced only a few years ago, but according to “New Materialist Cartographies” website there are already twenty-two distinct variations (who knew?), listed alphabetically from “activist materialism” to “vital materialism.”

Marshall Sahlins (2002:73–74), not one to suffer foolishness gladly, had this to say about the parade of paradigms (as he called them):

In the social sciences you couldn’t tell a paradigm from a fad.... In the social sciences, the pressure to shift from one theoretical regime to another...does not appear to follow the piling up of anomalies in the waning paradigm, as it
does in natural science.... There is an inflation effect in social science paradigms, which quickly cheapens them. The way that “power” explains everything from Vietnamese second person plural pronouns to Brazilian workers' architectural bricolage, African Christianity or Japanese Sumo wrestling.... Paradigms change in the social sciences because, their persuasiveness really being more political than empirical, they become commonplace universals. People get tired of them. They get bored.

14. “This heterogeneity means that archaeology has no center. No core. There is no single orthodoxy” (Witmore and Shanks 2013:383). “So how have we arrived at the current state of affairs?....Fragmentation is also a hyper-defensive strategy implemented in the wake of hypercritical aggression; the fragment is that which is most resistant to critique.... Critique is a healthy aspect of the discipline” (p. 385). VanPool and VanPool seem excited by “theoretical plurality” in their “Introduction: Method, Theory and the Essential Tension” (VanPool and VanPool 2003:1): “In recent years, archaeological theory appears to have fragmented into ‘a thousand archaeologies’ (Schiffer 1988:479).... Needless to say, this is an exciting time for archaeology as a discipline.”

Is it? Not all of us are charmed by fragmentation, nor do we celebrate its perverse diversity. John Bintliff is one among many annoyed, and I feel his pain:

By constantly changing the goalposts, the list of required sacred texts, theory teachers have led young scholars to feel intellectually inadequate, since hardly have they scoured the pages of Lévi-Strauss so as to parrot Structuralism, then they are told this is dropped in favor of Giddens’ Agency theory, and so on. Keeping up with cultural fashion, rather than bringing students to self-evaluation of intellectual approaches places power in the hands of teachers. We have found an increasing trend in classes, for students to repeat pages of leading theory texts as factual accounts of the world.... If one challenges students or young researchers to justify why a particular concept or approach has been taken, it is generally the case that the answer is merely that “a leading authority wrote this.” Citation of sacred texts becomes more and more the only authority needed to prove a case-study, rather than matching several alternative models to the data. (Bintliff 2011:8)

15. A recent one, Harris and Cipolla 2017 (which I read, cover to cover), is advertised thus: “Written in a way to maximize its accessibility, in direct contrast to many of the sources on which it draws, Archaeological Theory in the New Millennium is an essential guide to cutting-edge theory.” Not just a vade mecum but a (very necessary) précis/exegesis. Among many theory guides out there, Gibbon 2015 is level-headed and refreshingly undertheorized.

16. Older théorie almost always ultimately references nineteenth- and twentieth-century European philosophy. If one is trying to understand ancient societies, modern European philosophy seems like an odd place to shop. But still we turn to the consolations of philosophy. There are a range of philosophical choices—to be facetious—and by our choices we shall know us. I cringe when I see appeals to Heidegger; he was not a very nice man. It’s hard to take seriously arguments that reference as seminal thinkers the likes of mad Aby Warburg (Olivier 2011; whom we will meet shortly) or H. P. Lovecraft (Harman 2012; whom we will not meet). I read Lovecraft when I was a kid; then I grew up.

17. Ian Hodder: “I think there has been a shift in what the goal or the object has been. The goal of archaeology used to be the study of the past through material remains, but I
think it has shifted or, rather, ought to shift, to be the process of studying the relationships between people and their material pasts [in the present]” (Hodder 2013:130). For a chorus of well-theorized assent, see Olsen et al. (2012).

Archaeology is not just in the present, but active in the present: Lynn Meskell, describing “cosmopolitan archaeology” (théorie-of-the-month, a few months back): “Cosmopolitanism describes a wide variety of important positions in moral and sociopolitical philosophy brought together by the belief that we are all citizens of the world who have responsibilities to others, regardless of political affiliation” (Meskell 2009:1). See also Stottman 2010 and McGuire 2008.

18. The imposition of modern, Western philosophical discourses—and even those that reject Western ontologies are framed and argued in Western logics—upon the ancients makes perhaps even less sense than up-streaming Classic Ethnographies onto the denizens of Chaco and Mesa Verde. What would the people of Mesa Verde make of Latour or DeLanda? Probably what modern Indians made of Marx: Just another Old World shell game, foreign and irrelevant (chapters in Churchill 1983).


20. Friendly colleagues closely engaged with théorie warn me that these generalizations may not represent the Ontological Turn of its core thinkers—while admitting that these generalizations of the Ontological Turn do reflect the views of graduate students and professors who engage only the secondary literature. But the problem is, grad students and their professors are the ones who apply these notions and, perhaps, make policies. What Viveiros de Castro or Latour think may be less important than the Ontological Turn as applied in American Anthropological Archaeology. As we saw in chapter 2, imported concepts mysteriously morph in Pueblo Space. In théorie as in Wonderland, words mean just what you choose them to mean—neither more nor less (attrib. to Humpty Dumpty).

21. One wonders, other than academic gamesmanship, why we need yet another word for culture—first “habitus,” now “ontology.” A question which has of course been asked: “Debate: Ontology is Just Another Word for Culture” (Venkatesan 2010). “At the end of the discussion, the audience voted on the motion. The motion ‘Ontology is just another word for culture’ lost with 19 votes in favor, 39 against…. Had the word ‘just’ not been part of the motion, the outcome may well have been very different” (Venkatesan 2010:199). And thus is knowledge made.

22. I’m not starting at shadows: Two enthusiasts of the Ontological Turn question backward projections of the ontology of the moment: “New animism [for example] seems to unintentionally replicate some of the representational patterns that Said critiqued: taking one instance or moment of radical alterity and embellishing it to create an Other world envisioned by a western academic. It seems like Orientalism in a new guise” (Harris and Cipolla 2017:203). They continue, questioning the seeming universal enthusiasm for animism: “There is no reason to think that animism, per se, has any role to play in the pasts I investigate [in Europe]. It has already proved popular to apply these ideas there, and I find this a little problematic as rather than making the ontologies of the past more varied, it can reduce their complexity” (p. 205).

Remember when everything was shamanism? Even Maya kings? It’s like that.

23. There are exceptions, e.g., Ortman 2012.

24. Attempts to link modern languages to ancient Chaco are as difficult—and probably futile—as assigning “cultural affiliation” to thousand-years-gone ancient societies. But we do it, because we must.
25. I agree with Severin Fowles:

I will be frank and admit, first, that as an archaeological anthropologist I am not interested in speculating about multiple realities; I am happy to leave this to physicists and philosophers.... As I see it, the problem with going further and adopting ontological pluralization as an anthropological methodology is that this move ends up being so ironically, tragically, and embarrassingly modern.... There is nothing more profoundly modern than the effort to step outside modernity. (Alberti et al. 2011:906–907)

26. Hodder 2003a. Hodder does not seem to run with the newly turned Ontologists; but flirtations with epistemic relativism in his and other foundational Postprocessual théorie sowed the fields from which we reap this harvest.

27. Do I exaggerate? No. Recall from chapter 5 that of Anthropology’s fantastic four sub-disciplines, three have been eclipsed in the Native Southwest. Conventional ethnography and historical linguistics are essentially gone, and bioarchaeology is on the run. Their parent subdisciplines have been largely transformed into applied anthropologies. Archaeology, too: Most of our archaeology is applied CRM. There’s nothing wrong with Applied Anthropologies, but I’d hate to see conventional Southwestern archaeology diminish just when it might escape a century’s confinement in Pueblo Space, and actually figure a few things out.

28. “We begin to witness archaeologies built within indigenous ontologies or between indigenous and western worlds.... The general idea is that we are trying to move beyond the limitations of a western perspective that sees one world best explained through scientific inquiry” (Harris and Cipolla 2017:180, 181). “This is the crucial move from one world to two or more worlds. It is a move that takes western science off its pedestal and asks: what am I (the western observer) missing?” (p. 184). “Several scholars have raised concerns about the turn to these kinds of alternative or multiple ontologies.... Nonetheless, these ontological critiques are having a noticeable impact on many disciplines including archaeology, and offer very provocative tools to think with. As Bruno Latour has asked, are these kinds of approaches simply another way of looking at the world, or something that explodes our assumptions?” (p. 185).

29. A reviewer of this book manuscript suggested that, instead of Olivier, I consider Ian Hodder’s (2012) Entangled, a book far more likely to be read by Southwesternists. (I’ve read it several times.) There are many good bits in Entangled and far less pointless théorie. And there’s that meet-em-in-the-elevator thing.

Hodder’s book leads up to what he calls “tanglegrams” which on first blush look remarkably like flowcharts or even (gasp) systems diagrams (a point noted by other readers; e.g., Mills 2013). The fact that the relationship between human and material is, to varying degrees, reciprocal and “entangled” has been an anthropological chestnut since the hoary days of Man the Tool-Maker (Oakley 1946). Not just tools; a prominent midcentury British philosopher opined, “We shape our buildings; thereafter they shape us.” Hodder maps intriguing entanglements between and among rooms at Çatalhöyük—features, objects, and so forth. A larger entanglement would surely be the architecture itself: What sort of society built that bizarre beehive, and how did its densely packed modularity shape the next generations? (Hodder deals with these questions elsewhere; e.g., Hodder 2006.)

