Archaeologists today are being urged from within and outside their profession to incorporate aboriginal oral traditions in reconstructing culture histories. Such challenges usually ignore or at least drastically underestimate the difficulties in doing so. Not least among those difficulties is that of attempting to reconcile inherently and profoundly different ways of conceptualizing the past without violating the integrity of one or the other or both. The pro and con arguments are examined theoretically and as actually employed in discrete instances. These raise such problems of incommensurability as to severely limit the fruitfulness and even desirability of making the attempt.

Hoy en día se espera que los arqueólogos, adentro como de fuera de su profesión, incluyan en sus historias culturales las tradiciones orales de los indígenas. Estas peticiones por lo general ignoran o por lo menos no dan suficiente importancia a las dificultades de esta empresa. Entre las más imponentes es la dificultad de reconciliar dos modelos del pasado que son inherentemente y profundamente distintos. Aquí se examinan los argumentos de ambas posiciones en teoría y ejemplos. Estos esfuerzos indican grandes problemas, e inclusive sugieren que la empresa es inútil.

This essay examines current challenges to incorporate North American Indian oral traditions in archaeological theory and practice. Because the essay’s focus is on archaeology as it has been and potentially will be affected by considerations of such traditions, for purposes of this discussion archaeology is privileged when confronted by incompatible oral traditions, ethnographic, linguistic, or any other information unless they can be shown singly or in concert to enjoy preponderant weight. Of course, preferred answers to cultural historical questions such as archaeology addresses lie at the point of convergence of multiple independent evidentiary lines. But short of that ideal, researchers must provisionally go where the predominant evidence and most parsimonious explanations lead, bestowing greatest trust on those grounds they know best.

However much one may agree or disagree with Leslie White’s conclusion that culture historical studies “may well be counted as the most substantial and illuminating achievements of cultural anthropology” (1966:36), it would be improper to surmise that such studies are the only subject of interest to archaeologists. Nevertheless, the following exposition is focused on one aspect of just that: specifically, the use by archaeologists of oral traditions as historical data.

As considered here, archaeology is a primitive science with high aspirations. Pursuing these implies dedication to intellectual rigor. A part of anthropology, archaeology also is one of the historical sciences as that term has been defined to include the simultaneous consideration of nonrecurrent or “contingent” phenomena (as in the evolution of Mammuthus primigenius, the founding of the League of the Iroquois, the Battle of the Little Big Horn) and the “immanent” or recurring and unchanging properties inherent in the physical universe (such as isotopic half-lives, universal constants of erosion, the unidirectionality of time) (see Simpson 1963:121–122 for a concise, classic statement). The enabling assumptions for archaeology as a component of the scientific enterprise are those of any science. They include the existence of a real world independent of observers regardless of how we construe what we apprehend; the conviction that that world is in principle knowable, at least within the limitations of our biological and cultural endowment; that it is sufficiently orderly to have permitted the growth of testable knowledge, knowledge that builds on itself (even by paradigm

Ronald J. Mason ■ Department of Anthropology, Lawrence University, Appleton, WI 54912–0599

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shifts) and facilitates the posing and investigation of questions previously beyond imagining; and that the procedures and findings of all the sciences are ultimately mutually compatible. They cohere.

Coherence—“constilience” in Edward O. Wilson’s terminology (1998)—is the inspiration, motivation, and Holy Grail of science. It is the glue, the gravitational field attracting and holding together the phenomenological kaleidoscope of the scientific purview. It is mutuality of logic, back and forth intelligibility, adherence to canons of evidence, and the search for an orderly whole by the reduction of seeming disparity to the fewest number of general principles. It is demanding in its rigor and suspicious of unsupported assertions. But such commitment should not be confused with a claim to omniscience. Science, especially in its historical branches, cannot afford the hubris of consigning to irrelevance all that is not immediately resolvable into its own terms. Distinguishing fact from fiction is seldom simple where it counts. The often fine line separating the two can be a taut line that trips the unwary. While this should not discourage venturing into foreign territory, it is simply prudent to watch where and how one steps. Especially is this so given some of the novel challenges archaeology now faces. The ancient compound question “What do we know and how do we know it?” has renewed salience today with the intrusion, invited or otherwise, of native and surrogate voices in the conservation and “construction” of history. Previously muffled voices are now amplified and command attention, and academic purveyors of “the Western way of knowing” stand accused of ethnocentrism.

Although often used interchangeably, in this essay, following Jan Vansina (1985), oral history refers to the memories and recollections of the individuals who experienced or witnessed in their own lives the events they relate; its maximum time depth is thus that of the oldest surviving narrator in the relevant community. Oral traditions extend back beyond living memory and are believed by their narrators to be more or less faithful renderings of the older happenings to which they refer. Thus, they are not regarded as fictions or fairy tales. This essay examines both the ways native oral traditions have been used by archaeologists and how their future role vis-à-vis archaeology is being currently discussed. Except as aboriginal knowledge-claims discordantly intrude where they do not belong, they are irrelevant to what follows. All people are or ought to be free to believe or disbelieve what they will. Centrally pertinent to this discussion, however, and fair subject for rebuttal is the promulgation by some Indian commentators and their allies in and outside academia of the idea that all knowledge claims deserve equal status. Encouraged by politically interested but intellectually extrinsic agencies, such thoroughgoing relativists logically link the doctrine of intrinsic equivalence of culturally relative “ways of knowing” to appeals for their inclusion in the universalist paradigm of science, assuming, that is, they do not simply reject the latter entirely.

Oral tradition is the facies of “traditional knowledge” concerned with the preservation (“reproduction”), presentation, and explanation of events in now (as of the time of telling) extinct generations. In preliterate societies it is taken as testimony of cultural tenure, a transmittal of the essentiality of identity in a people’s journey from ancestral to contemporary times. Except where captured in writing, as in journals of exploration or ethnographers’ notes, it exists only in living memories and is passed on verbally with or without supporting ceremony or other props.

Among many Native Americans, oral tradition is respected not only as a legitimate repository of knowledge about the past but, especially when entwined with sacred or religious beliefs, as is more often than not the case, as the sole, genuine, and invariant source. So considered, it is not surprising that, whatever uses oral traditions may have in aiding archaeologists in their work, archaeology is of little or even no relevance to Indian adherents of traditional knowledge. As asserted by Roger Anyon et al., “Archaeologists are interested in learning about the past. Native Americans are interested in maintaining the cultural traditions they inherited from their ancestors who lived in the past” (1997:78, 83–84). As those authors also point out, it is not invariably the case that all or even most Indians see no value in archaeology, but rather that archaeology and oral traditions are separate categories of knowledge each appropriate in its own cultural context. Each “is simply another way of knowing the past” (1997:84). In this compartmentalization of knowledge claims, oral tradition is more impervious to archaeological implications than vice versa. Archaeology’s essential relevance, it appears, is its practical application in helping with Indian land claims, water rights, NAGPRA repatriation cases and, less importantly—unless directly supportive of those
concerns—in corroboration of oral tradition (see Swidler et al. 1997).

A Red Herring

Requiring immediate discard if real issues are to be addressed in discussing the relations of oral tradition, traditional knowledge, and archaeology is the false but astonishingly durable claim of only quasi-translatability among languages. An example is the following assertion by Anyon et al. (1997:79):

There is really no way to adequately translate and interpret into English, for example, how Zuni observations lead to conclusions about the world. English simply lacks the critical concepts needed to characterize Zuni thought processes in this regard.

This atavistic opinion is flatly contradicted by the testimony of millions of fluent bilinguals and by mountains of linguistic studies that demonstrate that anything that can be said can be said in any language. If a psycho- or sociolinguistic statement is the intention of the authors, that is equally indefensible. The philosopher Nicholas Rescher (1997:30) rebuts this idea:

Anthropologists do often say that a certain society has a conception of rationality that is different from ours. But that is literally nonsense. Those others can no more have a conception of rationality that addresses an object different from ours, than they can have a conception of iron that addresses an object different from ours, or a conception of elephants that addresses objects different from ours. If they are to conceive of those particular things at all, then their conception must substantially accord with ours. Iron objects are by definition what we take them to be; “elephant” is our word and elephant our conception. If you are not talking about that, then you are not talking about elephants at all. You have simply “changed the subject,” and exited from the domain of the discussion (his italics).

Perceptions of Historical Veracity in Oral Traditions

Evaluations of the historical veracity of folk or oral traditions vary, of course, among archaeologists, ethnologists, folklorists, and historians from high trust to suspicion and even dismissal. At the high end of the spectrum of regard, oral tradition is granted at least equality with documentary evidence or ethnographic or archaeological data, however high or low these may be thought to rate. A few specific examples of such esteem may be found in Dongoske et al. (1997), Lurie (1960:802–804), Radin (1923), Schmidt and Patterson (1995), and Swidler et al. (1997). Its polar opposite relegates the historical content of oral tradition to irrelevance if not worthlessness (e.g., Lowie 1915, 1917; Trigger 1976:19–20; Tuchman 1996 [but with salient exceptions, mainly respecting oral history as distinct from “tradition”]). Most scholarly opinion is distributed somewhere between these opposites, less often revealed in statements of explicit allegiance to a general theoretical program than expediently injected in the context of a specific problem (e.g., Brumbaugh and Jarvenpa 1990; Clifton 1998:30; de Laguna 1958, 1972:24–29, 286–288; Hickerson 1988:32–33; Kehoe and Kehoe 1959; Pendergast and Meighan 1959; Salzer 1993; Swanton 1915). Typical of this latter genre is an openness to oral traditions as employable independent historical data, but with cautionary reservations about how they should be used.

Although not focused on North American problems, excellent reviews of some of the recurring issues raised in attributing historicity to oral traditions and histories have been published by David Dunaway and Willa Baum (1996 [esp. Lynwood Montell’s essay]); Jack Goody and Ian Watt (1968); David Henige (1982); Gregory Nagy (1996); and Jan Vansina (1985).

Reasons for Using Oral Traditions in Archaeology: Précis

The literature on the uses of oral traditions in archaeological reconstructions of the past commonly offer one or more versions of one to seven justifying arguments. While no single publication includes them all, most make reference to more than one as warrant for whatever is undertaken. That two of these species of justification may reasonably be considered but special cases or subsets of another will be apparent. However, their repeated citing as if they were independent of and additional to other reasons has prompted their separate listing below. This catalog is distilled from many sources without, it is hoped, caricature or over-simplification. It does not include such intellectually extraneous reasons as stipulations of government agencies and the rhetoric of political, racial, or cultural assertiveness. Some of these, however, will necessarily make an appearance in the following discussion because of their increasing salience in discussions of the nature of the past and how it should be represented or “produced.”
The pertinent pro arguments seem to reduce to the following:

1) Oral tradition is as valid as any other kind of information about the past and deserves equal status.

2) Who should know their own history as well as, let alone better, than those who made it, even if they never wrote it down?

3) Oral traditions, being “emic” phenomena, are extraneously unchallengeable and must be accepted as independent information.

4) Much of the past survives exclusively in the spoken word.

5) Verbal traditions and Western historiography access uniquely different kinds of knowledge and thus address parallel realities, each as valid as the other.

6) Archaeology and oral traditions overlap and supply in their combination a richer view of the past than either offers alone.

7) Archaeology, a product of Western civilization, is ethnocentric and must be balanced if not replaced by “alternative histories.”

Reasons for Not Using Oral Traditions in Archaeology: Précis

The case against employing oral traditions in the practice of archaeology weakens if it does not invalidate each of the foregoing arguments. Additionally, less simply reactive disapprobation appears in one or another version of the following four contentions, either alone or in some combination. These distill into the following:

1) Dependent on memory and verbal transmission, oral traditions are simply not trustworthy.

2) The genre by its nature is more an artifact of contemporary culture than a record of the past.

