

JOHN H. MOORE

The University of Oklahoma, USA

## ETHNOS AND ETHNIC PROCESS ON THE NORTH AMERICAN PLAINS

In 1982, Bruce Trigger encouraged ethnohistorians to emphasize the study of „change“ in ethnological and archeological data, although he did not specify what kind of change we should be studying (1982: 1). In the same paper, however, Trigger denied that change could „be inferred from general ethnological principles“ (1982: 13). I might introduce the present paper, then, as an attempt to accept Trigger's charter, while disregarding his cautions about ethnological theory. In fact, I wish to advocate the particular benefits of conceiving change in the abstract as „ethnic process.“ I will do this both by inducing toward general ethnological principles, which I will define, and by inferring from these principles to concrete illustrations from the North American Plains.

I should emphasize at once that I do not claim to have discovered the theories or processes I will discuss here. For the most part, I have merely collected together various theoretical ideas and tendencies which have appeared in the ethnohistorical literature over the last several decades. But I will argue, most emphatically, that taken together these ideas and tendencies constitute an integrated and well articulated structure of theories about history, about language, and about archeology. As parts of a new synthesis, they constitute a powerful theory for explaining many aspects of Plains ethnohistory which presently seem puzzling.

I will try to outline these problems in a useful manner, focussing on the Great Plains, and separating the issues into three categories: 1) historical 2) linguistic and 3) archeological. After initially considering some logical aspects of the problems, I will then present some specific illustrations from my own recent work on the Cheyennes.

### Some Logico-Empirical Problems

Waldo Wedel himself, one of the developers of the „direct historical approach,“ cautioned against extrapolating too far backward in time when approaching „the unknown through the known, the prehistoric through the historic“ (Strong 1935: 6).

In the first chapter of *Prehistoric Man on the Great Plains*, Wedel warned that „the number and reliability of historical records drops off rapidly before the year 1800 . . . so that it is no longer safe to apply modern tribal names to the earlier groups“ (W.

Wedel 1961: 7). Anyone who has tried to follow a particular tribe from the North American Plains back through the early documents is aware of the problems involved. On the 1805 maps of Lewis and Clark, for example, perhaps half of the entries do not correspond clearly to known historical tribes (See Figure 1). Even earlier, the tribes named by Carver and the Verendryes are somewhat less familiar, while the tribes named in the Jesuit Relations are the least familiar of all (Carver 1976, Wood 1981, Thwaites 1896). Helen Tanner's recent atlas provides an excellent cartographic illustration of how tribal affiliations become more and more problematic as one works back in time (1986).

Unfortunately, many ethnohistorians have made it their professional business to connect these groups of unknown affiliation with tribes known historically, so that lengthy debates have raged, for example, over whether the Kilitian, the Monsoni and the Muskegoes were Cress or Ojibwas (A. Greenberg and Morrison 1982: 77). Many other such assertions and debates have unfolded over the last several decades (W. Wedel 1961: 278-311, M. Wedel and DeMallie 1980). In all such cases, the temptation is to seize upon various kinds of etymological and geographical information, and even the biographical data of narrators, to make a case for one relationship or another. And all too frequently, an ethnohistorian invents a methodology in an *ad hoc* manner for a particular problem, and then never uses the same methodology again (Delanglez 1941). The end purpose of such efforts, also unfortunately, seems merely to be the demonstration of a relationship between a known and an unknown group, to the satisfaction of the author and perhaps other participants in the debate.

Even when such efforts are successful, however, I feel that the more interesting question, and the more important one, is left unexamined: What kind of social, historical or *ethnic process* is represented in the relationship between two named groups? Why are they listed as separate groups on one document and a single group on another? Did one group merely absorb the other, or are there different kinds of historical processes which might have occurred? My point is that it is more important to understand the *nature* of the connection, than to recognize that a connection exists.

While the comparative examination of historical documents often leaves many named groups with no known affiliations, as in Figure 1, the opposite problem exists in examining the results of analysis in historical linguistics, as in Figure 2. In this case, as we work backwards in time, we do not have too many groups, but too few. And in fact the farther back we go, using the more-or-less standard techniques of historical linguistics, the fewer groups we have.