My marginalia in Entangled includes this question: “Re-write this [book] without the word ‘archaeology’?” Except for several specifically archaeological examples—which could easily be replaced by nonarchaeological examples—I think the answer would be “yes.” Is that a weakness or a strength?
30. Originally published as *Le Sombre abîme du Temps: Mémoire et archéologie*, and reportedly written at the urging of Sander van der Leeuw, who provided a back-cover blurb along with Michael Shanks. Not to be confused with Paolo Rossi’s 1984 *The Dark Abyss of Time*, originally published as “Segni del Tempo” in 1979 (Feltrinelli Editore)—a pioneering “Big History,” Olivier is not a flash in the pan; see also his chapter on “Time” in the ponderous *Oxford Handbook of the Archaeology of the Contemporary World*, edited by Paul Graves-Brown and Rodney Harrison (Oxford University Press, 2013).

31. If Warburg’s name seems familiar, it might be from his pilgrimage in 1895 to Hopi, to see the “snake dance.” He never saw the snake dance but he wrote about it, and published his photos of Hopi kachinas (Warburg 1939, reprinted as Warburg 1995). Google “Warburg image” and you’ll find a photo of Warburg wearing a hemis kachina mask. Ouch.

Darwin’s influence on Freud is well known—the key difference being that Darwin’s ideas worked, Freud’s did not. (Freud’s failures, strangely, do not disqualify him from théorie.) Walter Benjamin seems an odd third alongside two architects of modernity—typically, one would draw Marx for three-of-a-kind. Benjamin, of course, was from a later generation. Loosely associated with the Frankfurt school, his eclectic writings on literature and art—today associated with a resurgence of Romanticism—made him something of a cult figure, after his tragic (and Romantic) death fleeing the Nazis. Benjamin’s cult is more than matched by Aby Warburg, a German art historian of roughly Benjamin’s generation, who left Germany ahead of the Nazis but died before the War. Warburg too has achieved cult status, with institutes and journals devoted to his eclectic and (mildly put) idiosyncratic thought. Benjamin and—more so—Warburg are central to Olivier’s argument, to his critique of archaeology.

32. Quo vadis history? In the end, Olivier wants it both ways—and perhaps that makes sense to him. After a book spent decrying historicism and historical senses of time, he insists:

This in no way means that we must abandon the basic historicist perspective of archaeology whose aim is to reproduce the material reality of the past.... The point then is not to reject history, but rather to assign it to its true place in archaeology by making the discipline the “science of the past” that it has always sought to be. This is a theoretical matter; archaeology must define its objective: not the past, but what one might call that which is subject to the past. Whereas history seeks to establish what happened to people, archaeology explains what happened to things.... Archaeology studies how things and beings have “absorbed” past events, both how they came to evolve on the basis of their situation and their inherited past, and how they contributed to these evolutionary processes. (Olivier 2011:189)

33. Olivier chides: “We have never been able to free ourselves from archaeology as culture history, principally because one of the basic hypotheses of the discipline holds that the past that lies outside of recorded history is historically knowable” (Olivier 2011:182–183). His “culture history” is not the American culture history; and his archaeology—for which, prehistory is unknowable—is not American Anthropological Archaeology.

34. The quote continues: “It is a concept which puts archaeology in harmony with its subject, which is to say the material archives of memory of the past, by redirecting the discipline’s focus to the study of filiations” (Olivier 2011:xvii).
What are “filiations” and how do we find them? These questions are not explicitly addressed (this is, after all, French théorie); but here Warburg seems relevant. Olivier laments that “we no longer know how to deal with artifacts and ways of organizing them that once seemed so obvious to us; we no longer know what grid to place them in.” Warburg had grids—and they were brilliantly aesthetic or completely bonkers or both. Warburg was nuts—not an opinion, a real (and very sad) diagnosis of mental illness; a fact that Olivier acknowledges (Olivier 2011:149). Warburg obsessively collected and collated art images according to his own peculiar methods, creating a gridded *Mnemosyne Atlas*—a catalog of filiations?—“schizophrenic collages.” Olivier admires the *Atlas*: there is madness in Warburg’s method, and apparently that’s attractive: “The temptation has been great to integrate Warburg’s reflections into the thoroughly traditional approach of the Academy where, normalized at last, they could take their place. Warburg’s ideas have to be seen for what they are: pathological and delirious, to be sure, but extraordinarily perspicacious and fecund” (Olivier 2011:152). Madness, it seems, is the path to filiation.

35. I wish I’d coined that one, but it was Marshall Sahlins in an essay titled “Know Thyself”: “There is a certain species of academic whiffle bird that is known to fly in ever-decreasing hermeneutic circles until…” (that’s the end of his sentence; Sahlins 2002:76). In the fifth edition of this small book, he completes the sentence, indelicately: “…fly in ever-decreasing hermeneutic circles until it flies up its own backside” (Sahlins 2018:67).

36. This, from a recent edition of the tour leaflet at Mesa Verde’s Far View site:

> Archeologists Have As Many Questions As Answers Based On The Results Of Excavations. From Your Visit To Mesa Verde National Park, We Hope You Will Join With Us In Speculating About The Everyday Life Of The Ancient Puebloans. How Were Their Lives Like Ours? How Were They Different From Ours? Each Year, New Archeological Research Provides Us With More Answers And New Questions.

And this, from the NPS brochure for Tusayan Ruin (http://www.nps.gov/grca/planyourvisit/upload/Tusayan.pdf):

> As you walk around the ruins, remember that the history of these people and their culture exists only through the artifacts found at this and similar sites and through the stories of their descendants. You will notice that many statements in the brochure and on the signs begin with “perhaps,” “it seems” or “maybe.” There are few definitive answers.

Those refrains float like flute music over every Southwestern site: mysteries, questions, speculations.

37. Southwestern archaeology could institute a “swear jar” convention: Anytime an archaeologist says “I’m only guessing” or any variation of that phrase, it’s five bucks in the jar. The funds could be used for deprogramming us from Pueblo Space, and reprogramming us in positive thinking.

38. If this were easy, everyone could do it—a conundrum for Community Archaeology. We say we want everyone to do it, but we do not REALLY want everyone to do it. Archaeology is specialized work, requiring training and experience. It’s not the same thing, but would we want Community Brain Surgery?

39. I can offer no hard proof, but anecdotally it seems that many (most?) of my colleagues who use “just-so stories” dismissively are unaware of their august provenance
in Kipling’s fables. I suspect those same scholars, if made aware of the first “just-so” author, would be equally dismissive of the old White male imperialist who told us how the camel got his hump and the beginning of the armadillo. This, despite the fact that Kipling’s “just-so stories” directly addressed items of real archaeological interest: The origins of domestication and writing, for example. Incorrectly, of course, but amusingly.

40. And to these two articles, I could add a recent and important book, Michael Schiffer’s (2017) *Archaeology’s Footprints in the Modern World*. Not a compilation of what archaeology needs to do, like “Grand Challenges” and “Key Issues,” but a brilliant listing of what archaeology already does for the wider world. He addresses fourteen themes, each with three examples, for a total of forty-two good things about archaeology. Last and presumably not least, his final theme is history: “Revealing our prehistoric past” (p. 281) with three examples “In the Beginning,” “From Foragers to Farmers,” and “The Urban Revolution.” The thirty-nine other themes range from “debunking myths” to cultural tourism to supporting environmental sciences, and so on. It’s an exhaustive list, and slightly exhausting: I was reminded, faintly, of lists of “practical applications” issued by NASA to support funding the space program: Tang, Corn- ingware, freeze-dried ice cream, cordless portable vacuums, and so on. But Schiffer’s book is a good, hefty exhibit to present when asked: Archaeology, what is it good for?

41. In “Grand Challenges,” process trumps history; history did not constitute a Grand Challenge:

The 25 grand challenges presented here focus on cultural processes and the operation of coupled human and natural systems—not on particular events of the past. While this will not surprise archaeologists, to a nonspecialist there is a notable lack of concern with the earliest, the largest, and the otherwise unique. This focus on the dynamics of culture indicates no lack of regard for prehistory; the facts of the past provide the evidence that is essential for us to confront all of the problems presented here. (Kintigh et al. 2014b:7)

How is it that we know those “facts of the past”? They are more than a chronological list of events; there are causes and consequences. We need history: The “facts of the past” are events linked through history. It should be noted, in fairness, that “Grand Challenges” was commissioned by the National Science Foundation. NSF does not often fund history; that would be NEH.