3) Oral traditions are closed belief systems, beholden to authority and impervious to external challenge.

4) All or parts of oral traditions may be considered sacred, only partly or not at all accessible to outsiders; guardians of such lore determine what may be released and how it may be used.

Discussion of the Pro Arguments

Pro Argument #1. Oral Tradition Is Just as Valid as Any Other Kind of Information about the Past and Deserves Equal Status.

Pro Argument #2. Who Should Know Their Own History as Well as, Let Alone Better than, Those Who Made It, Even If They Never Wrote It Down?

These two arguments supporting the use of oral tradition are intimately linked, often being conflated in the same statement even though they are mutually contradictory. (Different histories, as histories, are equally valid or they are not.) When students of culture history endorse either or both of these views, they have not thoroughly considered their implications, have ignored them and are untroubled by logical incompatibility, or are simply exercising a political but empty concession of noblesse oblige. The latter, apt to be more consciously committed, is the more serious, being disingenuous with those who may hold those propositions honestly. It is certainly the case that either or both arguments are taken seriously by many American Indians, including some who work as archaeologists, much of the general public, as well as by contributors to the historiography of the “anti-colonial” and “anti-neocolonial libraries” (e.g., Martin 1987; Schmidt and Patterson 1995; Swidler et al. 1997). The declaration of at least equal validity of oral and scientifically based histories, while usually qualified by context or amendment, also has been baldly endorsed without modifications, as the following examples show.

Roger Anyon and his coauthors give clear support to these two related propositions, decrying any implication “... that oral traditions are less valid than scientifically based knowledge” (1997:84). Maintaining that “oral traditions contain cultural information about the past [including what they call ‘real history’], carefully preserved and handed down from generation to generation within a tribe” (Anyon et al. 1997:78), they conclude that each kind of knowledge (scientific archaeology and oral tradition) “... is simply another way of knowing the past.” However, expounding one variation on a theme common in the writings of proponents of this position, they introduce the not un-important qualifier “... oral traditions can transcend scientific knowledge with respect to cultural heritage” (Anyon et al. 1997:84). The equal validity of these two “ways of knowing” is thus compromised and turns out to be less than universal, with science occupying the inferior position. Within whatever limits may sometimes be set
on epistemological equality, as in the previous quotation, the seriousness of adherence to the fundamental postulate is difficult to mistake. This is even (especially?) true where, through error or misrepresentation, supporters of this view claim to find approbation where critical examination of contending views is in fact intended (Ferguson et al. 1997:250, misquoting Stephen Shennan 1989:2 [1994:2]).

One cannot object, obviously, to the special privilege of transcendence granted oral tradition over scientific knowledge by Anyon and his coauthors as long as “cultural heritage” remains the referential context. Analogously, Biblical literalists cannot be expected to reject their commitment in order to accommodate a discordant scientific worldview. But yielding to the “cultural heritage” qualifier makes nonsense of those same authors’ outrage that archaeologists do not treat the two kinds of knowledge equally and do not consider oral tradition an integral component of scientific history, presumably on something like an equal footing with stratigraphy or radiocarbon dating. These authors collided with a logical brick wall and didn’t know it, although other wreckage at the same place should have warned them.

Either a trifle more cautious or simply more confused, some of the contributors to the volume The American Indian and the Problem of History (Martin 1987) hit the same obstacle. Thus, Calvin Martin, while seeming to argue, albeitopaquely, for the desirability of combining Indian and “white” (Euroamerican, Western) versions of Indian history, dams the latter for intellectual imperialism (the Western scholar “... colonizes the Indian’s mind, like a virus commandeering the cell’s genetic machinery” [Martin 1987:6]) and builds a wall into which he then runs:

... the two core philosophies [of Indian and Western history and almost everything else] in operation here, the biological [Indian!] and anthropological [“white”!], are fundamentally antagonistic and irreconcilable, at least as presently practiced. There are two very different cosmic errands being carried out here (Martin 1987:9; my brackets).

Some of the same enthusiasm for incompatibilities is exhibited by Kurt Dongoske and his coauthors (1997). They claim that somehow (apparently because “it is religious leaders who maintain this type of information”) the modern Hopi and Zuni tribes know that they may “... claim cultural affiliation with the Mogollon, Hohokam, Salado, Fremont, and other archaeological cultures” (Dongoske et al. 1997:605) and that “... Zuni and Hopi ancestors in the ancient past... traversed, lived in, and buried their dead throughout almost all of present-day New Mexico, Arizona, and portions of Colorado, Utah, and Nevada” (1997:606). Furthermore, “much of what defines cultural or ethnic identity is contained within the history of the members of that [a Native American] culture, and members of the tribes are in a good position to identify the traits that are used for self-identification” (1997:606).

This last statement is one of many to be found in the literature linking the autonomy of and high confidence in oral tradition to the unique value to be seen in the self-knowledge invoked in the second argument. There is intuitive appeal in the notion that no one can know the history of a people better than the people who made it (of course, it was the ancestors of the people who claim to know their own history who made it; the claimants didn’t).

Three quarters of a century ago Robert H. Lowie, invoking a distinction between history and stories about the past, dismissed this claim, concluding:

... I have a very strong suspicion that lurking behind the readiness to accept primitive for real history is the naive unconscious assumption that somehow it is no more than fair to suppose that people know best about themselves. This assumption, of course, need only be brought up into consciousness to stand revealed in its monstrous nakedness. The psychologist does not ask his victim [sic] for his reaction-time, but subjects him to experimental conditions that render the required determination possible. ... How can the historian beguile himself into the belief that he need only question the natives of a tribe to get at its history? (Lowie 1917:163).

Nevertheless, to repeat a crucial point made earlier, a sensitive issue is ethnocentric sovereignty: “we” Zunis, Cheyennes, Mohawks, whomsoever, by right of heritage are in a position to know more about ourselves and the history that produced us than can be known to or be legitimately substituted for by outsiders. Thus, Russell Handsman and Trudie Richmond, championing “... everyone committed to building more open, culturally democratic communities,” seek to construct “counterhistories” that are
much more sympathetic to oral traditions than has been the case with "colonialist practices" (practices that are said to include most contemporary archaeology) that "...continue to marginalize and misrepresent the lives and experiences of Indian people" (1995:90–91). Archaeologists must, in the words of some others, "respect the values of oral traditions in ways that do not demean either [its or scientific] approach[es] to understanding the past" (Anyon et al. 1997:85).

Of course, people are entitled to their beliefs and ought not be belittled for them. But just as those raised in and participating in a social world ordered by a pre-scientific metaphysics are not to be required to jettison their beliefs for those of scientists, unless they presume to work at science, so are the latter relieved of analogous constraints. When incompatible statements are made or positions taken on the basis of those quite disparate "ways of knowing," mutual respect and tolerance are simply humane accommodations. But this does not license importing folk beliefs into scientific discourse any more than mandating its reverse. Such mutual proscriptions do not thereby imply epistemological equivalence in either the reliability of factual claims or the means by which they are attained, but simply respect for the inviolability of what some would liken to the rules of different games. But seeking the past is more than games or gamesmanship, otherwise who would care?

Pro Argument #3. Oral Traditions, Being "Emic" Phenomena, Are Extraneously Unchallengeable and Must Be Accepted as Independent Information.

The "emic" perspective, of course, proffers the "insider's" view: presumably history as it was lived by its participants, not as viewed by outside commentators. But as already noted, except for the immediate past, the province of what we are calling oral history, all of the oral traditions perpetuated in American Indian societies are in fact contemporary commentaries on ancestral actions. It is not the ancestors who speak but their progeny. Nevertheless, proponents of this third argument claim unique insights derivable only through enculturation in and acceptance of the values and belief system of the particular society whose history is at issue. That history is unassailable except within the historiography that produced it, being a subjective, in the sense of socially introspective or intra-societally private production, vs. an objective or inter-societally, publicly accessible, knowledge claim. The autonomy of such histories is usually taken on culturally relativistic grounds as an ethnographic given, regardless of whatever evidence may be adduced of alteration through generational transmission or via diffusion of elements from other societies. As a self-portrait of a particular society's history, the received account must be evaluated on its own terms; it is no more to be challenged than is that society's kinship system or cosmology. Students of culture have to accept what they find, whatever their personal disbelief, and try to understand what they have learned both as a unique phenomenon and as an instance of a class of similar phenomena. That the people being studied may not agree with whatever interpretations or conclusions are reached in no way commits either them to change their ways or the observer to apologize for the sense made of his or her observations.

Proclamations of independence of oral tradition and the nature of its relationship to archaeology are straightforwardly promulgated in a number of the essays published in the influential volume Native Americans and Archaeologists (Swidler et al. 1997). A few excerpts from one of the more trenchant of those essays, the earlier cited work of Roger Anyon et al. (1997), exemplify this point of view:

... the utility of archaeology to enhance Native American oral tradition in traditional cultural contexts is limited and often irrelevant (p. 78).

While oral tradition can be very illuminating for archaeologists, most archaeology has little meaning to Indians as a way to enhance oral tradition itself within a traditional cultural context (p. 80).

[An archaeological site in New Mexico], ...is also illustrative of the relevance of oral tradition to archaeology but the lack of relevance of archaeology to the oral tradition itself (p. 81).

The Zuni, however, have made no use of the archaeology to corroborate their oral traditions. It simply is not necessary because archaeology has no relevance in this aspect of Zuni oral tradition (p. 82).

In the present world of governmentally funded archaeological practice, the ground-shifting perspective reflected in declarations like those just cited was translated into operational imperatives when the Native American Graves Protection and Repatriation
Act of 1990 (NAGPRA) came into existence. However, being the product of a political act, not the result of scientific logic, that legislation, however puissant it proves to be, is irrelevant to the epistemological focus of the present essay.

But returning to the argument of oral tradition autonomy, including also the frequently inferred or even explicitly expressed addendum asserting relevance to scientific culture history, the problem of connecting the two becomes salient. Leaving aside gratuitous remarks about the racism of the American academic community and the charge that its members recoil from dealing with complexity, Roger Echo-Hawk makes this point in his chapter in Swidler et al. (1997). How and to what degree autochthonous oral traditions can or should be conflated with archaeological data is an issue revisited in the following sections of this exercise. The necessary corollary of the thesis of oral tradition autonomy is the same status for scientific history, whether based on documentary, archaeological, ethnological, linguistic, or biological data. This corollary authorizes, indeed mandates, the scientist to employ oral traditions only if, when, and as they seem useful and are compatible with the tenets of scientific historiography.

Pro Argument #4. Much of the Past Survives Exclusively in the Spoken Word.

In many places, for many times, for many kinds of questions, oral tradition is all there is. Is it to be ignored because it stands alone? Yes, was Robert Lowie’s forthright answer to that question when he declared, much to the objections of Roland B. Dixon (1915) and John R. Swanton (1915), “...I cannot attach to oral traditions any historical value whatsoever under any conditions whatsoever” (Lowie 1915:598). At the end of his essay, he retreated just sufficiently to state:

The utmost I am able to concede is that a tradition referring to the remote past may furnish a starting-point for linguistic, archeological, or other investigations; but our knowledge of native history will in the end depend wholly on the result of these inquiries (Lowie 1915:599).

That for much of antiquity oral traditions are all we have is self-evident. The statement is inarguable. With writing absent north of Mesoamerica until introduced by Europeans, with even a rich archaeological record dumb in prodigious dimensions, and with the historical insights and implications of linguistic, ethnographic, and physical anthropological evidence always limited and often ambiguous, how can oral tradition’s position as sometimes the lone gateway to the past be challenged? Indeed, it cannot, and Lowie himself did not. He admitted the obvious and then argued, in effect, “So what?”