In the science of historical linguistics, much effort has gone into the construction of genetic diagrams or family trees — phylogenies of related languages. And we should remember that each node on a genetic diagram such as Figure 2 is supposed to represent not merely an abstract relationship, but a real language community from earlier in time. As Mary Haas put it, „Every protolanguage was in the same way once a real language, whether or not we are fortunate enough to have written records of it“ (Haas 1969: 32). In his recent book, *Language in the Americas*, Joseph Greenberg expresses the same idea, referring to each node as a „common source“ for descendent or „daughter“ languages (J. Greenberg 1987: 5).

Linguistic evidence from the Old World, however, indicates that in addition to there being protolanguages which have surviving daughters, there are also *extinct* languages which have no direct descendents, so that the total number of spoken



languages in the past is larger than that represented only by the proto-languages. But in the New World there are apparently no written records of languages extinguished before the Columbian period. Consequently we cannot look at documentary evidence to determine such things as phonological interference or loan words from these languages, as we can with linguistic evidence from the Old World. That is while we can document certain kinds of ethnohistorical and linguistic relationships between the Hittites or Sumerians (both speaking extinct languages) and their neighbors, we cannot do the same in North America (Thomsen 1984, Macqueen 1986). How many extinct languages there might have been in North America, what their structures were, and how their speakers might have interacted with proto-Algonquians, for example, or proto-Athabaskans, is now difficult or impossible to tell.<sup>2</sup> All we can say for sure is that the phylogenetic tree diagrams shoving North American proto-languages are misleading when they imply that the linguistic and cultural picture in the past was more simple than it is now. These diagrams are also misleading when they imply that the process by which new languages are created is necessarily a slow and gradual one, analagous to the doctrine of „gradualism“ in biological evolution. Alternatively, a model of linguistic change might be offered which is more analagous to Eldredge and Gould's model of „punctuated equilibrium,“ in which short periods of drastic and rapid linguistic reorganization are followed by long periods of relatively minor change (Eldredge and Gould 1972).

The archeological evidence in North America contradicts both the historical and linguistic evidence. While the historical documents indicate that there were more named groups, „tribes,“ in the ethnohistorical period than now, and while historical linguistics implies a steadily-diminishing number of languages as we retrogress in time, archeological evidence indicates that the number of distinct „cultures,“ as evidenced from their material remains, has been about the same, at least since the Archaic (Jennings 1968: 128–129, Stoltman 1978).

There is, however, an important similarity between archeological and documentary evidence. Both show *discrete* historical units which are *bounded in time*, not ones which change gradually from some ancient prototype, as in the linguistic model. Typically, a prehistoric culture known to archeologists *begins* in some geographical or ecological region, consolidates, expands and develops to maturity, and then disappears, at least in that region. Harvey, for example, has provided an excellent description of the origin, maturation and dispersion of Oneota culture in northwestern Iowa, giving careful attention to ecological factors and political environment. Like most archeologists, however, she finds it problematic to determine whether Oneota was a „new“ culture, or whether the culture-bearers had migrated from elsewhere (Harvey 1979: 220–233).

A general, underlying problem in the archeology of North America, a classic question on this continent as elsewhere, is how and why, within a framework of general cultural diversity, do discrete and identifiable cultures in a particular region arise, consolidate, grow to maturity and then disappear? This cultural question is in some ways equivalent to the obvious but profound question which Darwin asked in biology, why are there discrete species in nature, instead of a continuum of infinitely diverse individuals? In the case of culture, we want to know not only why there are discrete populations representing homogeneous cultures, but how the geographical patterns of distribution can change through time. In what cases do these patterns reflect the movements of populations, as opposed to the diffusion of culture across

societal boundaries? Apparently the situation in North America, of discrete and coexisting cultures, existed for hundreds if not thousands of years before Columbus. It seems significant that while the cast of characters has changed, the number of characters has remained about the same (compare Figures 3 and 4). And we have the problem of connecting the complex pattern shown by archeology with the linguistic picture, which shows increasing simplicity toward the past, and with the documentary picture, which shows increasing complexity, at least for the past 500 years. Related to all these problems is another classic problem in archeology: whether archeological „cultures“ are the same as ethnological „societies.“ That is, if one could travel back in time and visit Harvey's „Oneotas,“ would they be found to constitute an ethnic and linguistic unit, as well as an archeological one?