42. An incredulity to pre-historiography; but the concluding thoughts on “Key Issues” are commendably broad and well worth quoting:

If we were to identify an area of concern, we would stress the need for archaeologists working in the SW/NW region to remain engaged with our colleagues working in other areas of the world, to participate in broader discussions of alternative theoretical perspectives, and to continue the development of new research methods. The literature on the SW/NW has become vast and the discipline as a whole has become more specialized, both trends that can have the unfortunate impact of encouraging us to more narrowly focus on our own specific geographical areas and to attempt to understand those areas from limited theoretical perspectives. (Plog et al. 2015:19)

43. Epic fail, in my opinion. I am not the only one to notice, of course. For example, Randall McGuire in a notable (1994) “position paper” on “Historical Processes and Southwestern Prehistory.” Having correctly diagnosed the problem, McGuire saw...
three kinds of history in anthropology: (1) cultural sources, “Ralph Linton at his breakfast table” analyzing the diffuse origins of bacon, eggs, and coffee; (2) cultural difference, Geertz’s notion of historical change as “continuous cultural process with few, if any, sharp breaks”; and (3) material social process. McGuire (1994:200) also noted that “historical processes of change” were present in pre-historic as well as historic times. And he concludes nobly:

Developmental change [evolution] can only be understood in the context of real historical sequences where nondevelopmental change, digression, and diversity are as important to the understanding of change as the regularities and abstractions that evolutionary studies seek.... History is made by the actions of real people not abstractions. People are not free to make history any way they want. Their actions are conditioned by...circumstances of their existence, even as their actions transform these circumstances. (McGuire 1994:200)

In that same volume, Norman Yoffee (1994:341–342), considering the sad state of historicity in Southwestern archaeology, suggested back in the early 1990s (using that era’s trope of the negative), “Perhaps archaeologists, increasingly self-conscious about how they know the past, will soon be teaching courses on ‘prehistoriography.’ (Not.)”

Alas, as Fowles and Mills (2017) note: “The SFI volume [which included McGuire’s position paper and Yoffee’s bon mots] had limited impact at the time...but more recent work has revived the debate.” (I was at that conference!)

See also Alice Kehoe’s extended critique of ahistoricity in American Anthropological Archaeology, Land of Prehistory (Kehoe 1998).

History turns some people off. Chip Wills complains: “It is common today for archaeologists to describe inferences about the meaning of their data as ‘history’ rather than hypothetical reconstructions, a difference that often encourages elegant storytelling about how people got from one temporal point to another in the past, but frequently lacks the historiography that historians utilize to evaluate the knowledge claims made in such narratives” (Wills 2009:284–285). Beyond the remarkable claim that history is “common” in American Archaeology, I agree absolutely that we lack a developed pre-historiography. That should be Job One, and it probably won’t involve hypothetical reconstructions.

44. “How do we ‘do’ history for ancient times?” — I put this question to most of the archaeology faculty at one of our leading universities, very kindly assembled for a lecture I was giving. One of their senior scholars—a wise and thoughtful archaeologist!—replied: We make it up as we go along. Fair enough: That’s what I did writing A History of the Ancient Southwest, made up methods on the run. It was an experiment, and the outcome was some ideas about pre-historiography.

45. This is very close to the argument of Timothy Pauketat (2001) for “Historical Pro cessualism”—an earlier effort that is curiously uncited by Beck and colleagues (2007). It is cited in passing by Susan Gillespie in her comments. Eventful archaeology has made an impact, which is great; see, for example, Gilmore and O’Donoughue 2015. Events are history!

46. The full quote, from http://peterturchin.com/cliodynamics/:

Cliodynamics is the new transdisciplinary area of research at the intersection of historical macrosociology, economic history/cliometrics, mathematical modeling of long-term social processes, and the construction and analysis
of historical databases. Mathematical approaches—modeling historical processes with differential equations or agent-based simulations; sophisticated statistical approaches to data analysis—are a key ingredient in the cliodynamic research program. But ultimately the aim is to discover general principles that explain the functioning and dynamics of actual historical societies.


48. Michael Shanks’s “An Archaeological Narratology” (in *The Archaeological Imagination;* Shanks 2012:127–144) is humanist théorie actually taking on pre-historiography. (I do not count Michel de Certeau’s 1988 *The Writing of History* and others of its ilk—it’s *prehistory* I’m after.) Like most of Shanks’s stuff, fun to read but of little practical application. He waves at historiographers he finds congenial and offers us a series of diagrams with titles like “Phantasmatic reality,” “The Semiotic Square,” “The voice against the wind,” and so forth. He concludes: “There is no definite end to these diagrams” (p. 144), which I read two ways: No termination or no purpose. “This is another way of pointing out that the archaeological, or antiquarian, imagination is far wider than what is now the discipline of archaeology” (p. 144). He likes that vagueness; I don’t.

More usefully: Ian Morris (2000:310): “There is no single, all-best structure for an archaeological narrative. Shanks (1992) and Tilly (1992) suggest that archaeologists might experiment with nonlinear narratives.... But I defended the value of chronologically tight, sequential stories.... I also emphasized the humanistic basis of cultural history, and the centrality of the event as an analytical category.... The principles involved—of how to weave a rich and compelling narrative from the material record—are much the same whether we are looking at Çatalhöyük or Annapolis.”

The range of approaches discussed in Van Dyke and Bernbeck (2015) *Subjects and Narratives in Archaeology* encompass a variety of media and genres—creative nonfiction, fiction, theater, docudrama, and more—but strangely (to me) not straight-up historical narrative. If we can do all that other stuff, surely we can do simple straight-up history.

49. Pauketat’s (2013) *An Archaeology of the Cosmos: Rethinking Agency and Religion in Ancient America* is a brilliant effort and stands alongside Fowles’s (2013) *Archaeology of Doings: Secularism and the Study of Pueblo Religion* as two of the most successful recent studies of ancient religion. Impressive energy and prodigious intellect went into researching some of the least knowable aspects of the ancient past. With good results: They are both great efforts and good reads. If we can do that—know religion and cosmology—surely we can do narrative history.

50. A southwestern application appears in the introduction to an edited volume, *Big Histories, Human Lives* (Robb and Pauketat 2013), which “combine[s] interpretive strands to create a multiscalar history...[bringing] together the ideas of history as multiple genealogies of practice and the vision of history as a palimpsest of qualitatively different processes” (Robb and Pauketat 2013:26). For the Southwest, they offer a complex diagram (their Figure 1.1) illustrating “history as multilayered processes.” Time runs up the Y-axis, ranging from “a few centuries” to “millennium”; parallel ribbons of multilayered processes are punctuated or delimited by areas of “historical ontology,” “ways of doing things,” “historical landscapes,” “local political histories and cycles,” and “tipping points.” It’s interesting, but it’s not narrative. But surely it is
underwritten by silent narratives—silent in that they are (in this presentation) unexamined. Where do the stories woven into the Y-axis come from?

51. But not, of course, all. Pauketat’s (2007) *Chiefdoms and Other Archaeological Delusions* cleared the way for his historical approach in the Southeast by slashing and burning through American Anthropological Archaeology’s dead wood and detritus. Pauketat’s book so roused his readership that a sixty-plus-page “book review forum” was published in *Native South* (Volume 2, 2009), a series of critical essays (not all negative!) by leading Southeasternists, with a response from Pauketat. And another extended discussion in *Social Evolution and History* 9(1), 2010. In his response to his critics in *Native South*, Tim writes,

My goals involved giving the establishment a slap in the face, not out of malice but because of the clear and present danger to our archaeological heritage.... The stakes, I submit, are high enough to demand a new approach. Why do archaeology at all if we simply recycle old ideas, over and over again? Time to wake up. Time to do things a little differently. (Pauketat 2009b:127–128)

52. According to Sassaman:

To rewrite Archaic “prehistory” as history is to write about Archaic experiences with migration, encounter, ethnogenesis, coalescence, and fissioning. I believe most Archaic specialists would acknowledge that these sorts of events and processes truly mattered, but I am afraid that few would privilege such factors over the constraints and limitations of “nature.” (Sassaman 2010:xvii)

53. It might be objected that my arguments are well and good for the densely dated and data-ed later Southwest, but they cannot apply to earlier hunter-gatherer situations. Sassaman shows that is not so: The Eastern Archaic, once freed from “the conceptual straightjacket which is ‘prehistory,’” (his words) requires a historic approach.

54. Attempts to inject *Annaliste* notions into American Archaeology “have not resulted in major advances in understanding time, scale and change in archaeology” (Robb and Pauketat 2013:12).

55. “The role of narrative in explanation has received considerable attention in most of the subdisciplines concerned with questions of historical process.... Archaeologists, however, have been curiously reluctant to consider the proposition that their reconstructions of the past are fundamentally narrative in character” (Ballard 2003:135).

56. Like everything accessible to théorie, “narrative” has been complicated and even made subject of a subdiscipline with its own journals, etc. See, for example, Herman 2007a, 2007b, 2012.

57. “In the past fifteen years, as the ‘narrative turn in the humanities’ gave way to the narrative turn everywhere (politics, science studies, law, medicine, and last, but not least, cognitive science), few words have enjoyed so much use and suffered so much abuse as narrative and its partial synonym, story” (Marie-Laure Ryan 2007:22).

58. Alex Callinicos (1995) offers a useful analysis of historical narrative in “History as Narrative” (his chapter 2)—in opposition to “History as Theory” (his chapter 3).