We cannot know them [oral traditions] to be true except on the basis of extraneous evidence, and in that case they are superfluous since the linguistic, ethnological, or archeological data suffice to establish the conclusions in question. When linguistic comparison has proved the close affinity of Crow and Hidatsa, the tradition of a common origin shared by the tribes speaking those languages does not lend to the result one iota of additional certainty. Where “other data” are lacking, the use of the oral traditions for historical reconstruction must be discountenanced as a matter of obvious methodological caution. We cannot safely reject as mythical that part of a tradition which conflicts with our conception of physical possibility and retain the remainder as correct (Lowie 1915:598).

While folk or oral traditions must be acknowledged as all there is to represent important pieces of the past, whether reliably or not, they command respect and attention as human documents. Furthermore, as ethnographic data they reveal invaluable insights into aspects of a people’s psychology, values, social structure, cosmology, etc. But Lowie’s uncompromising stance with respect to their status as history (in the modern Western sense of that term) is difficult to refute, however indelicate—indeed offensive—it may appear to present-day sensitivities. His 1917 paper in The Journal of American Folk-Lore is a commanding piece of work and deserves to be read by contemporary students of folklore, history, and anthropology. In it he asked and answered this question:

If we do not accept aboriginal pathology as contributions to our pathology, if we do not accept aboriginal astronomy, biology, or physics, why should we place primitive history alone on a quite exceptional pedestal, and exalt it to a rank co-ordinate with that of our own historical science? (Lowie 1917:162)

But still, we might ask, in the absence of anything else, may we not provisionally accept all or parts of oral traditions until such time—if ever—as they are refuted by a more compelling type of testimony? And if more than one tradition tells the same tale, does not that agreement confer authenticity to the
account and, by extension, to the genre itself? Responding to these queries raises others. How detailed or vague are the “histories” with respect to actors, roles, story line, temporal perspective, place? To what degree can the independence of seemingly corroborating traditions be established? And how? How matter-of-fact as opposed to symbolically embellished are they? These are knotty questions, of course, the answers to which are contingent on the particular cases. Even willingness to consider such questions invites consideration of the theoretical admissibility of oral traditions as potential sources of information for scientific studies. However, even a historiographer more open to the possibility than Lowie has had to conclude that

... one of the historian’s most difficult problems is that oral evidence changes imperceptibly as time passes. ... a principle of natural selection tends to operate, by which those traditions that are best able to outlive changing circumstances are those that exist today. But, as with plants and animals, surviving requires that they adapt to whatever changes they encounter. However reluctantly, we must assume that many contemporary versions of traditions are to some extent the debris of an obliterated past, the result of its mental landscape being repeatedly exposed to weathering, its shapes deposited in secondary patterns and shifting with the wind. Inevitably, many traditions cannot be regarded as historical fact. Accepting this will be hardest when there is nowhere else to turn, but this is a pity rather than an argument (Henige 1982:4–5).

I might well have quoted Bahr (1998a), Goody and Watt (1968), Montell (1996), or Vansina (1985), among others, who have employed oral traditions in their own research, to the same effect. A large component of the problem reduces to the question “Which oral history? Whose version?” In James A. Clifton’s pithy observation, “Today’s tradition often enough is last year’s novelty” (1998:30).

Pro Argument #5. Verbal Traditions and Western Historiography Access Uniquely Different Kinds of Knowledge and Thus Address Parallel Realities, Each as Valid as the Other.

At face value, this could be taken as such a declaration of incommensurability as to vitiate any meaningful dialog between proponents of the two epistemologies. Attempts at communication, even if initiated, would immediately fail for lack of mutual intelligibility. This dire conclusion is weakened, however, by the fact that the argument seems to be deployed more as strategy than principle and is more honored in the breach than in practice. Taken as less uncompromising than presented, at least one chink in the armor of disputation is discernible.

Whether strictly or loosely adhered to, pro argument #5 may be seen as a re-worded composite of the first four affirmative arguments, especially of #3 which grapples with the issue of the “emic” perspective. It is indeed that, but it also appears to capture a further nuance, even an additional meaning, that I think is easier to detect in their combination than in any one of those previous four arguments considered separately. That added meaning, if I am detecting it aright, is not unique to Native American or any other oral tradition but is familiar in Western historiography as well. It emerges in Roger Echo Hawk’s caution on oral tradition “historicity” (see below); in Dorothy Lippert’s inquiry, “but isn’t there a sense of wanting to connect with the ancient people that drew all of us into this [archaeological] profession?” and her lament, “It seems to me to be dangerous to define a past that does not possess a human soul” (1997:123); in Patricia O’Brien’s complaint that “Too many studies not only do not deal with women in the archeological record, they do not deal with men nor even with people” (1990:62, her italics); Lynwood Montell’s sympathetic recording of cultural historians and folklorists expressing concern about “jeopardizing consideration of the human element in history” (1996:180); Joan Gero’s self-answering question “Are we [archaeologists] really trying to reduce...the amount of prehistoric human consciousness that accounts for what we observe, eager to eliminate human agency altogether, to get an automatic correspondence between actions carried out in prehistoric time and physical evidence?” (1990:115–116); and, to implicate myself as well, in my own sometime glimpse of ghosts amidst the debris of extinct communities even though I think I know where they properly reside (e.g., Mason 1981:109).

While the authors I have quoted were addressing a variety of problems, I do not believe I do them an injustice by linking their words to a common, but not commonly voiced, sense of a lingering deficiency in what it is those of us engaged in archaeology think we know about the past, a deficiency foreign to oral tradition. True enough, pro argument #5 asserts that
the two “ways of knowing” manifested in oral traditions on one hand and scientific or Western historiography on the other are mutually independent and equally valid pathways to the past. (At this point the objection may be raised that neither of these actually gets at the past—that we really have no way of getting on to that braided or multi-strand trail followed by the people themselves who lived in the past and experienced it as the present. There is no way to resurrect the engaged immediacy of what they were about, no matter the pretensions of archaeologists or even lineal descendants to “speak” for extinct generations.) The “two [accessible] ways of knowing,” if we accept the dichotomy, would seem to unlock different doors, each opening onto disparate “realities”—unique ontological realms requiring non-interchangeable intellectual (and emotional) equipment to explore.

Part of the “equipment” required to negotiate the realm of a sovereign oral tradition, but prohibited in the scientific, is religious belief:

. . . scholars are hesitant to defer to Indian experts on oral traditions since most such experts are religious leaders who emphasize the spiritual aspects of oral traditions and who typically see academic analysis as inappropriate. A religious approach accepts oral texts as the source of holistic truths rather than as documents that require evaluation for historicity, . . . (Echo-Hawk 1997:89).

It would appear that for many adherents of traditional knowledge the scientific proscription of religious beliefs in reconstructions of cultural history is a fundamental flaw. While in secular discourse, as just seen, the flaw resides in the seeming absence of human immediacy. Whatever their differences, both perspectives recoil before pictures of the past that seem to show abstract, reified cultures and naked environmental forces mechanically grinding out histories, with people almost a byproduct. With such concerns, elucidating the past with the canons of scientific methodology alone can be seen as hitting the target but missing the point. What seems to some people as a denial of human agency in history is a highly debatable charge, of course, but it is not one to be lightly dismissed. For people who have long taken their history from traditionalist narrators, the threat to venerable certainties and the self-identifications they support understandably invite defensive reactions. This must be especially so when the people affected perceive the new dispensation as a “take no prisoners” usurpation of received wisdom by an abrasively foreign creed, one that disallows the recalcitrant into its mysteries and demands that native potential initiates be intellectually born again. One does not have to accept oral traditions, nor be religiously inclined, to empathize with those in this predicament. And the scientist may empathize, too, perhaps having experienced in his or her own life the same or similar dilemma. This is the fate all must risk who would “live the examined life.” And those who choose it must not recoil from the consequences of such an examination. Nothing is gained but that something is lost.

Still, there lingers a sense widely shared by scientists and non-scientists alike that more is present at an archaeological site than physical remains. Put another way, many people bring more than clinical curiosity to such places. Anyone who has looked has seen it among museum-goers and visitors to historical and archaeological sites: a desire, even a need, to see with one’s own eyes what has previously been known only vicariously and, moreover, if possible, to physically touch. As a Civil War buff, I reacted more than intellectually when I first saw the Gettysburg battlefield. Emotions are engaged as well as the intellect in encounters with the past, and cannot be totally divorced. But whereas they need not be by those whose principal interest is “. . . in maintaining the cultural traditions they inherited from their ancestors . . .” (Anyon et al. 1997:83–84), it is the duty of scientists, as scientists, to disentangle as rigorously as possible their effects. However much emotion is spur to motivation and a reward of research, it is a potentially treacherous companion in the exercise of reason.

Intellectually, everybody knows that people now faceless and nameless once inhabited the places identified as archaeological sites and that those former inhabitants once had been real flesh-and-blood men and women and children just like themselves, however different in particularities. But more compelling than this is the feeling that the former inhabitants somehow linger in their works to remind visitors of the indispensability of people, however much the strangers focus on potsherds, biface-uniface ratios, sociocultural systematics, law-like generalizations, and so on. It is a mystical feeling, undoubtedly, but one not in opposition to or in defiance of a commitment to rationality. This is knowledge with an emotional impact, with an immediacy
that thrills and humbles. Although hesitantly publicly voiced, it is, I think, a not uncommon, if fleeting, part of archaeological experience. These were not, after all, amoebas who built the ruins and dropped the detritus moderns now contemplate, nor were they space aliens. They were our kind, our species, of our evolutionary lineage. We, the visitors, reach out hoping to touch them and, believing that in some sense we have, we discover our commonality. We can look into the bony faces of unearthed skeletons and, like Hamlet, see our own mortality. Scientist, humanist, butcher, baker, microchip maker, we can see and come away from such a site anywhere in the world and know that we have had an encounter with our own, more ancient selves.

The claim of mutual independence of two “ways of knowing” about the past, one reliant on verbal traditions and faith, the other on empiricism and logical argument, is a fact. The further requisition of different “realities” correspondingly unlocked by those methods is another matter, one dependant on how “realities” is to be defined, whether simply as artifacts generated out of antecedent “knowing” or as that which precedes, is independent of, and is revealed by the accretional lessons of learning how to know. In the first case, argument is pointless because communication is impossible. The second scenario is precluded by contradiction (there can be, by definition, but one reality). What remains are knowledge claims, of which only one of the two sets invites external verification or discrediting.

This is a simplified rendition of a more complicated situation, of course. There is a good bit of empiricism and logical thinking in oral traditional history, and the realm of scientific archaeology and historiography supplies ample testimony to the universal human capacity to live with compartmentalized contradictions and even more comfortably with ambiguity. There is leakage between these modalities, and each is not as impermeable as might at first seem. The genuine contrasts are in degrees of potential and actual openness to correction by appeal not to authority but to evidence and rigorous argument. If little of the latter is allowed, then both “histories” must go their own ways, never to converge. And that would be a pity. If testing is not proscribed, then the ground is prepared for the next affirmative argument.

Pro Argument #6. Archaeology and Oral Traditions Overlap and Supply in Their Combination a Richer View of the Past than Either Offers Alone.

This thesis differs from the foregoing in positing enough common ground to make each historiography potentially relevant to the other. It thereby relaxes, if ever so slightly, the strictures of thoroughgoing cultural relativism, the armor of oral tradition under the terms of pro arguments #3 and #5. It now admits some vulnerability of traditional history to external critique and offers the hope of richer views of the past through collaboration rather than intrinsigent co-existence. Still, that collaboration involves traffic along a predominantly one-way street.