### Three Seminal Articles

I want to suggest here that the solution to many of these problems of articulation among historical, linguistic, and archeological data can be found in three explicit though much-neglected articles published over the past several decades. They are similar in their theoretical suggestions, although different in their ethnographic illustrations. The earliest and most important of these is Roger Owen's 1965 article, „The Patrilocal Band: A Linguistically and Culturally Hybrid Social Unit.“ Next in chronology is Susan Sharrock's 1974 articles, „Crees, Cree-Assiniboines, and Assiniboines: Interethnic Social Organization on the Far Northern Plains,“ and most recent is the article by Patricia Albers and Jeanne Kay, „Sharing the Land: A Study in American Indian Territoriality“ (1987). I wish to make the extravagant claim that these articles, taken together, represent a synthetic theory which has the potential for solving a wide array of problems in ethnohistory. This theory centers around what Owen called the „hybrid band.“

Owen said that the hybrid band results „when two social structures characterized by utterly unrelated languages find themselves geographically, or socially, contiguous“ (1965: 685). In his article he presents five ethnographic examples of multilingual and multicultural bands, none of them from the North American Plains, and discusses the social, linguistic, cultural, and biological consequences of hybridization. He emphasizes intermarriage as the mechanism for inter-band amalgamation.

In her article, published a decade later, Susan Sharrock considers several ethnohistorically-known „Cree and Assiniboine“ bands by reference to their ethnic, linguistic, territorial, cultural and societal characteristics. She identifies three forms of interrelationship in these bands which she calls „Early Alliance, Intermarriage and Polyethnic Coresidence and Fused Ethnicity“ (1974: 95). Somewhat hesitantly, she places these forms in a developmental sequence, although she largely draws merely the taxonomic conclusion that „interethnic social organization be interpreted on the basis of two independently varying criteria – ethnicity and coresidence“ (1974: 95, 117).

In their very recent article, Albers and Kay document in detail the multi-lingual and multi-cultural character of the so-called „tribes“ of the northern Plains and western Great Lakes in the ethnohistorical period. These authors emphasize the manner in which groups shared territory and resources among themselves, even during periods of intense warfare. Their examples undercut, to a considerable extent, the idea that named entities from ethnohistorical times, „tribes,“ can be defined in terms of an



exclusive territory as drawn on a map of political boundaries. They show instead, that the area they examine was a congeries of hybrid groups of various kinds, living together in various stages of alliance and intermarriage and in complex patterns of hostility and intermittent warfare.

The strength of all these articles is their emphasis on the band as an analytic unit, rather than the tribe or nation. All in all, I think that ethnohistorians, with the notable exception of Wood (1971: 70, Wood and Downer 1977), have exhibited a misplaced obsession with so-called tribal entities, and have neglected the importance of the component residence bands, which are in many ways more important. The „tribes“ or „nations“ which have received so much attention in scholarly literature were often merely temporary political alliances among bands which were themselves much more stable than the tribes in their membership, more complex in their structure, and more fundamental in their economic functions. It is at the band level, for example, that attention is paid to matters of ecology and domestic economy, and that the process of hybridization through intermarriage actually begins. Here extended families are structured and ranked among themselves, labor is divided in the family by age and sex, and schedules of hunting and collecting activities are worked out. It is bands not tribes which generate concerns about ecological resources, adjust their populations to suit the circumstances, and first perceive the benefits to be gained from inter-band or inter-tribal alliance.

For most of the year, national identity is irrelevant to the local nomadic band, which plans its own involvement in a variety of shared economic and social activities. And many bands, being multi-lingual and multi-cultural, could and did change allegiance from one tribal nation to another very quickly. From my own research, for example, I note five „Teton“ bands enlisting in the Cheyenne nation in ethnohistorical times, and three „Cheyenne“ bands ending the free period enrolled on reservations as Brules' and Oglalas (Moore 1987: 204–250). And I know from browsing government censuses that there are many other such examples from other reservations in these early days.

As a group, the three articles mentioned above not only draw attention to the importance of bands in general, but also show the importance of a particular kind of band, the hybrid or multi-cultural unit. They demonstrate convincingly that such groups are not mere oddities, but are a regular part of ethnohistory. George Grinnell, for example, has listed twenty-eight „foreign tribes“ who intermarried significantly with the Cheyennes, including a group of Blackfeet men, supposedly „enemies“ of the Cheyennes (Grinnell 1962, I: 39–40, 1900: 72). In reviewing the ethnohistorical literature, it seems odd that the only group whose hybrid nature has been emphasized is the Me'tis, perhaps because it is an Indian-European hybrid, not an Indian-Indian hybrid (Giraud 1986, Brown and Peterson 1985).