59. “Archaeological explanations evidently work, insofar as the discipline is able to proceed with shared understandings of plausibility, but it is not clear that archaeologists necessarily understand how or why their own explanations succeed in convincing their colleagues. What I seek to explore in this paper is the contention that archaeological explanation develops within and through frameworks that are fundamentally narrative in character” (Ballard 2003:135).
60. R. G. Collingwood, an inspiration for British “interpretive archaeology,” was a historian turned archaeologist! His The Idea of History (Collingwood 1956[1946]) is still essential historiography.

61. In contrast, in Preucel and Mrozowski’s (2010) Contemporary Archaeology in Theory: The New Pragmatism, only one of thirty-two contributions even mentions history (Timothy Pauketat, with Historical Processualism).

62. Life imitates art, or art imitates life? White asserts that history follows western literary traditions, but what if fictional literary plots mirror trajectories familiar from historical realities?

63. In 2013, historian Robin Fleming was declared a 2013 MacArthur genius for mixing material (archaeology) with documentary sources to write history for early medieval Britain. See Fleming 2011. Other historians who rush in where archaeologists fear to tread include Brooks (2013), Foster (2012), Fraser (1988), and Kelley (2003). As the poet Mac Rebennack said: If we don’t do it, somebody else will.

64. They’ve been thinking about how to do it—more than we have, alas. The introductory essay by (big) historian Daniel Lord Smail and cultural anthropologist Andrew Shryock from a 2013 American Historical Review forum, “Investigating the History in Prehistories,” is worth an extended quote (since few archaeologists read American Historical Review):

To work in the distant past, we must shift our focus, break a few well-entrenched analytical habits, and familiarize ourselves with new literatures and methods. As difficult as this retooling itself might be, working outside the narrative arc of modernity is an even greater challenge. What would this move entail? First, it would require that we analyze trends and events in ways that do not preconfigure them as moments of origin or points of culmination. Every developmental sequence would have to be connected to preceding conditions that generate an explanatory present, and this cascade of connectivity would reduce our narrative recourse to high contingency (or rupture) and increase the utility of comparison. Second, working outside the arc of modernity would mean that storylines could privilege neither themes of mastery over nature nor a growing capacity for freedom or agency, notions of moral progress, or attempts to associate these trends with increasing social complexity. It would be wrong to dismiss these tropes as misleading ideological commitments—they are often indispensable to social movements and political action—but they should not be treated as essential elements of historical storytelling. (Smail and Shryock 2013:722)

For their Deep History: The Architecture of Past and Present (Shryock and Smail 2011), they invited a few archaeologists to the party. History will have to “re-tool,” “break a few entrenched habits,” “work outside the narrative arc.” American Anthropological Archaeology will have to do so, too, and even more.

65. For the Southwest, several narratives bridge (late) pre-history to history. These are mostly the work of nonarchaeologists: Sociologists and historians. For example, Thomas Hall’s (1989) Social Change in the Southwest, 1350–1880; Scott Rushforth and archaeologist Steadman Upham’s (1992) Hopi Social History, which spans 1450 to 1990; William Carter’s (2009) Indian Alliances and the Spanish in the Southwest, 750–1750; James F. Brooks’s (June 2013) article in American Historical Review, “Women, Men, and Cycles of Evangelism in the Southwest Borderlands, AD 750 to 1750.” And of course my History of the Ancient Southwest and John Ware’s A Pueblo Social History.
There are others (and attempts in other regions; e.g., Kehoe 2002). Seek them out, see how they do it. We’re all learning here.

66. And later, Ian Morris argues that archaeology fifty years ago was history (at least in Europe) but today it has fragmented into science, social science (“probably anthropology”), or “archaeology is archaeology is archaeology…. Or maybe it is like literary criticism, or even a form of political activism…. But, overall, the one group that archaeologists hardly ever hold up as a model is the tribe of historians, the only other scholars to devote themselves systematically to the human past” (Morris 2010:3).

67. In his book *Archaeology as Cultural History*, Ian Morris (2000:3) proclaims: “Archaeology is cultural history or it is nothing. I hold this truth to be self- evident, but like most such truths, the problems begin when we try to say exactly what it means.” He argues cogently that neither Processual nor Postprocessual archaeologies have come to grips with history and historiography—contra Postprocessual noises about the Historical Turn. Here’s Morris (2000:24) on Postprocessual’s engagement with history:

> Despite postprocessualists’ rhetoric, the 1980s historical turn was less serious in archaeology than in other social sciences, largely, I think because it went on within frameworks inherited from 1960s arguments about culture history vs. culture process, and ultimately from late nineteenth-century divisions of academic labor…. To the extent that sociologists and anthropologists talk about history, postprocessualists take over their language. They have been excited by what they have read in Bourdieu, Giddens, and Sahlins, but they have taken history as second-hand from them, feeling little need to engage directly with historians.

Morris (2000:19) notes that “by the 1920s they [in US] had enough evidence to conclude that the natives did have some kind of history before Europeans arrived.” He lauds Walter W. Taylor’s (1948) insistence on history, but “Taylor’s lone voice could not overcome three generations of institutional divisions and anthropological hostility to historicism” (Morris 2000:21).

It is important to note that Morris’s “cultural history” is not American Anthropological Archaeology’s “culture history.” They are very different things. I assume the reader knows the American usage of “culture history”—a nonnarrative time-space matrix showing us who and what was where, when (Lyman, O’Brien, and Dunnell 1997). Morris’s “Cultural history” is a trend in academic history which, in brief, imports anthropological sensibilities into conventional history. For a review of cultural history, see Arcangeli 2011.

Peter Burke (2004) surveys that kind of “Cultural History,” a disparate and divided approach in History, which arose in the 1970s and by century’s end was ascendant in the academy. “The common ground of cultural historians might be described as a concern with the symbolic and its interpretation. Symbols, conscious or unconscious, can be found everywhere, from art to everyday life, but an approach to the past in terms of symbolism is just one approach among others. A cultural history of trousers, for instance, would differ from an economic history of the same subject” (Burke 2004:3). “One of the most distinctive features of the practice of cultural history, from the 1960s to the 1990s, has been the turn to anthropology” (Burke 2004:30). But “New Culture History” is no longer new. “It becomes impossible to avoid the question whether the time has come for a still newer phase…a more radical movement or…a rapprochement with more traditional forms of history” (Burke 2004:100). He cites,
with approval, Marshall Sahlins, who showed that “it is possible to write cultural history itself in a narrative form, very different from the relatively static ‘portraits of ages’ of early cultural history” (Burke 2004:123).

68. Ian Morris (2000:7) again: “The crucial issue is not the presence or absence of writing but the density, quality, and variety of data points.” Data points in the Southwest are dense indeed, high quality, and wondrously varied.

69. Hayden flirted with postmodernity (and is hailed as a hero by théorie historians) but he emphatically denies he’s a relativist and denounces nihilists like Lyotard: For White, the past actually happened. However, I’m not a huge fan of Hayden’s historiography. For more solid stuff, I recommend Ann Curthoys and John Docker’s (2005) Is History Fiction? American no-nonsense historiography: They dismiss relativism in their review of “Anti-Postmodernism and the Holocaust” (chapter 10); and in “History Wars” (chapter 11) they deflate théorie—referring Keith Windschuttle’s (1996) Killing of History. (Windschuttle said things that needed to be said, but he’s perhaps not the most useful ally.) For a more measured, academically acceptable product: Jeremy Popkin’s (2015) From Herodotus to H-Net: The Story of Historiography—quoted from time to time in this book.

70. You needn’t be a Positivist, if that’s socially awkward. But what’s the harm in a little informal Positivism? Not Comte’s Positivism, but a rosy upbeat notion of a knowable world. As the poet Mercer wrote and the chanteuse Fitzgerald sang: You’ve got to accentuate the positive / Eliminate the negative / Latch on to the affirmative / But don’t mess with Mister In-between. This is not a throwaway joke; we will return to this theme in the discussion of archaeology’s loss of authority—which actually matters.


As noted in chapter 4, note 24, we have Big Site atlases for every Pecos stage from Basketmaker III to Pueblo IV. We have the data. But we do not (yet) have the histories which could turn those data into historical geographies.

We need narratives! Note that Meinig used other people’s histories—much like Diamond and Turchin (discussed above). It is the synthesis and presentation that was uniquely Meinig’s own—and his cartographer’s!

72. Big History routinely casts cultures and civilization as agents. Rome did this, Han did that. There is an interesting conversation among historians about scale in American Historical Review: “How Size Matters: Questions of Scale in History” (2013, American Historical Review 118[5]:1431–1472).

We don’t like our taxons used this way: Michael Graves (2010), in his Choice review of History of the Ancient Southwest, noted with distaste that “cultural units operate at regional scales, with successive geographic florescence followed by diminishment and political leaderships that never seem to get it quite right.” Well, right: That’s pretty much how the deal went down. That’s standard in world history.

73. By focusing on nobles (political agents) I could be accused of ignoring commoners. Surely a complete history would have both? Well, we know a lot about Southwestern commoners: They’ve been our principal study for a hundred years. Until very re-
cently, we didn’t know we had nobles. Alas, many of my Southwestern colleagues still
can’t see ’em, through the fog of Pueblo Space.