The inequality of the potential mutual relevance alluded to above is of a decidedly different nature, of course, as viewed from the two sides. The archaeologist, trained in the philosophy and procedures of science, is obligated to follow where empirical data, logical thinking, and ever-evolving questioning seem to lead. The same is not true of the guardians and transmitters of traditional history, where, a priori, what has been accepted as truth from generation to generation is largely immune from critical questioning, though not from change. The fact that science in the form of archaeology and physical anthropology is occasionally conceded the ability, minimally and peripherally, to add new information to what oral tradition has previously offered its adherents is inescapably an admission of some inadequacies, though not necessarily errors, in received wisdom. In short, many American Indian spokesmen for oral traditions now accept that scientific evidence can be useful in adding information where traditional knowledge is either silent or safely ambiguous. Public acknowledgment that oral traditions are open to correction through scientific evidence, however, is extremely rare. The prevailing view is as quoted below.

Echo-Hawk [in his chapter in the same book from which this quotation is taken] is right when he says that the successful integration of oral traditions with information from archaeology and physical anthropology has great potential for reshaping the academic construction of ancient human history. From a scholarly perspective, the combination of human osteology, archaeology, and Native American oral tradi-
tions will yield a more complete understanding of the past than can be gained by using any one of those sources by itself (Ferguson et al. 1997:241, italics mine). (See also the congruous quotations previously listed under pro argument #3.)

Whereas genuine reciprocity in the acceptance of information between the two kinds of histories remains the exception rather than the rule, interest in using oral traditions as independent sources of data bearing on the past is lively in contemporary archaeology and anthropology and has a long tenure (for some early perspectives, see the exchange among Lowie (1915, 1917), Swanton (1915), Dixon (1915), and Goldenweiser (1915). As indicated in a preceding part of this essay, that interest runs the gamut from consideration but rejection through lip service to serious and sustained commitment.

Probably a hefty majority of archaeologists today would be willing to go at least as far as Hickerson (1988:32–33) or Clifton (1998:30) in crediting “memory ethnography,” “migration legends,” and “traditional history” as having some potential historical usefulness—more so as they relate to relatively recent time—and if used with caution. Such accounts are even more acceptable, of course, if compatible with other independent data. In fact, many scholars are prepared to grant credence to a considerably wider applicability than that endorsed in this “minimalist” position. That wider applicability may embrace not only a willingness to extend the time depth into which, it is claimed, oral tradition can throw light, it can add detail and facilitate a fuller, richer, and more humane appreciation of the past than is possible by use of conventional scientific methods alone. “Easy” examples, that is, those unlikely to be challenged without fairly substantial contradictory evidence, range from interviews with old native informants who plausibly claimed first-hand knowledge of the origin and meaning of certain boulder effigies or Petroforms made by Blackfoot Indians in the late nineteenth century and, by extension, second-hand accounts of similar features attributed to the Crow, Mandan, and Sioux peoples in the same century—information otherwise simply unobtainable (Kehoe and Kehoe 1959), to the use of Cree and Chipewyan archaeological field crew members who had actually built or lived in the camps or other sites ethnoarchaeologists were studying, and were thus able to supply researchers with sometimes wholly unanticipated classes of information, information sufficiently different from what archaeologists have been trained to expect as to cause them to wonder “... what was the real ‘folklore,’ our informants’ concept of the world or our anthropological vision of reality?” (Brumbach and Jarvenpa 1990:41). Other examples of a compatible nature include archaeologists calling on native informants, either directly or via records of oral accounts in ethnographies or historical documents, for what they can say about times equally recent or as remote as several centuries ago, assays of reliability normally decaying as temporal claims increase. Such exercises may provide the archaeologist with local place names, information pertinent to site functions (memorials, for instance, or shrines not recognizable from physical traces), names and attributes of historical or mythical personalities and deities associated in native consciousness with particular places, recitations of mundane as well as fabulous events not documented in writing, helpful suggestions regarding puzzling site features, possible stipulations of certain artifact functions and meanings not otherwise likely to be knowable to archaeologists, and attributions of things or places to specific clans, medicine sodalities, or ceremonies (see the essays in Swidler et al. 1997 for representative cases). A reasonable expectation should be that particular instances of any of these putatively expert attestations may diverge radically from others in difficulty of assimilation into the paradigm of archaeological inquiry. It is incumbent on the researcher, of course, to receive these solicited or proffered opinions with a willingness to learn from them, but also with polite caution and a determination to distill from them only and whatever is susceptible to independent verification or falsification.

As oral traditions claim to descend from more remote generations or to issue from a timeless past, they correspondingly relinquish much or even all of whatever historical value researchers may be inclined to grant other members of the genre reflective of the recent past. Historical memory inexcapably erodes over time and its credibility becomes so degraded that the feasibility of meaningful testing is attenuated to the vanishing point. What archaeologists and historians continue to debate are the rate and degree of erosion and attenuation. Probably all students have their own scales beyond which they are unwilling to go. Even so committed a reconciler of the two gen-
res as J. M. Levi (1988) retains the distinction between myth and history.

Consideration of oral tradition allusions to temporally remote—even prehistoric—times is a daunting and even desirable task, provided its undertaking does no violence to the evidential demands of modern historiographic discipline. No one embarked on this type of project will seriously consider as historical data the patently fabulous: claims of New World genesis, for example, or underground tribal origins or, in a refreshing change, at least for me, the absolute assurance I once received from a self-identified British Columbian tribal historian that her people retain detailed “memories” of their ancestors crossing the Bering Straits. Indeed, something of equal incredibility has recently been published in an academic journal and endorsed as genuine, if “messy,” history by a student of Tsimshian oral traditions who purports to find evidence of “precision” in the recitations of adaxw (“epic”) stories asserted to collectively span at least 10,000 years (Miller 1998). An “ethno-ethnology,” as the author calls his work, surrenders its critical invulnerability when offered as something else additional to its intrinsic nature and when random odds and ends of decontextualized geological and archaeological data are preferred as corroboration. Nevertheless, serious studies laying claim to impressive time depths, that is, before five or six generations ago, do come readily to mind. The following selections have been chosen for critical examination because they are among the most frequently cited and influential examples of the genre.

Ice Movements at Yakutat Bay, Alaska

Frederica de Laguna (1958) has proposed the concordance of Icy Bay-Yakutat Bay (northwestern Alaska panhandle) aboriginal traditions and radiocarbon dated glaciological events extending as far back as approximately A.D. 1400. The concordance emerges in legends de Laguna collected of episodes of glacial movements blocking and unblocking those bays and opening new land for settlement, her estimate of the likely age of those stories, and the radiocarbon-dated geological evidence of ice movements in the area. This often-referenced paper unfortunately lacks supporting information about the dating of the legends necessary for its critical evaluation. This is likewise the case for the extended account later published in her much broader study of the history and culture of the Yakutat Tlingit (de Laguna 1972:24–29, 286–288). She has since pointed out to me that her 1958 article was an incidental by-product of her larger research project (personal communication). At issue here is how the estimation of the ages of the legendary events was arrived at. How independent of the geological/radiocarbon study was that estimation?

The first of the two relevant radiocarbon dates was processed just two years after de Laguna’s final field study and four years after her first, the geological investigations that procured the samples for dating overlapping with hers. Given the geologists’ desire to inform her of their results, it is evident they had been in communication regarding their separate research programs. The file report on the geology is dated 1957, only a year before de Laguna’s publication. One would like to know how definitive de Laguna thought her time estimates to be before the geological studies were completed and to what extent—if at all—she was retrospectively influenced, before finishing her own paper for publication, by the results of the then still youthful but prestigious authority of the radiocarbon method. These are questions that suggest some caution before accepting at face value the suggested congruence of a folkloric relative sequence and an independent radiometric chronology.

The Ancient Mukwitch in Utah

A much-cited study purporting to demonstrate the tenacity and reliability of oral traditions extending back at least a further 350 years to before ca. A.D. 1150 concerns modern Paiute Indians in southern Utah. David M. Pendergast and Clement W. Meighan (1959), while excavating the Paragonah site in Utah, report being impressed by the compatibility of contemporary Southern Paiute beliefs with archaeological evidence of ancient incursions by Puebloan people into that part of the Great Basin, people the Paiutes called Mukwitch. But the authors’ impressibility is difficult to understand given the general character of the alleged parallels (presence of pottery, metates, pit houses, surface storage structures) and their dismissal without comment of commingled error (the Mukwitch grew no corn—only one informant thought they did, they had knowledge of trains, “they knew that white men were coming to their land,” they had no baskets) and the fantastic (some of the Mukwitch turned to stone). A more mundane explanation of the correspondence of Paiute tradi-
tions and the archaeology, and one invoking much less necessity to suspend disbelief, is to credit Paiute society with a less extravagant memory and a greater sensitivity to their contemporary social and physical environments. They had long explored much of the terrain or knew people who had visited where they had not; surficial archaeological sites are commonly encountered and the Paiutes knew the location of many of them; collectors and archaeologists had been at work in southern Utah before Pendergast and Meighan; all but one of the Paiute informants spoke English and had long been in contact with non-Indians; newspaper and magazine stories inevitably diffuse to even the illiterate; and Bernard DeVoto's jaundiced regard for that state notwithstanding, even Utah had and has local radio newscasts.

A Navajo-Hopi Drama

In dramatic contrast to the foregoing allegations of reliable memories surviving from the distant past in Utah are the creative disparities in oral accounts separated by a mere 44 years regarding a known historical event in Arizona (Vansina 1985:19–20). Two versions of the event were published in 1892 and a third in 1936. The first two record firsthand (“oral history”) reminiscences of two Hopi survivors of a Navajo attack made on a party of 10 Hopis as they trekked from Fort Defiance to First Mesa sometime in the period 1853–1856. Among the at least four Hopis killed was the village chief. The Hopis launched a retaliatory raid soon thereafter. Peace was then made between the two tribes when a boundary was established to separate them. Although one of the two oral histories is more detailed than the other, they are both in substantial agreement.

The third version of this story, that published in 1936, is no longer a personal narrative but is a secondhand rendition—an “oral tradition,” the original narrators and participants being dead. As Vansina points out, significant alterations to the “history” were made in the interim. Not least among them is the mythologizing of the hostilities and the validation of the boundary establishment, and the gross exaggeration of the personal qualities and role of one of the original participants. A lot of invention took place in the years between the history and the tradition.

Ridge Ruin: Symbols vs. Narrative “History”

But returning to the previous discussion of Pendergast and Meighan respecting their claims of impressive age for Paiute historical memories, mention must be made of the famous publication to which they appeal for ancillary support. This is the report on the unique late Pueblo II/early Pueblo III burial excavated at the Ridge Ruin site near Flagstaff, Arizona (McGregor 1943). This incredibly rich burial of “a prominent old man of forty” (McGregor 1943:298), dated to the first quarter of the twelfth century, was accompanied by numerous artifacts McGregor took to be of a “ceremonial” nature. Some of these artifacts were shown to several Hopi Indians who thereupon not only agreed with the archaeologist’s “ceremonial” identification but felt that they could specify the particular ceremony they represented, even giving local community cognates for its name. All this despite the gap of eight centuries intervening between the burial’s interment and its excavation and the fact of Hopi agreement that the ceremony in question had ceased to be performed about 50 years previously. McGregor was convinced the Hopis knew what they were talking about when one of their number hazarded a correct guess about the approximate geographical provenience of the site from which the artifacts came. He was further convinced when three of their number successfully (and independently) described other ceremonial items they had not been shown but which they believed should have been found with the burial. These were not simply generic categories like pottery and arrowheads, but accurately described complex things: “a clublike object with serrated edges, a double-horn-like object, and a cap with a point on the top” (McGregor 1943:295–296).

Here, then, is a powerful example of analogical reasoning backed up and verified by successful prediction from an incomplete test sample. It would be difficult to argue that it represents something less than a continuity of symbolic cultural practices, even if specific points of meaning are likely to have undergone some alteration during those eight centuries of the ceremony’s tenure. Like the signification of the fish or the cross in Christian symbolism, the diagnostic attributes critical to the Hopi exegesis of the physical vestiges of a prehistoric ceremony depended for their recognition on the survival of meaning linked to form. While not an example of historical narrative, the interpretation of the Ridge Ruin burial does testify to the longevity of aspects of oral tradition, in this case the conservation of certain symbolic devices. And properly used, that is a tool
not to be neglected in any reconstruction of cultural history.