I am arguing here that all the hybrid situations on the Plains deserve the same attention as the Me'tis, because hybridization is not merely a peculiar and occasional event, but a major mechanism for social, linguistic and political change in the prehistoric and ethnohistorical periods. But what are the motivations for the creation of such groups? That is, when the mechanism of hybridism is unleashed, what are the motivations of the two contributing groups? And what is the particular form of the interaction? Although two mechanisms for hybridization are often mentioned in Plains ethnohistory – intermarriage and adoption – I would argue that the latter is merely a variety of the former. Seldom were foreigners adopted who were not also intermarried.

The three articles cited above concur that intermarriage was very important for creating alliances which facilitated trade and military alliances. And these two motivations — trade and alliance — are made very explicit in the historical documents themselves, both by the authors of the documents and by their informants. A third motivation, the avoidance of incest, is equally important, but more subtly and more difficult to assess.

Human societies usually rationalize the incest taboo both on biological and sociological grounds, although the categories of restricted mates are highly variant among different groups. The nomadic pastoralists of the Plains usually conceptualized their exogamy bilaterally, as the avoidance of marriage between any two persons who were known to be related consanguinally by any connection at all. By contrast, among the larger-scale, sedentary societies of the Middle Missouri and eastern North American, a clan system was maintained which rigidly defined potential mates by reference to a unilineal kin system, and thereby sustained a viable marriage pool for men and women reaching adulthood.

In a pastoral band of several hundred persons, however, incest avoidance was a more severe problem, as we can see from a consideration of the mathematics of the situation. Suppose, for example, we begin with a normal Plains marriage arrangement in which a cohort of siblings in a band marries another cohort to which they are not related. Without going through all the mathematical operations, let me only note the conclusion that after only two generations of subsequent endogamy in the band, forty years or so, everyone in the youngest generation is a sibling or cousin to everyone else, even discounting kinship extension and differences in fertility, which might make the situation occur even earlier.

Plains Indians, then, had to marry out of the band to avoid incest. And because contiguous bands usually contained relatives, a young person might have to find a spouse at considerable distance. So it is not only that young people were *pulled* into marrying „foreigners“ by the opportunities for trade and political alliance in the ethnohistorical period, they were also *pushed* by the necessity of avoiding incest.

The same kind of mathematics used to show the consequences of incest taboos can also be used to show the linguistic impact of only a few intermarrying spouses. Suppose, for example, that a band of two hundred persons comprises only four foreign grandmothers. Normally on the Plains, these would have been sisters or cousins to each other married to the sons of leading men to endorse some alliance for trade or warfare. After only four generations in a band of stable population, 120 members of the band ( $8 + 16 + 32 + 64$ ), 60 per cent, would on average be descended from one of these grandmothers. That is, it would be perfectly possible for the last surviving grandmother of the four to witness the day in which the majority of the band would be descended from „foreigners.“ Obviously, the potential is great in this situation for many of these descendants to be bilingual, especially if contact were maintained and additional spouses obtained from the foreign band.

### Hybrid Bands Among the Cheyennes

The overall importance of paying attention to hybrid bands first became apparent to me in the course of some recent ethnohistorical research, mostly reported in my book, *The Cheyenne Nation: A Social and Demographic History* (1987). When I began



my field and archival investigations, in 1980, I was only aware that one of the Cheyenne bands, the Sutaio, was said to be an adopted people, and that another, the Wotapio, was alleged by Mooney's informants to have Siouian origins. After further research, however, I discovered that not just one, but five Cheyenne bands had Siouian origins, while two others had resulted from Arapaho intermarriage, one band each were the consequence of Arikara and Mandan intermarriage, and possibly another band had resulted from Kiowa intermarriage. In fact, only one of the eleven or so Cheyenne bands, called the Tsistsistas, Bessie Band or Cheyennes Proper, was said to consist entirely of the descendents of „real Cheyennes.“

Looking at tribal nations contiguous to the Cheyennes, the situation at first glance seems to be similar. The Kiowas have one band called the „Kiowas Proper,“ in addition to comprising an „Arikara Band“ and the so-called „Kiowa Apaches“ (Mooney 1898: 227–230). The Arikaras had a „Crow Band“ and a „Cree Band“ (Fletcher 1907: 86). I suspect that thorough research into the individual band histories of other Plains tribes, beginning with the etymologies of their band names, would reveal a picture similar to that of the Cheyennes.