74. This argument, when I’ve presented it at conferences, etc., is met with annoyance or
sometimes even anger because it seems to cut Indians out of the loop. I’m all for hav-
ing Indians in the loop. My point is almost exactly what théoriciens are saying (and
what Lew Binford said, many years ago): We understand the past IN THE PRE-
SENT. Archaeology writes history or pre-history to be understood IN THE PRESENT.
We do not write fanciful accounts of how we think “history” might have been told
by actors and agents—in this case, by Indians—of ancient times; or (contra some
threads of théorie) how those then-current events might have been perceived or
understood by Indians of ancient times. How could we even pretend to know those
things?

Nor do we write fanciful accounts of how traditional history/heritage might be
told by tribal peoples. That is, literally, none of our business.

75. These principles seem solid, safe, and simple when compared to the frankly astonish-
ing assertions we read about spiritual life in the ancient Southwest. Statements about
what people thought and believed a thousand years ago have become routine, almost
de rigueur; but most are based on nothing more than a wing and a prayer—and Par-
sons’s (1939) *Pueblo Indian Religion*. I can and have justified my axioms and methods.
You may not agree with them, but at least their rationale is knowable. Insofar as there
has been methodological thought behind ideological up-streaming, it appears to be
vague, faith-based assertions that “Pueblos are very conservative about religion.” The
leaps are amazing.

76. They react to my work, rather than read it. *A History of the Ancient Southwest*’s one
negative review (honest, there was only one that I know of) came from a distinguished
archaeologist who fumed and sputtered about my jokey titles and taglines, and pretty
much missed the whole point of the book. Oh well. This happened before, with *Chaco
Meridian*, loudly condemned by people who hadn’t read it. More than a few people,
as it turns out. One such incident is retold by David Roberts (2015: Chapter 4).

77. “A conjunctural scaffolding helps to construct a better house of history” (Braudel
realm (economics) versus trends in another (demography), etc.

78. I’ve nagged so often about Thinking Big, my nags would fill a long and tedious tome.
Here’s another, not from me but from a superb prehistory/history of the Mediterra-
nean:

At its best, a close focus encourages us to look at regional and micro-regional
processes…. Without such insights, large-scale history becomes suspiciously
smooth, and attempts to understand commonalities and interconnections re-
main just-so stories…. At its worst, however, the result [of “close focus”] is ar-
chaeological and intellectual parochialism, a myopia that misses the linkages
and parallels that give structure and explanatory coherence to the whole, that
exalts what Freud condemned as the “narcissism of small difference,” with
each case unique unto itself. Degrees of diversity are hardly a surprise, given
that none of us is able to transmit even our genes and ideas with complete fi-
delity; far more striking are those instances where, despite the inevitability of
difference, we find that we can in fact still connect, compare, and generalize
to good effect. Archaeology… too easily finds itself in the position of a person
at the bottom of a well, who can see a small patch of sky with perfect clarity,
but misses the scope and constellations of the heavens. (Broodbank 2013:23)

> Related approaches in anthropology are interpretations based on cables, comprising multiple strands of evidence, and tacking, a dialectical process between the contrasting perspectives of the anthropologist and the subject. [citations omitted] This is analogous to the process that Wylie (2002) has called “triangulation,” whereby an interpretation that may not be entirely convincingly demonstrated from any one source becomes more precisely grounded by triangulation with other sources of varying independence. (Bell 2015:50)

My triangulation is a bit closer to David Hurst Thomas’s (1991) “cubist perspective”—looking at things from every angle. If extended outward, triangulation is not far off the historian’s technique of “contextualization”—looking back in from one more layer out, concentric contexts: Time, space, history, whatever. It’s always worth going two or three layers out, and then, when you think you’ve gone far enough, go one more.

My triangulation point of temporal peers is, I think, not the same as historian’s “side-streaming.” Side-streaming is the use of ethnographic information from contemporary tribes and peoples to infill details of a group of interest, for which that type of information is absent (e.g., Richter 1992:5; Hämäläinen 2008:13). This is rather like ethnographic “up-streaming” (discussed elsewhere). Temporal peers may be quite unlike the triangulation target.

80. At least, that was the story in Lekson 2009a. New data—discussed elsewhere in this book—gets Chaco going several decades earlier than we’d thought, with a great deal of input (wood, for example) from the south, rather than the north. These new facts, along with several other recent studies, require us to rewrite our pre-history. That’s fine: That’s exactly how history works, post hoc accommodative arguments! But not here, in this book. One task at a time.

81. Comparison has been largely abandoned in cultural anthropology; HRAF is conspicuous by its absence in most sociocultural articles in American Anthropologist. (Devotees remain, but only a few.) Comparison is also under fire in American Anthropological Archaeology, particularly from théoriciens. Not many of whom work in the Southwest, for now. But Tim Kohler (2012:331), in an approving review of Michael Smith’s (2012) Comparative Archaeology of Complex Societies, notes with regret that rigorous, systematic comparison is uncommon: “Once the camel’s nose of comparison has cracked open archaeology’s somewhat claustrophobic tent, who can tell what fresh air might get in?”

82. Not to be confused with counternarratives! This book and my History of the Ancient Southwest are counternarratives (not counterfactuals)—as Norman Yoffee defines them: “By counternarratives, I mean simply those ideas that have challenged prevailing wisdom, examining current ‘paradigms’ and finding flaws in them, and then producing new ideas and explanations for social behavior and change…. Now, this goal may seem perfectly obvious to archaeologists and world historians, but until recently, one may claim that few studies, especially in archaeology and ancient history, have taken up the challenge” (Yoffee 2016:346).

He develops four counterfactual histories of the twentieth century, and finds it a useful exercise. Another Classicist in the lead!

Most historians are less enthusiastic. Elazar Weinryb’s (2008:109) essay on “Historical Counterfactuals” in A Companion to the Philosophy of History and Historiography is typical:

Historiography is replete with counterfactuals.... Notwithstanding the ubiquity of counterfactuals in historiography, traditionally counterfactual thinking has been quite unpopular with historians.... Counterfactuals form a part of our understanding of the past because they play an essential role in explanations of action that try to take the historical agents’ point of view.... [Counterfactual] causal ascriptions have an essential role in practical inferences, as possible courses of action are viewed in the light of probable outcomes. (Weinryb 2008:109, 115, 116)

The British philosopher Winston Churchill (who is quoted several times in this book, sometimes accurately) was a prolific popular historian. He contributed to an interesting early volume of counterfactual essays, If, or History Rewritten (Squire 1931). Churchill’s essay, “If Lee Had Not Won the Battle of Gettysburg” (in Squire 1931:259–284), is a real twister: It starts with the counterfactual of Lee winning, and then spins a counter-counterfactual from that perspective of what things would be like if the North had won. Ties your brain in knots: I do not recommend this procedure.

84. Janet Spector’s (1993) use of “fictionalized accounts” in What This Awl Means was widely praised, as was Charles Hudson’s (2003) Conversations with the High Priest of Coosa—both scholarly books delving into fiction. The last time any scholar tried that in the Southwest was Adolph Bandelier’s (1890) Delight Makers—not counting many books by real novelists: Kathleen and Michael Gear, Louis L’Amour, Douglas Preston, and others.

85. It would probably be useful for archaeologists to study historical fiction or what today is called “nonfiction novel”—if there were courses or analyses or scholarly treatises. Are there? Not that I could find, in a search of the web and the library.

86. The Ontological Turn right down the rabbit hole. For ancient times and prehistory, that goal—knowing how men and women from the past thought about their world—seems as difficult (or more so) as the enthusiasms of not long ago for “the individual in prehistory.” Every once in a while we can see an individual, streaking like a subatomic particle across a sensor; but we can’t expect to build narratives about individuals, much less their thoughts and motivations. Except for truly exceptional situations, the quest for “individual in prehistory” was doomed before it began. So, too, ancient ontologies. As Henry James said: It’s humbug.

87. “Staying close to the data” sounds laudatory. It’s not; as a mantra, it’s nonsense. Stay really close to the data and you limit them to chipping rocks and breaking pots, occasionally stacking up stones for a house. Staying close to the data for a century gave us a corn-beans-and-squash Southwest, the Southwest of Pueblo Space.

Archaeological data are an uncertain small sample of a tiny fraction of fragmentary detritus representing—very indirectly—a vast, dynamic array of events. Binford and Hodder agreed that everything beyond the measurement of a posthole is interpretation. How “far” from the data can our interpretations go? How long can our chain of inference stretch?

88. For example, Bernardini 2005; Fowles 2013; Liebmann 2012; Ortman 2012; among others. A rich field on the right side of the Great Divide! My sketch of pre-history
(Lekson 2009a) ended formally at 1600, about the time the Spanish arrived; but in reality modern-ish Pueblos emerged about 1450. The era from 1450–1600 was not without incident; historically complex but underresearched. Recall that this late period was indicated for “history” in the list of “Key Issues...in the Archaeology of the American Southwest” (Plog et al. 2015).

89. Ronald Mason’s 2006 book-length critique of “ Archaeology and North American Indian Oral Traditions” met a rough reception. (The main title: Inconstant Companions.) Well worth reading, however, for a hard-nosed analysis of the issue. For a more optimistic (but not uncritical) assessment, see Whiteley 2002.