**Pursuing the Hohokam into History?**

Virtually every textbook dealing with North American Indians or archaeology highlights the Southwestern culture area as the model of cultural conservatism. Particularly in the case of the Pueblo tribes, it is cited as such a paradigm of continuity that archaeologists working here can be more confident than most of those working elsewhere in projecting ethnographically recorded social structures, symbolic representations, religious beliefs, etc. into the archaeological past. Although the degree to which this is true is often exaggerated, the claim is not without merit (e.g., Cordell 1997). Of pertinence in the present instance is how far oral traditions in this area may be credited with genuine historical content. One of the most frequently cited current proponents of historical credibility is Lynn Teague (1989, 1993). She asserts:

An earlier study (Teague 1989) presented a preliminary exploration of this issue. It was found that the oral histories [traditions] can be shown to conform to the archaeological evidence to an extent not easily attributed to the construction of an after-the-fact explanation for the presence of numerous ruins throughout the region. These histories reflect direct knowledge of events in prehistoric Arizona (Teague 1993:436, my brackets and italics).

According to various renderings of O’Odham (Tohono O’Odham or Papago and Akimel O’Odham or Pima) oral traditions (a) they are the descendents of the people responsible for the prehistoric Hohokam culture, (b) they are not so descendent, (c) their ancestors conquered the Hohokam people, (d) they are descended from both the conquered and their conquerors, or (e) no mention of the Hohokam is made at all (Bahr 1971; Teague 1993). References to warfare and conquest are attributes of the northern O’Odham traditions, while they are rare or absent entirely among their southern brethren. As the foregoing suggests, O’Odham oral traditions bearing on their origin and relationship to the prehistoric Hohokam culture are fairly numerous. They also are in whole or in part highly convoluted; mutating in length, consistency, and particulars; sometimes misrepresented due to translation errors (Fewkes 1912:42); and by their nature inviting of competing interpretations. Teague herself has noted that “no internally consistent composite account can be constructed from the recorded variants” (1989:156). (For some representative samples see Bahr 1971, 1998a, 1998b; Fewkes 1912:42–52; Mason 1921; Russell 1908:206–242, 247–248, 272–282, 339–346.)

In addition to the O’Odham accounts, some Hopi clan migration legends hint at Hohokam ancestry. Hopi pottery has been recovered from a number of late Hohokam sites, and Teague (1993:444–452) mentions certain other parallels that could support a connection. These include tales of a destructive flood, sacrifice of children, and overthrowing a priestly hierarchy with resulting social disruption (see below). A 1775 account by the Franciscan missionary Pedro Font cited by Teague (1993:447, see Fewkes 1912:58–61) reports that the Gila River Pimas attributed the Hohokam ruins in their country to the Hopis. The Zunis also have been nominated as participants in the “Hohokam multi-ethnic formation” (Shaoul and Hill 1998:377, 389–390; Teague 1993:447–450). Now determining whatever modern tribe(s) might be biologically or culturally derivable in whole or in part from the prehistoric Hohokam culture is a problem essentially distinct from that of appraising the historicity of the oral traditions, even though the solution of either problem would be useful in addressing the other. This critical distinction needs to be borne in mind.

The missionary’s account mentioned above is one of the chief props for Teague’s contention that oral traditions in the Southwest sometimes reveal a chronological (chronometric, in this case) accuracy that archaeological investigations later proved correct. Father Font, writing in October 1775 with respect to the aforementioned ruins, simply mentioned the suspiciously round figure of “some five hundred years ago, according to the histories and scanty notices of it which exist and are given by the Indians [Pimas]” (quoted in Teague 1993:441; see Fewkes 1912:59). Teague, subtracting that figure from the date of the priest’s writing, notes that the resulting date of A.D. 1275 falls within the range of those ruins, particularly those of Casa Grande, attributable to late (Civano phase) Hohokam. The several centuries of Civano phase occupations probably came to an end sometime before A.D. 1450 (Cordell 1997:200–202). At any rate, it is a virtual certainty that the half-millennium age estimate was that of Father Font, not the Indians, and should not be taken literally. Neither the Pimas nor any other native group
in the Southwest reckoned time in this manner or had the genealogical counts from which such an age estimate might have been extrapolated. Teague’s other arguments for her contention are drawn from certain O’Odham oral traditions and examined in the context of current archaeological information thought to be supportive of them.

The relevant stories are embedded in a world having men who grow feathers and turn into horrible raptors, game balls that impregnate girls and induce the birth of monsters, whole societies subsisting in the bowels of the earth, elites who turn themselves into rainbows or flies and have the ability to darken the landscape by blowing chimney soot from the palm of their hand, and so on. Played out in this world is a drama which, stripped of the fantastic, Teague believes is deserving of being called history (1993:436).

Briefly, the pertinent narrative begins with the adventures of the O’Odham culture hero Elder Brother, aka Montezuma (Mason 1921). This character lived in one of the Hohokam towns where he had a hard time getting along with the power-elite of that hierarchically organized society. He survived several assassination attempts and came back to life after several successful ones. By now thoroughly irritated, Elder Brother set about raising an army, recruiting men from far and wide (including the earth’s interior). In what appears to have been a prehistoric blitzkrieg, Elder Brother and his forces successfully attacked and overwhelmed the Hohokam towns on the Gila and Salt rivers, concentrating on those containing the great houses of prominent chiefs and religious leaders, the controllers of the wind and rain gods. Songs supposedly sung preceding the attacks are reprinted in Teague (1993:441–442), although they are of course impervious to validation or falsification. That aside, the “rulers” of some of the assaulted communities are given names incorporating a term translated as doctor. Teague cites an informant of Julian Hayden, in an unpublished manuscript, who volunteered that all Hohokam villages had a doctor or medicine man (1993:443), a pretty safe but not especially enlightening cross-cultural generalization dependent neither on access to history nor oral tradition. At one of the towns Elder Brother had to contend again, this time successfully, with no less than a fellow deity, one who had opposed him since the creation of the world (Teague 1993:442).

Another alleged sign of historicity in the traditions is the information that some of the Hohokam settlements were protected by okatilla stockades (Teague 1993:442), an architectural detail that would not have survived long enough for the historical Indians to have seen them at the prehistoric sites. Furthermore, their occasional former presence is only sometimes archaeologically inerable through soil discoloration alignments. Suggesting that the prehistoric presence of such simple and useful structures could only be known to the O’Odham people because of the veracity of the traditions ignores the fact of their recurrent construction into recency.

Further evidence of historicity, it is suggested, is the story of a great flood. There are more than one of these, but one in particular is supposed to have been contemporaneous with Elder Brother’s war and the downfall of the Hohokam towns. This flood is said to have followed hard on the heels of a severe drought. In some accounts there was no flood, the sacrifice of a child or children having successfully averted a threatened one (Bahr 1971:248–253; Mason 1921). However, Teague draws attention to regional physical studies (1993:443–444) that have yielded evidence of a drought ended by significant flooding at about A.D. 1358, a time within the age range but not coincident with the end of the Civano phase. Although compatible with some versions of the alleged events, this is in itself inconclusive, considering the competing versions of the tradition and the fact that major floods have recurred from earlier prehistoric time into the modern era.

Potentially the most compelling support for Teague’s endorsement of historical accuracy in some portions of those O’Odham oral traditions she finds credible and indicative of “direct knowledge of events in prehistoric Arizona” is their identification of the Hohokam archaeological sites that are the remains of the towns assaulted by Elder Brother. “The settlements identified in the oral traditions,” she says, “are an archaeologically accurate inventory of the late Classic period platform mound sites along a lengthy segment of the Gila River” (Teague 1993:440)—granting, that is, a willingness to extend the benefit of doubt in certain cases. All 8 of these sites are culturally attributable to the Hohokam Civano phase even though other superficially similar earlier Hohokam sites are in the same areas. There are approximately 18 of the latter in the immediate vicinity of the war route (Teague, personal communication). In the appendix in her 1989 publication,
Teague lists 10 settlements “encountered in Elder Brother’s conquest” (1989:175–176). Summarizing her information, it seems the O’Odham have supplied names for two of those settlements and for five of their leaders, the other five being unknown or controversial. She has indicated as certain or probable the locations of half of the towns, the others being either unknown or uncertain. Dating Elder Brother’s war near or at the end of the Civano phase—no later than A.D. 1450—indicates a gap of a good four centuries between that campaign and the middle of the nineteenth century and the early decades of the twentieth when the oral traditions were recorded. Even with the uncertainties just reviewed, the survival of such detail in purely verbal form over so long a period of time is remarkable. Remarkable, that is, if the story is not purely mythical or is a garbled representation of events from a later time mistakenly attributed to an earlier. Lacking space to consider this further here, it must suffice to indicate that one student of O’Odham oral traditions has in fact suggested two candidates for the latter possibility: the trauma of the Spanish entrada and the later impact of the Ghost Dance (Bahr 1971).

Recent linguistic studies either express opposition to Teague’s historical reading of the O’Odham traditions, at least with respect to their implications for language history (Hale and Harris 1979) or, aside from proposing a northern homeland for the Tepiman language stock, to which Upper Piman languages belong, they are neutral. Shaul and Hill (1998:392) find themselves unable “to distinguish between a scenario that sees these people (or some of them) [O’Odham speakers] as the contemporary descendants of participants in the core Hohokam complex or one in which they entered the core Hohokam area at the time of, or after, the Hohokam collapse.”

It may be that Teague has indeed identified genuine historical content in the O’Odham and Hopi oral traditions and, further, that she has correctly grasped its significance. Although many archaeologists are inclined to see some continuities between the prehistoric Hohokam and the modern O’Odham cultures, and other archaeologists are less inclined (Cordell and Smith 1996:229), accrediting the oral traditions as in some degree, in some sense, historical remains a separate issue and one that has not been definitively settled either in Teague’s papers or here. Teague has marshaled a variety of evidence and deployed arguments that compel serious consideration. Even with the sometimes less than persuasive supporting evidence criticized earlier, the fact of some concordance of O’Odham and archaeological site identifications commands attention and invites further attempts at explanation.

A Rock Art Story from Wisconsin

Conjoining archaeological data and an interpretation of the rock art at the Gottschall Rockshelter in southwestern Wisconsin, Robert J. Salzer (1987, 1993, 1997) maintains that he has been able to demonstrate “... that at least some of the stories of the modern Winnebago people were being told a thousand years ago” (1993:95). The “some” turns out to be one, specifically that identified as the legend of Red Horn, or He Who Wears Human Heads As Earrings, which, he says, “... has been passed on by word of mouth with sufficient accuracy to permit a twentieth century archaeologist to identify that story in the painted figures on the wall of a rockshelter” (1993:95). This conclusion, if correct, would be impressive testimony to the potential of verbally transmitted stories to survive in recognizable guise for something on the order of forty generations. If a legend collected in the early years of the present century by ethnographers (in this instance by Paul Radin [1948] among the Winnebagoes and Alanson Skinner [1925] among the Iowas) can indeed be shown to have existed a thousand years earlier, may not accounts of a more historical nature have similarly endured? To seriously entertain this latter possibility mandates a considerable burden of proof before conceding the legitimacy of the former claim. Unfortunately, the kind of clinching test like that provided in the case of the Ridge Ruin burial is missing in this case. Plausibility, even if granted in the case of Gottschall, does not equal probability, let alone certainty.