While investigating the cheyenne case, working slowly back through time, I discovered a period in the seventeenth century when proto-Cheyenne bands existed in a rather complex ethnic situation similar to the one described by Albers and Kay, in which many bands were apparently bilingual and bicultural. In my book I show that it was from this congeries of bands with confusing ethnic identities, each one known by several different „tribal“ names in historical documents, that the Cheyenne Nation was formed. But what do we mean specifically when we say that a nation like the Cheyennes „was formed“ or „came into existence“ at a particular time?

It is now over a decade since the publication of Morton Fried's book, *The Notion of Tribe*, and by now nearly every ethnohistorical scholar has an opinion on his criticisms of „tribe“ as traditionally defined. Many people agree with Fried that the orthodox definition, emphasizing a shared language, territory, political structure, and so on, cannot be defended within some ethnographic contexts. Others, however, myself among them, continue to insist that the idea of tribe, I use the term „tribal nation“ in my book, is useful for doing ethnohistory. My Cheyenne researches have convinced me, however, that all of us, pro-tribe and anti-tribe alike, are correct as regards the ethnohistorical period on the Plains. Some named groups on early documents are „tribes“ even by the most rigorous definitions, and others are not. While the general trend on the Plains is for nation-building in the seventeenth through nineteenth centuries, some groups were farther along in this process than others, while a few were even dissembling from a former condition of nationhood.

The question of why this cultural and political variability exists on the Plains should be conceived, I think, as an issue of dynamic *process*, not of taxonomy. That is, it is foolish to ask the question whether all the named groups on the American Plains were tribes or not. Some were and some weren't. The problem with Fried's critique of the concept is that he attempts to apply the same definition of „tribe“ to societies which are in radically different stages of development. While groups such as the Cheyennes and Kiowas had all the social requisites necessary for recognition as a tribe, and were busy obliterating former linguistic and cultural differences among their constituent bands, other groups such as the Tetons and Comanches were in much

earlier stages of the same process, still exhibiting considerable variation and disorganization. Still other groups, despite the general nation-building trend on the Plains, were rapidly disintegrating and dispersing, such as the Arikaras and Dhegiha Sioux.

One anecdote which illustrates very effectively the contrast between Anglo expectations of finding „tribes“ on the Plains, and the realities of ethnic hybridism comes from the Stephen Long expedition of 1820. In September, the expedition encountered on the Arkansas River an Indian camp comprising „... different nations here associated together, and consisting of Kiawas, Kaskaias, or Bad-hearts, Shiennes (sometimes written Chayenne,) and Arrapahoes“ (James 1823: 175). Approaching some men of the group, James continues, „One of us asked an individual, if they were Kiawas, and was answered in the affirmative; he asked a second if they were Kaskaias, and a third if they were Arrapahoes, who both also answered affirmatively“ (James 1823: 195–198).

Another striking example of heterogeneity, this one better documented, is the so-called „Dog Soldiers“ who rose to historical prominence as raiders along the Platte River Road in the Middle of the nineteenth century. They were a bilingual group, according to Hayden's report of 1863. He said: „They are at peace with the Dakotas, and have become so intermarried now, that it is hardly probable that they will ever break their friendly relation. So many of them speak the Dakota language, that their own language is not used at the present time in diplomatic affairs“ (Hayden 1863: 277). Tall Bull, a principal leader of the Dog Soldiers, was said by George Bent to have one Lakota and one Cheyenne parent, which Bent said was typical for Dog Soldiers (1968: 339). When the empty Dog Soldier camp on Pawnee Fork was captured in April of 1867, the desert tipis were inventoried in a military manner and the Indian scouts described 111 of the tipis as „Cheyenne“ and 140 as „Sioux“ (Carroll 1978: D16–172). At its height, the emerging Dog Soldier nation comprised several thousand people (Moore 1987: 197–204).

The institutional accoutrements of nationhood collected by the Dog Soldiers are also interesting. John Prowers, a white trader married into the Southern Cheyenne peace faction, reported that by 1865 the Dog Soldiers had „drawn off“ from other Cheyennes and refused to have anything to do with them (Prowers 1865: 144–145). At about this point in their development, the Dog Soldiers symbolized their separation from the Southern Cheyennes and Teton Sioux by holding their own Sun Dance in the center of their newly-defined territory in western Kansas (Creel 1879).