90. Two recent considerations, the first by Stephen Plog and the second by Michael Schiffer, of traditional histories and heritage in archaeology. Stephen Plog:

The connection between Navajo oral traditions and Chacoan patterns thus needs to be demonstrated, not asserted. Oral traditions are important sources of information that too often have been neglected by generations of archaeologists. They warrant the increased attention they have received in recent years. However, I agree with [cultural anthropologist Peter] Whiteley (2008: 575) that “a critical question for effective use of oral traditions in archaeological explanations...lies in their transfer to propositional statements subject to canons of testability. I do not agree with the skeptics who argue that no scientific propositions can be generated from oral traditions, but an unquestioning teleological reading from present socially embedded perspectives raises heuristic problems. If there is to be special pleading for some epistemologies, where critical scrutiny is somehow off-limits, this will vitiate their value for scientific explanation.” This is not to claim that all people must evaluate oral traditions in this manner. Different people choose different ways of knowing the past. For archaeologists, however, I believe that it is important for the advancement of knowledge that we evaluate propositions and we present the evidence on which those evaluations are based. (Plog 2010:382–383)

And Michael Schiffer:

People may accept their society’s stories without question, but archaeologists—as scientists—are a skeptical lot. We do not take stories at face value but treat them as hypotheses that may be evaluated on the basis of relevant evidence. Sometimes our research validates a story, other times it does not. Archaeological findings can become the focus of controversy, especially if they undermine traditional beliefs. (Schiffer 2017:3)

I applaud Schiffer’s (one hopes, intentional) nod to Indiana Jones’s dictum, “We cannot take mythology at face value,” which I was hoping somehow to use in this section. And now I have, without getting my hands too dirty. Thanks, Mike!

91. Peter Nabokov in his essay “Native Views of History” puts it thus: “The spectrum of American Indian narratives, behaviors, and symbols which carry any information faintly deemed ‘historical’ actually falls on any number of different points between the idealized poles of chronology (history) and cosmology (mythology)” (Nabokov 1996:9).

92. Traditional histories in the Bible lie at the core of the Western tradition, Western heritage. Credence or criticism given by archaeology to those holy texts famously split biblical and Levantine archaeologies. The former bolsters biblical heritage, the latter
writes history. Serious History treats the Good Book as any other traditional text—judging its parts relevant/irrelevant, right/wrong, etc. Elsewhere in the canon, the great classicist Moses Finley warned us about a seminal Western traditional history: “Homer’s heroes recite their genealogies frequently and in detail, and without exception a few steps back take them from human ancestors to gods or goddesses” (Finley 1990:27). Schliemann’s attempts to validate Homer by finding Troy or Evans’s search for Minos at Knossos are famous examples of European traditional histories (heritage) gone wrong. Viking archaeology conventionally was driven by its traditional histories, the Sagas. That’s changing: The actual archaeology is now in the driver’s seat, and the Sagas are subject to correction. Richard Hall, then director of the York Viking excavations, on changing archaeological engagement with Sagas:

Until quite recently the Icelandic sagas and other more learned works written in the twelfth and thirteenth centuries were regarded [by archaeologists] as almost infallible pointers to sites where named individuals built their farmsteads or famous events took place in the Viking Age, 300 or more years before the stories were written down…. Today, these literary masterpieces are treated more carefully by archaeologists…. Subconsciously, or with art and craft, the saga writers were reinforcing the views of their [twelfth- and thirteenth-century] contemporaries. (Hall 2012:155)

And of course there is the classic African work of Jan Vansina (1985)—American Anthropological Archaeology’s go-to citation for the reliability/unreliability of oral traditions. There’s more out there than his Oral Tradition as History, as it turns out.

9. Here are some highlights (Smith 2011b; internal citation omitted):

Among scholars, four positions on the historical usefulness of Aztec historical accounts can be identified: 1) a highly credulous attitude that assumes that most of the native historical records do indeed record accurate information if we can just find the correct interpretations; 2) the application of explicit historiographic methods coupled with key assumptions leading to the view that the Aztec histories do preserve some valid information on Tula, Tollan and Topiltzin Quetzcoatl; 3) the application of explicit historiographic methods and a more critical attitude, leading to the view that events of the Early Post-classic period (and certainly the Epiclassic period) are so far removed from the time of production of the surviving accounts that they are outside the realm of credible historical reconstruction; and 4) the assertion that no usable historical information exists in the native histories. The first and fourth views are fringe views that need not concern us further; the important issue is the distinction between the second and third positions. Comparative cases of oral political history indicate that such accounts rarely have great time depth; this finding supports the third position. (Smith 2011b:476)

He notes Joseph Miller’s (1980) studies in Africa, Fentress and Wickham’s (1992) conclusion that oral history reflects the “groups feelings and beliefs, rather than what the past itself was,” and David Henige’s (1974) The Chronology of Oral Tradition. “Henige (1974:190–191) concludes that in most cases, oral political history does not preserve reliable chronological information for more than a century prior to transcription” (Smith 2011b:477). “Given what we know about the context and production of native histories in Yucatan and central Mexico, and the results of comparative research by
Henige and others, it simply is not tenable to maintain that these traditions can provide historical information on Tula and Chichén Itzá” (p. 478)—sites about as old as Chaco. Worth noting.

94. Tribal traditional histories in the Southwest are, in Nabokov’s (2002:239) words, “uniquely American Indian blends of spiritual, documentary, and opportunistic contemplations of the past.” They challenge the conventional Western historian, he notes, but certainly the conventional Western historian must accord them evidentiary status. But, he concludes, “instead of cramming them into familiar paradigms, might we not temper the hegemony of Western historiography by interpreting it into them every now and then?” (Nabokov 2002:239; emphasis original). That is, instead of cherry picking from recorded “myths” and “stories” to write Western history (as I and many others have done), reverse the flow and offer Western history to the Native historians for their evaluation and interpretation and possible use. This seems a noble (if somewhat unlikely) goal, but fraught: What if the Western history I write is something Native historians do not want to hear? Which appears to be the case with my version of Chaco.

95. Severin Fowles observed: “How different the picture looks if when we seriously investigate the migrations, creolizations, battles, alliance and so forth that fill indigenous histories!” (Fowles 2013:91). Yes indeed; if not all the details, at least the assurance that there was indeed history in the ancient Southwest.

96. For example, Silko 1996 and Swentzell 1991. Here’s the Acoma poet Simon Ortiz (1994):

As a boy, I used to hear elders speaking about...the past, but not as some faraway event; it was as if it was right in the present. That’s why it is so important to regard the past: because it is the present.... It is all one moment, and I am certain that there is no line, gap, margin or barrier between the natural and the human environment; there is only the power and sacredness of existence of the Pueblo and all Native Americans know it. (Ortiz 1994:69, 71, original emphasis)

Larry Zimmerman, a strong advocate of Indigenous Archaeology: “Simply put, the past and the present are essentially the same in content and meaning, although details may differ. As a tradition-oriented Native American, if you know the oral history of your people, you need no other mechanisms for ‘discovering’ your people’s past” (Zimmerman 2001:173). Gertrude Stein might have said: There is there there, but no past past.

97. Roger Echo-Hawk is not alone in thinking that Native tales recall the megafauna, extinct after about 13,000 years ago. I worked closely with a professor of American Indian Studies from Isleta Pueblo, who had the same idea.

98. Fred Eggan (1967), a generally sympathetic observer, traced the decay of Hopi accounts of a notable nineteenth-century dust-up between Hopis and Navajos, in two versions recorded forty years apart. The first, close to the actual event, was detailed and specific to the event. The latter, four decades later, was vague on both events and causes, and tended toward general Hopi principles and cultural differences between Hopi and Navajo. Eggan was not discounting oral traditions as sources of history, but he cautioned that much critical analysis would be required to separate the historical from the mythological or programmatic. Of course, traditional versions of relatively recent events can be demonstrably accurate (e.g., Wiget 1982); but are those “traditional” or rather eyewitness memories?
Peter Nabokov (2002:67–76) reviews various estimates of “decay rates” for Native American oral history. Most historians, it appears, assume an outside limit of 100 to 150 years for historical “fact” which then slides first into tradition, then myth. It seems likely that tribes living on or near the places where events happened will have “sharper” and longer historical traditions, that is, more details deeper into the past, linked to landscape features; see also Basso 1996. Another intriguing review of Native American oral history and “myth-history”: Snow 2016.

99. Michael Smith (2011b:477) cites various studies which suggest “that in most cases, oral political history does not preserve reliable chronological information for more than a century prior to the transcription of the oral tradition.” That’s not very encouraging. But on the bright side for the Southwest, Smith cites other studies that suggest “that societies with more open systems of social stratification tend to produce historical traditions that are empirically reliable, whereas societies with more closed social stratification systems almost always produce mythologized official historical accounts” (p. 477). That is, noncomplex societies’ traditional histories are truer than complex societies; where history has more work to do, legitimizing kings and so forth. Perhaps court histories of ancient Chaco—for surely they had histories!—twisted the truth like Aztec court histories; perhaps those histories died with Chaco. Pueblo traditional histories after that episode of “closed social stratification” stuck closer to what really happened.

100. Keith Basso famously wrote that Wisdom Sits in Places—the title of his 1996 book on Western Apache traditional history. See also Nabokov 2002:Chapter 5, “Anchoring the Past in Place: Geography and History,” for a broader North American survey. It is interesting to note that Southwestern tribal traditions refer to specific archaeological sites, while tribal traditions of the East apparently fail to mention any place that could be construed as Cahokia (and many other major sites). Most Southwestern tribes are still on or near their lands; most Southeastern tribes were displaced.