At the Gottschall Rockshelter, archaeological evidence and radiocarbon assays have securely dated the group of pictographs believed to depict the Red Horn myth. This composition, as Salzer calls it, includes human and animal figures. There are three portrayals of humans and either two or four of animals (only two of the latter seem unambiguously independent of human costuming). The one figure selected as diagnostic is regarded as the protagonist himself, Red Horn. The other paintings, considered to signal some of the “supporting actors,” can cer-
tainly be construed as compatible with the legend, although they are not necessarily unique to it. Unaccompanied by the main character, they, including the two human figures thought to be giants, might as plausibly be attributed to any number of other stories, including those of Algonquian speakers as well as the Siouan-speaking Winnebagoes (Hochungaras) and Iowas. Although Salzer presents a full and careful comparison of the art styles and motifs of the Gottschall Rockshelter with those at Mississippian and related sites in the near and farther south, the critical query focuses on the recognition of the “diagnostic figure” as Red Horn. For this he (Salzer 1987:464, 1993:86) has relied on Robert L. Hall’s (1991:31) identification. That identification is based on “[. . .] what appears to me to be Long Nose God masquettas tattooed on his [the figure’s] chest in the area of each nipple. One of Red Horn’s sons had little human heads on each breast; the other had human head earrings like his father” (Hall 1991:31, 1997:148–151). Hall’s discussion of the possible significance of the Long Nose God motif and its relation to prehistoric ideology is both informed and ingenious. Nevertheless, the masquettas on the chest of the Gottschall pictograph that “appear” to him do not do so for everybody who looks for them. It may indeed be that some of the paintings were meant to record the Red Horn myth, but the evidence falls short of convincing. Geographically and temporally impressive continuities in certain rock art stylistic conventions have been demonstrated. Their meaning, inherently more elusive, has not.

In addition to scrupulous examination of whatever grounds may be put forward in support of particular oral traditions as credible history—the nuts and bolts, as it were, of the individual cases being considered—some overarching theoretical issues must be given their due as well. These include a) appraisal of the degree to which history and oral traditions reflect and incorporate common goals and conceptions of reality; b) appreciation of the likelihood of only partial, and perhaps very little, overlap in the foregoing and thus the inevitability of serious problems of incommensurability; c) recognition that whatever history and oral tradition are taken to be, each may vary in practice in its success in expressing its message; and d) the weight that must be accorded the nature and limitations of memory. Although some of these issues have already been broached, they will be treated in greater detail later in this essay.

Pro Argument #7. Archaeology, a Product of Western Civilization, is Ethnocentric and Must Be Balanced If Not Replaced by “Alternative Histories.”

The discipline of archaeology is indeed a product of Western European civilization, and a rather late one at that. It was ethnocentric in its origins in the sense that it emerged as a product of the cultural evolution of British, Danish, Euroamerican, French, German, Italian, Swedish, and other such nationalities and did not arise among the Aruntas, Dinkas, Fijians, Hopis, Polar Eskimos, Shah Nawazis, Yahgans, and others. It is not ethnocentric because it eschews the narcissistic, ethnically-unique, quasi-religious character of the latter societies in favor of a universal, culturally transcendental epistemology in principle open to anyone instructed in its procedures. Whereas areas of “traditional knowledge” enmeshed in the oral “histories” of, say, Zunis or Hopis, are off-limits to non-Zunis or non-Hopis (see Anyon et al. 1997), no such proscriptions obtain in scientific inquiry. In polar contrast is the “open admissions policy” of the universal sodality of archaeology and the other sciences. This sodality welcomes Aruntas, Dinkas, Fijians, and all others to undertake the rites of passage common to all would-be members. Although the absolute numbers are small, this membership is universally distributed, attracting as it does those who, while often cherishing ritual and respecting tradition, value testable knowledge more.

A defensive reaction of many Indians to archaeological conclusions at odds with their own traditional understanding adopts the argument of exclusivity: “You have your history, we have ours.” And the difference between the two is much more than meets the eye. Another trouble arises because the erroneous assumption is sometimes made that we are all talking about the same thing, but from different perspectives. This returns us to Rescher’s point about elephants in my previous dismissal of “A Red Herring”—to wit, if others are not talking about what we define “history” to be, they are not talking about history but about something else. And indeed this is often the case (Kuznar 1997; Rescher 1997). Traditional “history” is not history, although it may contain vestiges of history that may or may not be susceptible to “winnowing fact from fiction” (Cowgill 1993:561). Today, of course, many question such assertions, arguing that they are an illegit-
 intimat privileging of Western values over those of other people, a sort of epistemological bullying of the powerless, an attempt to take over and recast in our own image the cultures of others. But this is cant and does little to encourage communication let alone address substantive issues about the nature of cultural history and how we can possibly know it. Some critics of the Western “way of knowing,” as we saw earlier in pro arguments #1 and #2, champion “alternative histories,” knowingly combining apples and oranges or ignorant of the distinction altogether. For still others, none of this matters at all. One version of philosophical nihilism currently chic in academic circles, and even archaeological squares, sees nothing in historical disputations—or indeed in all knowledge and all claims to objectivity—but ideology, politics, and economic self-interest (Nassaney 1994). On such grounds, oral traditions must be as useful or as worthless as any other would-be source of information about the past. Power, not reason, tips the scales.

Discussion of the Con Arguments

Many of the objections to the use of oral tradition as an additional or alternative source of information for archaeological reconstructions of the past have been anticipated in the foregoing section. The following discussions elaborate on some of the more important of these and add further considerations. It needs to be stressed again that these negative arguments are specifically directed at the employment of oral traditions as historical data, that is, as having parity with the methods, data qualifications, and metaphysical stance of Western historiography.

Con Argument #1. Dependent on Memory and Verbal Transmission, Oral Traditions Are Simply Not Trustworthy.

Memories falter, and their recitation is as much an adaptation to the circumstances of their elicitation as a recapitulation of their birth. This occurs in a single lifetime and is universal. I remember the sheer force of the singing of God Save the King causing the chandeliers in our church to move during a ceremony sending off one of the local regiments to war. My father told me we would do this, so I watched, and we did. I know we didn’t, but I remember that we did. My cousin in the Dieppe raid remembered that more Germans shot at him than at anybody else. I remember discovering an archaeological site that my wife remembers discovering. The great-grandfather of a friend and colleague died of gunshot wounds in a manner appropriate to the hero in a Wild West novel or movie; his sister, alas, claims proof of an ignoble demise in circumstances more worthy of a farce. This person clearly saw X strike Y first, that person is adamant that Y struck the first blow. And so it goes. Everyone can offer their own examples. So commonplace, indeed unavoidable, are such experiences that whole literatures have evolved to deal with them and their implications for research in psychology, sociology, ethnography, and history, and for the practical needs of public opinion surveys, merchandising, law enforcement, social services, and jurisprudence (e.g., the reliability of eyewitness testimony).

Nevertheless, a still widely shared bias exists, even among many archaeologists, that credits Indians, a priori, especially those identified as “elders,” with powers of memory credibility far beyond anything that would be granted anybody else. It may well be that Native Americans, and other formerly nonliterate peoples as well, paid closer attention than do literate folk to preserving accuracy in their memories of especially important events. Even with mnemonic aids such as ritual chants and prayers and other associative verbal formulae, notched sticks, painted buffalo robes, wampum belts, and so on, the past is “preserved” in memory. There is nothing else. But in whose memory? What is the selective process whereby X’s memory is given ascendency over Y’s? And for how long and in what circumstances? Except for the social value that may be placed on the possession of a good memory, no evidence exists to indicate an intrinsic difference in the processes of memory acquisition, retention, or recollection among different societies. The psychological mechanisms are innately human, their product inherently transient. The forensic psychologist Elizabeth Loftus concludes that “Contrary to popular belief, . . . facts don’t come into our memory and passively reside there untouched and unscathed by future events” (Loftus and Ketcham 1991:77). Continuing life experiences interact with memory and so alter it that “. . . experimental psychologists think of memory as being an integrative process—a constructive and creative process—rather than a passive recording process such as a video-tape.” Researchers have been able to show that memory deteriorates over time. “After a week, memory is less accurate than after a
day. After a month, memory is less accurate than after a week. And after a year, memory will be less accurate than after a month” (Loftus and Ketcham 1991:77). Mark Twain once confessed, “It isn’t so astonishing, the number of things that I can remember, as the number of things I can remember that aren’t so” (quoted in Loftus and Ketcham 1991:31).

Memories don’t just fade [Loftus observes]. . . . they also grow. What fades is the initial perception, the actual experience of the events. But every time we recall an event, we must reconstruct the memory, and with each recollection the memory may be changed—colored by succeeding events, other people’s recollections or suggestions, increased understanding, or a new context.

Truth and reality, when seen through the filter of our memories, are not objective facts but subjective, interpretative realities. We interpret the past, correcting ourselves, adding bits and pieces, deleting uncomplementary or disturbing recollections, sweeping, dusting, tidying things up. Thus our representation of the past takes on a living, shifting reality; it is not fixed and immutable, not a place way back there that is preserved in stone, but a living thing that changes shape, expands, shrinks, and expands again, an amoebalike creature with powers to make us laugh, and cry, and clench our fists. Enormous powers—powers even to make us believe in something that never happened (Loftus and Ketcham 1991:20).

Placed in a larger context, that is, memories accumulated into oral traditions, Jan Vansina (1985:172) finds that “The effects of restructuring [the changes wrought by the operation of memory, changed social situations, and other contingencies] can be devastating to historical information.” Although still persuaded that oral traditions are worthy of careful consideration by historians, he cautions that “Temporal transpositions are frequent, fusion prevents one from disentangling the original elements that were fused, selection discards data, and secondary causes are eliminated by sequential reordering and by the operation of the cultural ideas of causality” (1985:172). Vansina, citing an African example, notes a further complication. Sometimes traditions will be jettisoned when confronted by a new authority: “. . . when an informant perceives from a question asked by a supposedly learned interviewer that the latter believes the answer to be such and such, he or she may simply concur, not just in a desire to please, but also out of a new found conviction” (1985:111). A candidate for a North American parallel concerns the builders of the famous effigy mounds of Wisconsin and its environs.

In his classic monograph on the Winnebago tribe, Paul Radin credited the historic representatives of that tribe with having “unquestionably” constructed the effigy mounds, even going so far as dating most of them to the eighteenth [!] century (1923:79–80, 82, 85). His information came from the testimony of living Indians themselves. Some of “the older people claimed to have distinct recollections of the erection of some of them” (1923:79). A quite different picture of this attribution has been published by James B. Griffin. The latter has written “When McKern [the archaeologist W. C. McKern] asked the people with whom Radin had worked how they knew their ancestors had built the Effigy Mounds they replied that Radin told them that was the case” (Griffin 1995:15–16). Since Radin’s time, anything remotely close to an eighteenth-century date for the effigy mounds has been ruled out by convincing radiocarbon and other evidence. And until just recently, almost all archaeologists have been agreed that the Winnebagoes had little or nothing to do with the famous mounds (see Mason 1993 and the other essays in Overstreet 1993).

Returning to the restructuring of oral traditions, whether or not learned interviewers are implicated, Jack Goody and Ian Watt (1968), as well as Vansina and other students, have laid stress on what I would describe as the continuous retreat of non-literate societies from their own pasts as new generations preempt the always limited capacity of social memory to accommodate their own contemporary needs. This process of retraction of the historical tail is ineluctable, and it affects genealogies as well as the other components of the cultural legacy that travel with society through time. As Goody and Watt (1968:33–34) put it, citing earlier work by Bronislaw Malinowski:

. . . genealogies often serve the same function that Malinowski claimed for myth; they act as ‘charters’ of present social institutions rather than as faithful historical records of times past (1926:23, 43). They can do this more consistently [than can literate societies] because they operate within an oral rather than a written tradition and thus tend to be automatically adjusted to existing social relations as they are passed by word of mouth from one member of the society to another. The social element in
remembering results in the genealogies being transmuted in the course of being transmitted; and a similar process takes place with regard to other cultural elements as well, to myths, for example, and to sacred lore in general. Deities and other supernatural agencies which have served their purpose can be quietly dropped from the contemporary pantheon; and as the society changes, myths too are forgotten, attributed to other personages, or transformed in their meaning.