It is interesting and significant that Cowie also reports a Sun Dance as the signal for the establishment of a new nation by another ethnically diverse group of northern Plains pastoralists, mostly Crees and Assiniboines, in 1868 (Sharrock 1974: 112–115). Comparatively across the Plains, it seems that the Sun Dance is consistently the marker for a strongly integrated tribal polity, a tribal nation. Indeed, from a political perspective, the religious content of a Sun Dance is almost irrelevant. What is important is that the ceremony demands the attendance of the bands who are members of the nation, and Sun Dances require the arrangement of these bands into a tribal circle. After the bands have signalled their membership in the nation by arriving at the ceremonies, from a political perspective it almost doesn't matter what takes place under the ceremonial arbor. The symbolism of the tribal circle itself is important for politics, not the ritual paraphernalia. How odd that Wissler could have studied the



Sun Dance comparatively in such detail, but missed the main point – that the Sun Dance provides the main rationale for various and sundry bands, many of them bilingual and multi-ethnic, to demonstrate by their participation in the Sun Dance, their membership in a tribal nation (Wissler 1921).

## Conclusions

The very essence of the theory of ethnogenesis, as applied to North America, is the idea that the same conditions which produce a political consolidation among diverse bands also create a discrete and homogeneous language, as well as a homogeneous inventory of material culture as perceived by the archeologist. With this in mind, let us go back to the three original problems laid out in this paper, relating to the lack of fit among extrapolations to the past based respectively on documentary, linguistic and archeological evidence. If in fact the names of polities, language phylogeny, and similarity of material culture all result from the processes of ethnogenesis, then the theory of ethnos has the potential for reconciling the differences among these three classes of cultural data.

Of the three, I think that the pattern of tribal and band names appearing on early documents is most vulnerable to the theory of ethnogenesis. Why do groups appear, disappear, and reappear on early maps and documents with such rapidity? The answer I think is that they are undergoing ethnic processes of various kinds – ethnogenesis, transformations, ethnogeny and assimilation. Named groups in the documents are constantly renaming themselves, inventing new ethnonyms, or else being renamed by other groups, as a consequence of participating in ethnic processes. In my recent book, I try to show how documentary clues can be used to discover ethnic processes at work among Cheyenne bands, and I believe that there are similar documentary clues for uncovering ethnic processes among other Plains groups.

Concerning linguistic opportunities provided by theories of ethnogeny, there are considerable risks involved with abstract theorizing, in part because of the controversies surrounding the postulations of Bickerton and his supporters concerning the consequences of the multilingual situation (Goodman 1985). But nevertheless, stable multilingual situations – the primordial linguistic stew – is well documented not only for North America, but also for other continents (Rhodes and Todd 1981, Yallop 1982: 27–30, Morphy 1979: 2–7). The controversy comes in postulating that creoles and pidgins derived from this stew might represent the origins of many if not all languages and language families. So when we point to the ethnogenic situation, the hybrid band, as an entity capable of inaugurating rapid linguistic change and perhaps a new language, we must be careful to specify what kind of change we are talking about.

First of all, the ethnogenetic band, in the first generation, most likely comprises a mutually bilingual situation, rather than manifesting a pidgin or creole language, although Sharrock and Wolfart both present evidence for „the formation of a marginal (pidgin or creole) language“ (Sharrock 1974: 117). By now a great deal is known about this kind of situation, both from historical studies of written languages like English, which underwent an accelerated hybridization with French in the thirteenth century (Baugh and Cable 1951), and also from the observation of modern languages such as Chicano Spanish, in which it is possible to say, for example, „Si va take una

muchacha el dominant role," in a manner strikingly parallel to Middle English (Penalosa 1980: 58). Wolfart has noted the same kind of „extensive borrowing“ between Cree and Ojibwa (1973: 1317).

But unfortunately for those who wish to postulate that North American languages are capable of very quick phylogenetic divergence, gradualism is still deeply entrenched in the historical linguistics of North American languages. The dominant idea still is that the important nodes of Amerind languages, proto-Algonquian, proto-Hokan, etc., are several millenia removed from the present day. However, the rapidity of change in some languages observed in modern times argues that these nodes could have been much more recent, and the theory of ethnogeny suggests a social mechanism for rapid language change — a situation in which everyone is mutually bilingual, resulting in a new negotiated language, characterized by extensive changes in phonology, morphology, and syntax (Polome' 1983, Auer 1984).