101. Pueblo people left Chaco and Aztec by 1300. Shortly thereafter—some would say even at that time—Navajo people became the sole proprietors and custodians of the old Chacoan polity. Navajo people live in and among its ruins and have names and stories for many key features, natural or built (e.g., Kelley and Francis 1994; Linford 2000). Those stories are often quite specific, in contrast to the poetic generalities of White House. For example, the powerful figure non-Native people call “the Great Gambler,” who one knowledgeable Navajo described as “our king” (see Appendix).

Navajos may have seen Chaco’s fall. They certainly learned much (shortly after the fall) from Pueblo people, incorporated into Navajo knowledge through capture, marriage, trade, ritual relations—a broad band of channels. Pueblo peoples’ traditional histories of Chaco—and the lessons and principles learned there—are central to their heritage; but not perhaps the details. (At least in accounts I’ve heard.) Navajo people know and share details. Navajo people live on that landscape and encounter the ruins every day, constantly refreshing memories through daily life. Relatively few Pueblo people visited Chaco places, on periodic pilgrimages; thus (I think) the details faded while the key themes and morals remained.

Acoma, Zia, and several other Keres Pueblos have traditional histories of White House, a notable place to the north where wonderful and terrible things happened (see Appendix). At White House, “people got power over people” and, ultimately, that was judged to be wrong for Pueblos. During Florence Hawley Ellis’s work for the NPS’s Wetherill Mesa Project, she identified White House as Mesa Verde. I have
suggested it was Chaco Canyon. Several Keresan Pueblo people have told me that those specific identifications are misguided: White House was the whole Four Corners area—which I would like to interpret as the Chaco/Aztec polity.

102. When the people left White House (see preceding note and Appendix), en route to a new home to be revealed by portents, they stopped to perform a "ceremony of forgetting." Matthew Stirling published an early account: "It is not known how far they went, but finally they stopped at a place where they went through the ceremony of forgetting. It is not known how far they went when they symbolically crossed the four mountains and left their sickness and trouble behind" (1942:75).

Barbara Mills (2008b) wrote eloquently about "remembering while forgetting" at Chaco—not in reference to this story, specifically. Suppressing powerful historical or ritual knowledge, or confining it to a few who need to know, appears to be common among Pueblos and, of course, many other societies. Including our own. We release information on a need-to-know basis.

The problematic events at White House (Chaco/Aztec?) were never forgotten, but the ceremony of forgetting probably ensured that the full story is remembered (and recounted) only when needed in the present—the bad example with which to correct improper behaviors.

103. Leigh Kuwanwiswima (Hopi) writes: “Few researchers have bothered to ask the Hopis who they are. Those who did were treated with suspicion by the Hopis and on some occasions deliberately given misleading information” (2004:43).

104. Leslie Marmon Silko (Laguna) wrote, “The ancient Pueblo people sought a communal truth, not an absolute truth. For them the truth lived somewhere within the web of differing versions, disputes over minor points, and outright contradictions tangled with old feuds and village rivalries” (Silko 1996).

105. The Indian Land Claims Commission, established in 1946, used traditional histories in its work: Compensating tribes for lost lands. The Native American Graves Protection and Repatriation Act of 1990 also considers traditional histories in establishing cultural affiliations of cultural items off reservations. Traditional histories can be potent legal tools.

106. When NAGPRA was new, Hopi sent a now-legendary letter making potential claims on just about everything everywhere. Why not? Hopi clans, indeed, have continental provenances, from sea to shining sea. Given the sad history of Indian Lands, going big made a lot more sense than playing small.

107. Consider Michael Katobie and Delbridge Honanie’s (Hopi; indeed, Artists Hopid) kiva mural at the Museum of Northern Arizona at Flagstaff. It begins with Chaco and ends with the World Wide Web.

108. "Rather than grading different narratives for some form of absolute truth content, we should be asking which alternative we find the most useful relative to the immediate question at hand" (Ballard 2003:144). Different narratives, like different Capital-“T” Theories, may be useful for different parts of the story.

109. Ian Morris rightly insists that we must develop ways to evaluate archaeological narratives: “There are ways to move outside a world of competing representations, and… this is necessary if we want a cultural history of society rather than a bloodless, aesthetized literary exercise” (Morris 2000:16–17).

Anthropology has been curiously uninterested in systematic evaluations of the kinds presented here. Or perhaps I slept through that lecture. Liebow and colleagues (2013) recently suggested standards “On Evidence and the Public Interest” in Anthropology, with these qualities of evidence: Credible, acceptable, actionable:
By credible, we mean “bias free.” By acceptable, we mean the degree to which the findings of a study or group of studies conform to conventional wisdom derived from local knowledge. By actionable, we mean the degree to which the findings of a study or multiple studies are feasible to implement in a particular situation. (Liebow et al. 2013:642)

These are not so much standards of proof or levels of certainty, but criteria for forming and implementing policy.

110. The past should not be presented as a matter of belief. To “believe” lowers us to the level of religion: “It’s time to ask yourself, Dr. Jones: What do you believe?” (attrib. Walter Donovan).

111. This tag has also been attributed to Bernard Baruch, apparently on the basis of a quote in the January 6, 1950, Deming Headlight (!). Stunned by this unlikely Luna County provenance, I have made no effort to confirm or deny the Baruch attribution. If it is not true, I wish it were so.

112. Gwinn Vivian, in particular, took me to task:

Lekson notes his frustration with colleagues who ask for “proof” to support his revised Puebloan history. I am frustrated by his admonition that “we allow ourselves to see” this “extraordinarily visible, knowable example of political continuity across time and tide” using only “circumstantial, anecdotal, (and) juristic” evidence. In 1994 Lekson, Linda Cordell and George Gumerman urged the building of “a comparative archaeology of polities and their residual landscapes” that would include “archaeologically-knowable empirical patterns of architecture, settlement, and region...” In 1999 Lekson observes that such an archaeology does not exist. Why not use the Chaco-Aztec-Paquime data to initiate just such an archaeology? The Chaco Meridian presents some of the data; it is time to develop a methodology worthy of testing those data. (Vivian 2001:144, internal references omitted)

I did indeed call for a comparative archaeology, all those many years ago; and by and large, it still does not exist. When I tried to compile compendia of Early State landscapes and so forth, I was hissed away by those studying Early States for my presumption in thinking that Chaco could be usefully compared to that group. They were right: Chaco was not an Early State, it was a secondary state. And those are so varied in structure that I despair of single-handedly making sense of them.

All archaeological evidence is circumstantial, and much is anecdotal. It was the “juristic” form of my argument that, I think, most annoyed my annoyed colleagues. Perhaps that was the wrong term—or, as we shall see, perhaps it was in fact appropriate—certainly I think to make better historical arguments, archaeology might learn more from law than from science.

Oddly enough, it was only upon writing the preceding sentence that I remembered Vine Deloria telling me precisely that, long ago; something like, “Archaeologists don’t know how to mount an argument; they should take some training in law”—as he had. So maybe I owe this interest to Deloria.

113. A terrible word, but Roget offers nothing better. “Convicentment” is entailed by “I am not convinced”—the kiss of death in the archaeological phrase book.

114. You’d have to really care about Grant to work back to source references, analyze the two arguments, and essentially write your own biography of the man. Most of us don’t care that much about Grant, except on a fifty.
115. For the Pueblo III conference, we picked people who had the requisite knowledge and who—importantly—would play well together. The result was a well-structured volume (Adler 1996) that inspired a series of time period pieces, all of which were, in their time, essential. For the Chaco Synthesis, I tried to engage everyone who had any standing on Chaco matters (several declined; but the offer had been made). The result was a volume (Lekson 2009a) that, despite a reasonably logical organization, was all over the map. It has been described as a landmark volume, but also correctly criticized for its many “Chacos.” Chaco chaos.

116. History uses peer review, of course. Looking at the American Historical Review’s description of peer review, the process seems very much like the exercises with which we are familiar.

117. Editors of scholarly journals are active shapers of content. Each journal has a mission, a style, and a set of parameters for what’s appropriate and what’s not. Editors have a huge impact: Remember when the Tedlocks took over American Anthropologist? That was the beginning of the exodus of archaeologists from AAA. Even today’s pay-to-play online journals: Their mission appears to be making money and advancing the careers of entrepreneurial scholars. That’s a mission, a style.

118. “Post-publication review” is new (and not yet widely used) but interesting. Publish, review, and, depending on the reviews, list or delist. Online preprints have huge potential: Cheap, quick, and gets out to the audience. But will we read ‘em?

119. Lopping off the right quartile: “Perhaps the most widely recognized failure of peer review is its inability to ensure the identification of high-quality work. The list of important scientific papers that were rejected by some peer-reviewed journals goes back at least as far as the editor of Philosophical Transactions’s 1796 rejection of Edward Jenner’s report of the first vaccination against smallpox” (Michaels 2006:224).