One of the most important results of this homeostatic tendency is that the individual has little perception of the past except in terms of the present; whereas the annals of a literate society cannot but enforce a more objective recognition of the distinction between what was and what is. . . . Myth and history [in non-literate societies] merge into one: the elements in the cultural heritage which cease to have a contemporary relevance tend to be soon forgotten or transformed; and as the individuals of each generation acquire their vocabulary, their genealogies, and their myths, they are unaware that various words, proper names and stories have dropped out, or that others have changed their meanings or been replaced.

. . . The pastness of the past, then, depends upon a historical sensibility which can hardly begin to operate without permanent written records; and writing introduces similar changes in the transmission of other items of the cultural repertoire.

While subsequent commentaries on texts may vary, and the original writers of those texts may often have had purposes other than or additional to honest reporting, the written record is the closest we are ever apt to come to whatever it was that came to be set down in text. Furthermore, there sometimes exist additional contemporary and/or later texts relevant to the first and to each other that can be consulted for purposes of authentication, cross-checking, augmentation, changes in interpretation, and so forth. Here, at least, is direct access to an original voice; oral traditions offer at best word-of-mouth renditions of word-of-mouth stories with little or no hope of comparison with older versions, let alone the prototype. And as history, oral traditions appear to have but a shallow time depth. How shallow? Scholars such as Jan Vansina, Donald Henige, and Donald Bahr have labored to take soundings.

In his impressive cross-cultural sampling of non-literate societies, Vansina has discovered a nearly universal trifurcation of the past as revealed in detailed analyses of oral traditions (1985:23–24, 168–169). These describe a metaphorical hourglass, albeit with elastic sides capable of stretching or compression. The upper chamber is oral history or “recentness” (Bahr 1998a), the time of the recent past, the shallow past of real, remembered people and events as recollected and commented on by the oldest members of a community; although typically calibrated by genealogy, this is time susceptible in principle to calendrical measurement and it involves locatable places. The lower chamber of the hourglass is oral tradition, the time of “ancientness” (Bahr 1998a), of origins, the timeless past of myth, of the relatively deep but unfathomed time of creators and heroes who confound the distinctions of what in recent times separate gods, humans, and animals. Both chambers contain a lot of information. The recent chamber’s contents, however, rapidly dwindle with increasing age as the neck of the hourglass is approached. That neck is what Vansina (1985:23–24, 168–169) has labeled “the floating gap.” It represents either an hiatus or a featureless frontier relieved only by one or a few names or references. The length of time it encompasses is typically unknown and unknowable. For all intents and purposes it may be millenia or a mere generation or two—a “wink” as Bahr puts it (1998a:32).

The tenure of the floating gap and the two chambers of the hourglass is most succinctly expressed in Bahr’s comparison of Pima, Maricopa, and Yavapai mythologies:

> Since the mythologies are continuous, we can get some idea of the length of this most ancient period. The ancientness seems brief. As for the time at the post-gap, recent end, for these peoples and for all those whom Vansina surveyed, approximately all of the world’s tribes, the recent past is also quite short, going back only about 100 or 150 years from any given ‘now.’ In short, if the gap was a wink, the time of ancientness ended just somewhat before the birth of the oldest living person; and the ancientness, too, was brief, that is, the ‘running time’ or ‘told time’ of an entire mythology does not seem to be long, the equivalent to a few generations at most . . . (Bahr 1998a:32).

Mythologies serve more than one function, but probably their chief function is, as Malinowski (1926:23,43) and many later students have convincingly argued, as “charters” or foundational claims for the legitimacy of the societies promulgating them. And as such, they almost inevitably acknowledge the
existence of neighboring groups, albeit usually retrospectively and through indirect, if only to signal the individual society’s separateness (Bahr 1998a, 1998b). They are adaptive inventions. And myths and oral traditions change with contemporary contingencies as well as with the passage of time. The historicism of oral histories is limited, shallow, and retracts from a past that slips into, and eventually though, the neck of the hourglass into myth. I find the metaphor of the hourglass appealing, but I would add to it a small hole at each end. As societies traverse time, they ingest, as it were, but a portion of the externalities impinging on them; these enter the upper chamber of the hourglass while the larger world of externalities passes them by, unperceived even, and unknowable to them. At the bottom of the lower chamber is the other hole, a black hole through which, inevitably, even myths are sucked into oblivion. The hourglass remains, now stretched, now compressed, moving with the rest of society through time, its contents refreshed even as the old are gradually lost. But only in that upper chamber, the time of recentness, is there a realistic chance of isolating anything that may be meaningfully labeled history. All else, valuable for other ends, is not “. . . to be understood as fragmentary and corrupted remains of stories that once existed in the same conceptual framework as recent European narrative history, or at least chronicle” (Cowgill 1993:561).

Con Argument #2. The Genre by Its Nature Is More an Artifact of Contemporary Culture than a Record of the Past.

Con Argument #3. Oral Traditions Are Closed Belief Systems, Beholden to Authority and Impervious to External Challenge.

Although these two arguments share some common ground, the assertions they are meant to represent are not of equal weight or validity as they stand.

People need stories to live by, and for most people most of the time it doesn’t matter if they are true, so long as they are believed or at least serve the current needs they are thought to address, in which case they will be believed anyway. When they cease to be believed and are felt to be inadequate to society’s needs they are modified or replaced (Bohannan 1952; Halbwachs [in Coser 1992]; Leach 1954:264–278). Those needs are usually mundane rather than abstractly philosophical and pertain to the individual’s place in society, rights and obligations inherent in that place, societal identity, rights to territory and resources, relationships with other societies, and so on. Only in literate societies, and then among a minority of the population, is there the possibility and the actuality of systematic and sustained interest in historical inquiry and, in theory at least, in disinterested questioning and testing. While it may be correct to say that in both cases current needs are being served, in literate societies an enduring record of the past accumulates and is available for critical scrutiny and testable modeling. The upper chamber of the hourglass expands at the expense of both the floating gap and the lower chamber. Not that the latter disappear (“most people most of the time . . .”). Witness the ongoing confrontations in the popular culture between evolutionism and creationism, modern Biblical scholarship and the fundamentalist doctrine of inerrancy, genetic conceptions of “race” and the categories printed on government forms, archaeological criteria for positing cultural affiliation of artifacts and some of those mandated by NAGPRA, and so on.

The third con argument is only partly true. While hardly wide-open systems congenial to criticism, and certainly recycling received wisdom, even if imprecisely, oral traditions of neighboring groups are in some sort of communication or “conversational relationship” with one another, as noted by Bahr (1998a), and as indicated by overlapping elements, including dollops of recognizably similar myths. Interacting with one another and subjected to some of the same vicissitudes of life, it would be strange if any oral tradition had ever been totally impervious to outside influence, however ethnocentric it typically is. Except under unusual conditions of extreme stress, such as those implicated in the diffusion of new institutions like the Ghost Dance or the Native American Church, resistance to challenges to traditional knowledge is apt to be most pronounced where important religious considerations also are involved. And this leads to the next and last con argument.

Con Argument #4. All or Parts of Oral Traditions May Be Considered Sacred, Only Partly or Not at All Accessible to Outsiders; Guardians of Such Lore Determine What May Be Released and How it May Be Used.

Insofar as this is true, the would-be incorporator of traditional history into scientific reconstructions of
the past must treat it with extreme caution or even ignore it altogether on the jurisprudential grounds of "inadmissable evidence."

"Hopi cultural advisers," say Anyon et al. (1997:85), "are the best judges of what aspects of oral traditions constitute historical information and what aspects constitute esoteric knowledge that should remain confidential." The Hualapais want only tribal members to conduct any research that may be done with Hualapai oral traditions "so that sensitive information can be controlled and the tribe can be sure it is used for appropriate purposes" (1997:86). "The Zuni Tribe does not encourage the use of oral traditions in scholarly research, except in a very limited fashion by researchers employed by the tribe and for purposes Zuni cultural advisers feel are acceptable to tribal cultural sensitivities" (1997:86). Echo-Hawk maintains that "a religious approach accepts oral texts as the source of holistic truths rather than as documents that require evaluation for historicity" (1997:89). Comparable assertions abound. As stated earlier in this essay, people are fully entitled to believe as they wish and to guard their beliefs from foreign intrusions.

Now neither Indians nor anybody else, however, can reasonably expect whatever they have to offer to be unquestionably accepted as historical data, particularly when what may be equally relevant, or indeed even crucial to understanding what has been offered, is withheld on the grounds of sacredness or some other privilege. There is no room for the consideration of private information as data in any archaeology aspiring to scientific status. In the contemporary political climate, what should be a truism needs reiteration and defense. Notwithstanding published as well as private calls for "the incorporation of traditional history into the suite of evidence used by archaeologists for interpreting the past" — even though doing so must "potentially require the reconciliation of contradictory views" (Dongoske et al. 1997:606) — the blunt fact of innumerable and fundamental irreconcilabilities must be faced and not fudged. But this is difficult if not impossible to do when the would-be integrator or incorporator is permitted to use this information but not that information and thus cannot evaluate the connections and merits of whatever the controlling authority decides he or she may use.

If religious conceptions are held to have relevance in scientific discourse, their sponsors should be prepared to see them receive the same rough treatment meted out to secular data. If instead they are withheld in whole or in part, those ideas should be privileged by scientists and all fair-minded people to enjoy sanctuary undisturbed. And in that case, archaeologists are ill-advised to place any reliance on a pick-and-choose sampling of the bits and pieces that may be dribbled out to them from secret troves they can have no hand in assaying.

Conclusions

Although legally required to accept politically mandated as well as scientific criteria in meeting the stipulations of NAGPRA, probably most archaeologists in North America are inclined to caution in their own research in taking oral traditions as histories. Because information is always scantier or less definitive than one would like, the possibility that oral traditions incorporate real historical information that might usefully be conjoined with the archaeological is a tantalizing prospect nevertheless. Distinguishing genuine promise from the siren songs luring the incurious onto epistemological shoals is the problem.

Addressing one aspect of this conundrum, that of foraging for possible clues to absolute chronologies, David Henige, albeit using mainly African and Near Eastern data, has characterized that effort as a hunt for a chimera. We too often erroneously assume, he says, that non-literate societies "have remembered their past 'calendrically,' that is, linearly, sequentially, and even chronometrically. In fact, achronicity is one of the concomitants of an oral noncalendrical society" (1974:14). Relative chronologies frequently occur, obviously, but they are usually minimally informative and often manufactured to address current interests rather than preserve arcane historical ones. Not uncommonly, the relative chronology is but an expression of the metaphorical hourglass: recency and the ancientness of myth, with or without something signaling the waist. The usual character of oral traditional events and the magnitudes of the intervening "befores" and "afters" are tantamount to all but the most recent end of recentness being devoid of chronology. "History" sans chronology is not history. And this is not mere wordplay. Eric Wolf's magnificent sermon (1982) notwithstanding, Samuel Kramer (1959) knew what he was about when he wrote History Begins at Sumer.

Quite apart from the niggardliness or even absence of anything identifiable as chronology, any-
one having any familiarity with the genre at all is not surprised by the large tracts of oral tradition populated by fabulous beings involved in fantastic happenings. Even the most dedicated searchers after credible information in this corpus have little difficulty recognizing and dismissing the incredible—the contents and essence of myth, the lower chamber of the hourglass.