The whole idea of linguistic nodes might also be in line for revision. Although much-maligned, the idea of *mischsprache* has been supported by Mary Hass, who quotes Boas as saying that „it is not possible to group American languages rigidly in a genealogical scheme in which each linguistic family is shown to have developed to modern forms, but we have to recognize that many of the languages have multiple roots“ (Haas 1969: 84, Boas 1929: 7). Of the many recent examples of promiscuous speech diffusion which might be cited, especially striking is the case reported by Rhodes, in which speakers of Mitchif exhibit a French noun morphology and a Cree verb morphology (Rhodes 1977).

For interpreting archeological data, the theory of ethnogenesis seems already implicit in many of the syntheses attempted by North American scholars. Like the ethnos, an archeological horizon is seen by North Americans to have a beginning, a period of consolidation and expansion, and a terminus when the people apparently migrate, are transformed into a new ethnos, or become assimilated into other groups. The data and description of American archeology is, I think, already highly congruent with the notion of ethnos, although the vocabulary is very different.

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- 1.1. My special thanks to Patricia Albers and Helen Tanner for personal discussions which led to the writing of this paper. For their critical comments, I wish to thank Ray Bucko, Jeffery Hanson and Susan Vehik.
  2. Personal communication, Joseph Greenberg, November, 1987.



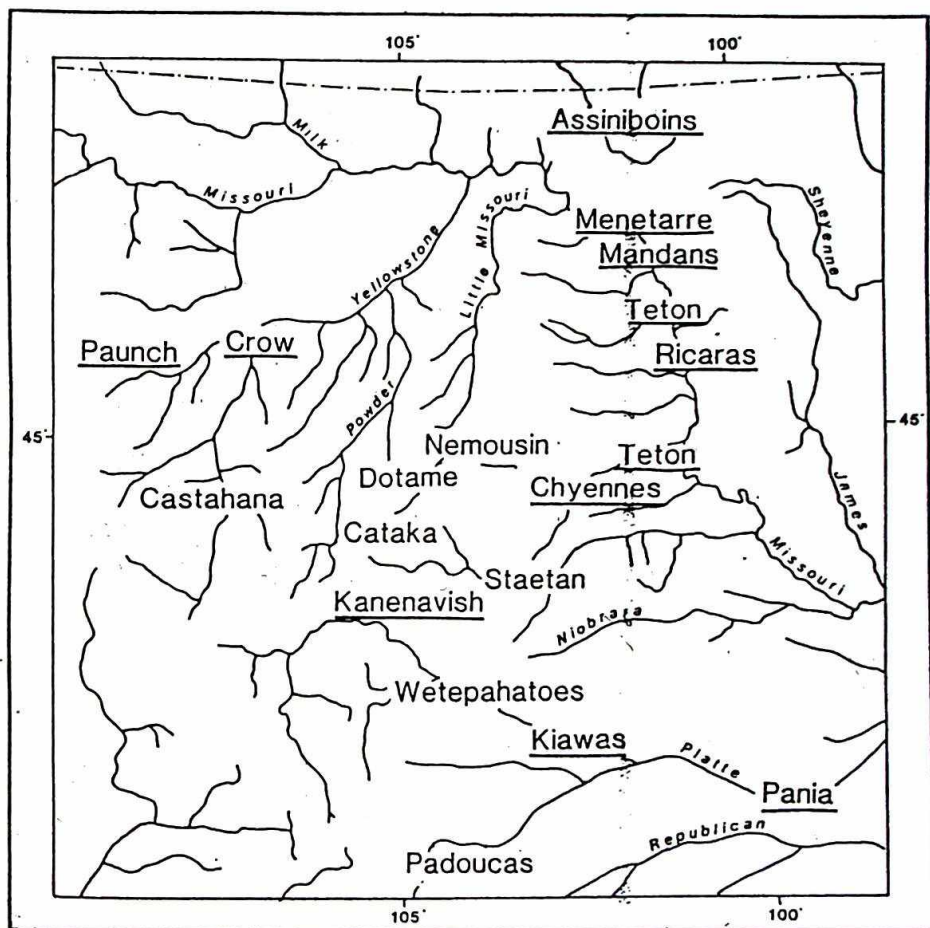


Figure 1. „Tribal“ names from Lewis and Clark (Moore 1987: 61). Underlined names have clear continuity to later historical times.