It may be useful to think about this question as “precision” and “accuracy.” Working within Pueblo Space limits us to a set of interpretations, which with great regularity we can predict will be made again and again: That is, they have the appearance of “precision,” hitting the same spot on the target again and again, the clear central tendency. Without the restrictions of Pueblo Space, our shots might be more scattered, but some will hit the target: “Accuracy.” Archaeology outside Pueblo Space: Less precise but more accurate.

120. My bell-curve metaphor is flawed on several counts. Most significantly, reviewers are not random. As a journal editor and sometime reviewer myself, I know that reviewers are picked because of their perceived expertise, and after that first criteria, because they are Good Citizens or they owe the editor a favor. Nonrandom.

121. Variously attributed to Benjamin Disraeli and/or Mark Twain, paraphrasing Disraeli. Among others.

122. The most strident: Carrier 2012. He ventures into an extended promotion of Bayesian history in aid of his quest to prove Jesus didn’t exist. One mordant blogger noted: “The Reverend Bayes originally used his theorem to prove the existence of God, while in his next book, Carrier will apparently use the same theorem to disprove the existence of the historical Jesus” (Bond 2013). Not a good book, but an interesting argument.

123. There are good reasons why frequentist (or formal) statisticians locked Bayes in the closet through most of the nineteenth and twentieth centuries. Bayes cooks the books. Bayesian statistics build in “prior knowledge” which can mean “bias,” hiding that statistical crime deep in the thousands of iterations involved in reaching Bayesian conclusions. Another good reason why frequentists discounted Bayesian approaches
was the sheer computational power required for Bayesian work; real roll-up-your-sleeves Bayesian statistics were possible only on mainframes before computers arrived on everyone’s desk in the 1980s. For an accessible account of the controversy, see McGrayne 2012; for inaccessible accounts, try the web.


126. Other standards of certainty exist, for example in intelligence/security. These can be life-and-death judgments. How sure are we? Should we send in the drones? Happily, far less hangs on archaeological certainty. An errant account of the past (how would we know?) does little real damage; an incorrect or inadequate predictive model might destroy a few sites; but nobody gets hurt. If the CIA goofs, it’s not pretty. For a peek at how spooks think about these things, see: Estimative probability, https://www.cia.gov/library/center-for-the-study-of-intelligence/csi-publications/books-and-monographs/sherman-kent-and-the-board-of-national-estimates-collected-essays/6words.html.

The CIA did not adopt this terminology. They must have a method, but it’s classified. I don’t know that for a fact, but I make that statement with High Confidence.

127. It seems that there are British precedents for invoking legal standards in archaeology: “Augustus Pitt-Rivers is often called the ‘father of British field-archaeology,’ and he introduced the concept that archaeological evidence should be able to stand up in a court of law” (Thurley 2013:107). I have not followed up on this; would current British theoretical archaeology survive Pitt-Rivers’s test?

128. For example, Kosso 2008. Discussing “evidence” he contrasts history-science versus history-courtroom trial. History, he says, is much more like a legal argument than scientific hypothesis testing (Kosso 2008:15–16). I agree.

129. For science, the Daubert test from the Supreme Court case Daubert v. Merrell Dow Pharmaceuticals, 509 U.S. 579 (1993):
   1. The theory is testable.
   2. The theory has been peer reviewed.
   3. The theory has defined reliability and error rates.
   4. The theory is generally accepted by the scientific community.

And for history: There are legal standards for what constitutes an “objective historian,” which came out of a UK court case over holocaust denial (Irving v. Penguin Books; briefly encountered in chapter 5). These standards were summarized by Wendie Schneider in the Yale Law Journal. My paraphrase from Schneider (2001:1535), regarding the (legally) contentious historian:

   (1) She must treat sources with appropriate reservations; (2) she must not dismiss counterevidence without scholarly consideration; (3) she must be evenhanded in her treatment of evidence and eschew “cherry-picking”; (4) she must clearly indicate any speculation; (5) she must not mistranslate documents or mislead by omitting parts of documents; (6) she must weigh the authenticity of all accounts, not merely those that contradict her favored view; and (7) she must take the motives of historical actors into consideration.
Schneider notes (2001:1531): “Historians are not alone among social scientists and other nonscientific experts in confronting an absence of coherent standards.”

130. For example: David A. Schum’s (2003) “Evidence and Inference about Past Events” summarizes case studies (in the book of that same name), and very usefully presents discussions/summaries of:

(1) “substance-blind”—“ignore the substance of evidence and focus instead on its inferential properties” (Schum 2003:15), and provide a chart or spreadsheet for characterizing: on one axis, “tangible, testimonial unequivocal, testimonial equivocal, missing tangibles or testimony, and authoritative records” and on the other axis, a scale of decreasing relevance: direct, circumstantial, and ancillary (my paraphrase of Schum 2003:16, Fig. 1.1).

(2) chains of reasoning to establish “directly relevant and ancillary evidence” (Schum 2003:21, Fig. 1.2).


(4) probabilistic arguments, including Bayes, Baconian, and Belief Functions (Schum 2003:25).

(5) Wigmore analysis [a kind of Harris Matrix of events and certainties]—widely cited (Schum 2003:26–32; see Wigmore 1937).

131. Preponderance of the evidence is the legal standard under NAGPRA for determining cultural affiliation. In practical terms, it establishes “cultural affiliation”; in archaeological terms, social identity. What is preponderance of the evidence? More probable than not; a bit better than 50-50. In practice, it is often (and inconsistently) translated as more classes of NAGPRA evidence than a competing claim, or more than half of the NAGPRA classes. I have seen preponderance of the evidence applied as a check-off: If one claim has evidence (of any kind or quality) for more of NAGPRA’s evidence classes than another claim, that is preponderance of the evidence. Hopi could list five of seven classes; Navajo could list only one: Result, it’s Hopi. NAGPRA is serious business. Metaphorically it’s life and death for tribes, and concretely it’s life or death for museum collections. If “preponderance of the evidence” is sufficient to restore patrimony or to empty a museum, I could argue that it should suffice for the far less serious problems of archaeological narrative creation. But I don’t make that claim; I’d like to hold archaeology to a higher standard.

132. Weiss (2003) equated “clear and convincing evidence” in law with “substantially proven” in science, with a probability of 90–99 percent. That seems impossibly high for archaeology. In his probability allocations, archaeology is more likely to hit “preponderance of the evidence—more probable than not; more likely true than untrue [50–67 percent]”; or maybe sometimes “substantial and credible evidence; probable [67–80 percent].” But “clear and convincing” in its legal definition, unshackled by statistical probabilities, is my goal.

133. Of course we fuss about evidence: Convoluted 14C, contaminated tests, compromised contexts, etc. But compared to arguments about interpretation, evidence is fairly straightforward. Thank God.

134. Thoreau was called both “Hank” and “Crank” but I am unable to find definitive authority that he was, in his time, called “Hank the Crank.” I submit that if he wasn’t, he should have been.


This approach cannot become a refuge for those practitioners unable or unwilling to organize and present their data coherently. The data upon which our narratives are based must be available for independent review and reinterpretation by others if only relegated to fine-print appendices at the back of the report.

I hope we can rethink and reinvent the notions of appendices and footnotes—relics of print.

137. My imagination wobbles between the Gordian Knot and an exploded baseball. Plutarch meets The Natural: “Ya really knocked the cover off that one, Alexander!”

138. Jeremy Popkin in his 2015 historiography welcomes the brave new world of “History in the Internet Era”:

The most significant historiographical developments in the years since 2000 have not been shifts in the balance between the various forms of history, but rather a new awareness of the importance of changes in the ways in which historical knowledge is communicated. No new methodological approach to historical research has had as much impact on history in the past two decades as the development of the Internet. (Popkin 2015:170)

139. I attempted to recast “top-heavy” Chaco along the lines suggested below, but in print for this book. It did not work, and you (and the publisher) can thank whatever gods might be that I left it among the shreds and patches on my cutting room floor. It had links within links indicated by brackets within brackets, some of which were so concentrically layered that it reminded me of the old John Barth story, “Menelaiad,” in which a half-dozen simultaneous narratives within narratives came together in the single word: """"NO!"""" But the structure I’m thinking of is not nested and hierarchical, but relational. So I tried a graph; that, too, was beyond my talents. What I’m thinking of is more than 2-D print can do, which of course is precisely the point. But more than my 2-D mind can do.

140. Opening up a giant economy-sized #10 can of political worms. But if we can issue permits for archaeological fieldwork, if we can trust crusty editors with our cherished prose, surely we can devise a scheme to keep out crazies—without the pitfalls and perils of peer review.

141. An odd relic of archaeology’s humanist origins: Sole authorship, lead author, etc.—not the multiple authorship of most scientific disciplines: Twelve authors, twenty authors!

142. For some interesting ideas on how to begin: Copplestone and Dunne (2017).

143. Lange et al. 1986. As published, her review read “the description of digging,” but Swentzell assured me it was meant to be “the destruction of digging.”

Appendix: Indigenous Chaco

1. A quote from a Pueblo man in a Solstice Project video (Sofaer 1999).
2. See, for example, Ferguson and Koyiyumptewa 2009.
3. E.g., Chapin 1940; Matthews 1889. And see versions presented in Kelley and Francis 1994. I have benefited from long discussions on this matter with Tim Begay and Taft Blackhorse, among other knowledgeable Dine people.