But the fabulous and incredible inhabit recency (the upper chamber of the hourglass) as well, especially as the floating gap is approached. Things and events that never happened occur here as well as things that did, things that belong earlier are brought forward, dislocated, and transformed into something else, events known on other grounds to have occurred are missing entirely or have been radically disguised by reinterpretation, mistake, or ignorance. Europeans are resurrected Indians, prehistoric linear mounds in Wisconsin were defensive works built in the nineteenth century, stone arrowpoints were made by worms or water spirits, horses have always been present, horses were just recently created or introduced by a culture hero, pottery was made by pressing clay into a hole in the ground, the different “races” had different creations, ravens talk, bears crave tobacco, people can turn into animals and vice versa—the recital is endless. And it is fascinating and indispensable for many anthropological and folkloristic purposes in addition to its intrinsic value as cherished cultural patrimony and even entertainment. This is not material to be laughed at or denigrated as worthless. It also is not to be made into something it is not. It must be understood on its own terms, in its own context. Freely granting the sometime survival of information of possible use to the archaeologist or other cultural historian, oral tradition is not academic or scientific history simply recorded in another language. These are distinct genres and should be treated as such. As just acknowledged, they are not mutually irrelevant, however. Even though the relationship is asymmetrical, as we have seen, cultural historians have long been and always will be attracted to anything that might help in throwing light on the past.

Archaeologists suspicious of injecting tidbits of traditional history into the data of their discipline must necessarily pay attention to the efforts of their less reticent colleagues (who in turn should reciprocate the interest by trying to understand the reasons for suspicion). Some of those efforts at using traditions have proven worthwhile, more seem trivial or even worthless, while most are of indeterminate value because of problems of testing. By comparing the disparate results, proponents and skeptics together might at least cease talking past each other, identify the causes of disparity, and, where necessary, agree to disagree and know the reasons why.

A good place to begin is the evaluation of sources. If the archaeologist uses information provided by a native informant, some sophistication in ethnographic data evaluation is virtually mandatory. How knowledgeable is the informant? How did he/she come by the information being sought? Is it verifiable? Clear answers to these kinds of questions are more likely to be approximated than realized. And that some informants may be identified by the community or by themselves as “elders,” a credential with known power to disarm otherwise worldly scholars, is a potential trap as likely to have been constructed by the information seeker as by its giver. As the literary critic Audrey Jaffe has written in another context, “The assertion of knowledge and authority. . . does not necessarily reflect their secure possession” (quoted in Rosenberg 1996:145).

In the majority of cases, however, oral traditions are derived secondhand from ethnographers or historical documents, or even more indirectly from ethnographers. These sources, too, have to be assayed for credibility if anything of weight is to be built on their testimony. And rare is the ethnographer—or anybody else—who is equally dependable even in all departments of his or her competence. Once again, examples will occur to most archaeologists. Lowie’s complaints from the early twentieth century about picking and choosing among oral traditions were noted earlier. His complaints are paralleled in more recent critiques. For example:

In the case of Radin and the Winnebago, his stature as an ethnographer casts a long shadow and throws the cloak of authority over whatever else he was doing besides his own ethnographic business. Archaeologists lean on the bits and pieces of possibly pertinent information, using them to support one or another interpretation of their archaeological data. . . . Radin’s collection of 20th-century memories may or may not represent earlier Winnebago culture at all. To agree with those recollections when they are useful and ignore them when they are not gives archaeologists a convenient authority to appeal to but no way of demonstrating that their analo-
gies are correct: connections between modern ethnography and even 18th century Winnebago life have not been established (Mason 1985:100, italics mine).

Nevertheless, foolish or angelic archaeologists will continue to pick and choose among the offerings of oral traditions. They should be aware, however, that doing so is tantamount to cherry picking in a minefield. One or more of three reasons might lure them to take the risk:

1) Hope for serendipity: good and unanticipated ideas can come from almost anywhere and should be pursued;

2) Extrapolation by analogy, especially where cultural continuity can be shown or reasonably assumed: some of the same information about descent systems, postmarital residence patterns, sodalities, sexual division of labor, symbolism, tool functions, and similar activities that ethnographers or ethnohistorians have teased out of traditional knowledge may prove enlightening to the archaeologist seeking a richer glimpse of the society once responsible for his or her physical data, even if only in the minimal sense of constraining the range of speculation; and

3) Historical inquiry: because only a fraction of the past survives in the archaeological record, the cultural historian will inevitably seek further insights wherever they may be found, be it via linguistic connections, biological affinities, statistical adducing of the diffusion of complex cultural attributes, or traditional knowledge. Indeed, the search for as many possible independent sources of information as possible is an obligation of any researcher. The method of convergent verification, where successful, enhances the likelihood of educated opinion arising out of “speculative history,” if not metamorphosing into genuine knowledge.

To pursue the consequences of any one of these investigative warrants is difficult enough using the assumptions and procedures of contemporary science and its paradigmatic equivalent in systematic historiography. Success in the former, however, becomes improbable as the latter are called into question by the intrusion of demands they are not designed to address. The most disjuncting of these demands today go beyond merely endorsing the archaeological adoption of verifiable cultural and historical facts that may be extractable from traditional knowledge. Some, in effect, call for the low-er of scientific/historiographic standards in the service of a well-meaning but corrosive ecumenism of the irreconcilable. The usual form of that demand—fair on its face, appealingly sympathetic, diversity-celebrating, and so democratic—is the call to eschew all vestiges of “hegemonism” and to embrace the doctrine of cultural relativism in archaeological practice. Versions of that call have been cited earlier in this essay. They all admonish archaeologists and Indians alike to be “respectful” of each other’s “different” but “equally valid ways of knowing.” In effect, however, they are directed more at archaeologists than Indians, urging the former to acquiesce in what would amount to the re-mystification of the past, thereby also recruiting them, as it were, to do the latter’s proper work of adapting themselves to the modern world. Either that or politely not to question the fiction that it really doesn’t matter if disparate knowledge claims are unequal in validity and the philosophies of knowledge underlying them are in principle and practice incommensurable. Honesty is not served by pretending that every account of anything is equally valid. On any grounds amenable to logical argument and empirical testing, Native Americans are descendants of ancient immigrants and did not originate in the New World, however much their traditional histories attest the opposite and sensitive educators urge equal treatment in elementary and secondary school instruction (Harvey et al. 1995:151). Neither the ancestors of the Teton Dakota nor anybody else originated in the Black Hills or crawled out of holes anywhere else. The Iroquois were not the constitutional model for the young American Republic (Tooker 1988), Indians probably did not make maple sugar until after European contact (Mason 1986), Chief Seattle did not make the famous speech attributed to him (Kaiser 1987), the bulk of evidence locates the genesis of the “ancient” American Indian concept of “Mother Earth” in the nineteenth century A.D. (Gill 1990)—even the indefatigable Vine Deloria Jr. (1997:213) is fatigued by this one, and all the yearning for the equal credibility of diverse “ways of knowing” will not make them what they are not, however “respectfully” treated and given equal billing at national monuments and in chic museum exhibits. Even metaphorical squares cannot be made into circles yet retain their original identity, the best intended obeisance to alternative knowledge claims notwithstanding.
Unlike information drawn from radiometric dating, stratigraphy, or seriological analysis, methods that have global legitimacy and are based on known physical laws or firmly grounded cultural principles, and that are themselves testable by independent means, most of the contents of oral traditions are of an entirely different nature and should not be treated in the same way. Oral tradition is quintessentially local, however much it tangentially acknowledges the existence of “others;” it is culturally specific, memory-dependent, and accepted on faith. Vis-à-vis other locales or cultures, oral tradition is private knowledge, not accessible except on its own authority. It is foreign to and independent of the body of axioms, postulates, corollaries, reductive reasoning, canons of evidence, and commitment to testing that unite physics, chemistry, geology, biology, archaeology, etc. into a common, coherent, consistent way of comprehending the world—what E. O. Wilson calls the search for consilience (1998). Oral tradition, on the other hand, is particularistic and unintegrated with any governing hierarchy of understanding other than the customs and mindsets peculiar to its articulators in their unique context of time, place, and circumstance. Put another way, ... societies that do not write their history produce narratives about the past that are exempt from critical scrutiny, ... these narratives cannot [therefore] assume the function of reflexivity that we associate with historical consciousness. In this sense, their history is tradition not because it is oral or undeveloped, but because it is shut off inside a lived relationship to the past and consequently to itself (Lenc lud 1997:62).

Like religion, you believe oral tradition or you don’t. And although, as with religion, there may be pieces of history embedded in particular oral traditions, they must be tested out by adherence to the rules of rational inquiry. But the possibility of so doing does not thereby confer equal epistemological status on the two “ways of knowing.” And oral traditions cannot simply be adopted wholesale into the structure of science or Western historiography without violating consilience.

Oral traditions, as stories relating a people’s traversing of time, differ from those other branches of traditional knowledge by means of which investigators learn about residence patterns, kinship systems, conceptions of gender, sodality initiations, and so on. The former proffers a mythologized accounting of formative experiences thought by believers to have made them the people they are today. The latter describe essentially synchronous phenomena repeatedly manifested in any individual’s lifetime and are, by that fact, relatively easy to verify. The uniqueness of oral traditions, invarially accompanied as they are by the other encumbrances previously mentioned, make them even more elusive than the chronological chimera of David Henige’s (1974) characterization. Their “prioritizing of historical meaning” over “factual accuracy,” their capacity for “myth-time” to “coexist with either the present or the recent past,” their common failure to see “few incompatibilities between the world of facts and that of dreams,” and “the continuing Native American acculturation [read annihilation] of time and space” (Nabokov 1996:25, 27, 53, 54), when combined with admonitions to practitioners of Western historiography to “somehow weave Native historicity into the bedrock of their accounts” (Nabokov 1996:54), has an ineluctable Alice-in-Wonderland quality. As we have seen with Martin (1987:3–26) and some others, the invitation to step through the looking glass is alluring. It also is a mistake. To study Indian historiographies in an attempt to understand them on their own terms is one thing: to appropriate and make of them a component or extension of Western historiography is another. Martin himself unwittingly poses the conundrum when he demurs the latter for forcing “timeless” people into time when they properly belong “in eternity” (1987:15).

When archaeologists try to augment the intellectual tools at their disposal by borrowing information from oral traditions, they need to exercise greater caution than current calls for such augmentation typically recommend. To preserve, let alone extend, the unparalleled power of science and systematic historiography to produce testable historical statements requires, like liberty, eternal vigilance. Caution and vigilance, however, do not mean proscribing chancy or innovative inquiry, wherever it may lead. There are, as has been shown, vestiges of genuinely recoverable pasts in some oral traditions. But because recording the past has necessarily always been incidental and subsidiary to the main functions of that genre—credentialing society’s authenticity while adapting it to current needs—it is a difficult task to tease out what may qualify as bona fide historical data. Is the possibility worth the effort? Does the possible injection of elements of false historicity war-
rant weakening the reliability of the less question-
able? Such questions will doubtless be differently
answered in each case with variable degrees of con-

This critical canvassing of the pros and cons
attending the employment of oral traditions in
archaeological reconstructions of culture histories
has tried, and largely failed, to find sufficient epis-
temological common ground to endorse such
employment as is currently in vogue in admonish-
ments if not practice. Throughout, the intent has been
to fairly present and respect the fundamentally dis-
parate natures of the two epistemologies. As stated
elsewhere (Mason 1997), archaeologists, like sci-
cists generally, are charged with truth-seeking, how-
ever elusive it may be and however displacing or not
of “other ways of knowing.” While the purveyors of
the older wisdoms are to be respected as people, their
recountings of ancientness are challengeable when
they are thought of as data roughly on a par with,
say, dendrochronology, seriation, or site distribution
maps. Current calls for cross-cultural historiographic
integration notwithstanding, it is the conclusion of
this essay that oral traditions are more often than not
roadblocks than bridges to archaeologists aspiring
to know “what happened in history.”

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