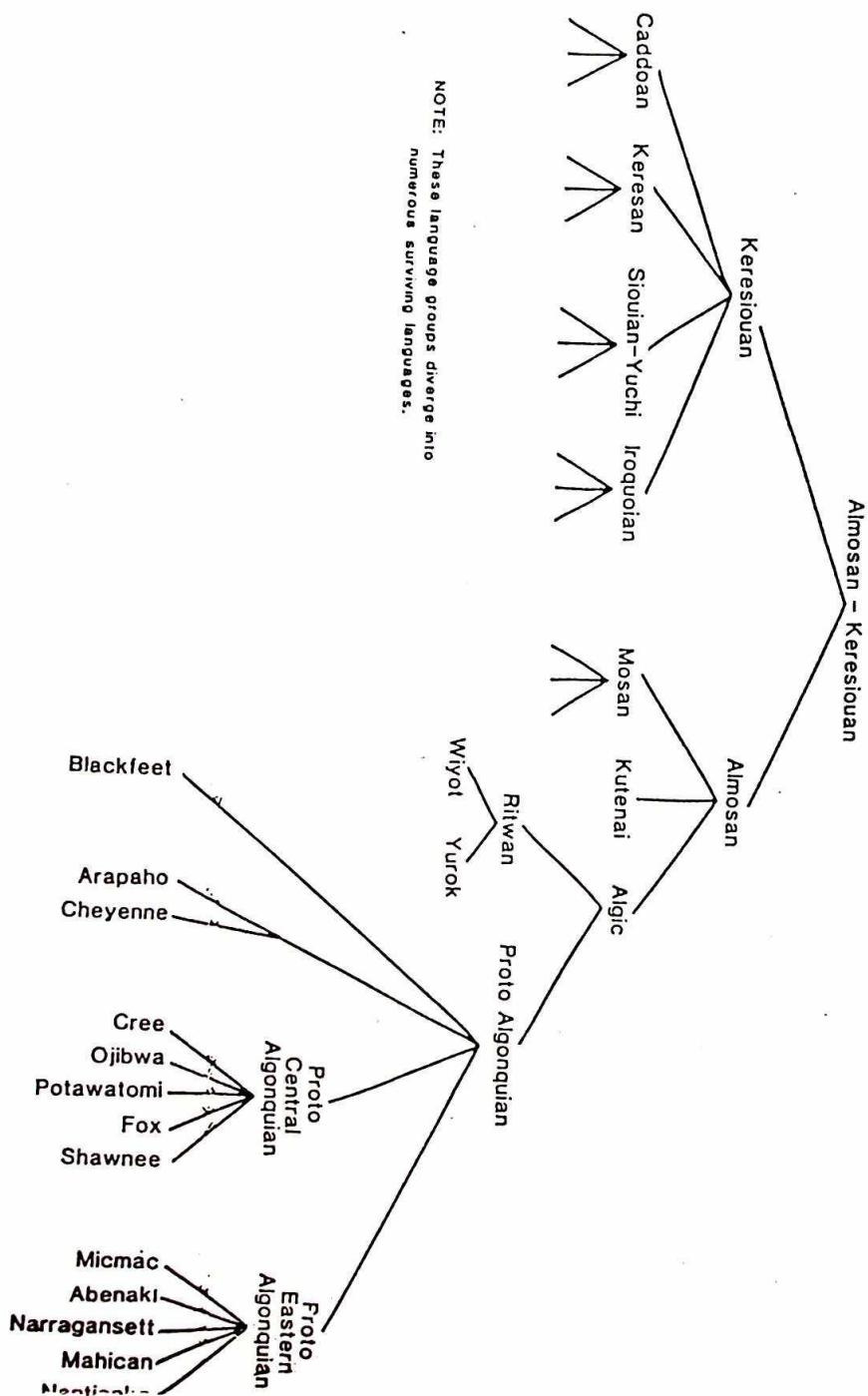


Figure 2. Phylogenetic tree of some Amerind languages. Based on information from Greenberg 1987, Michelson 1912 and Goddard 1979.



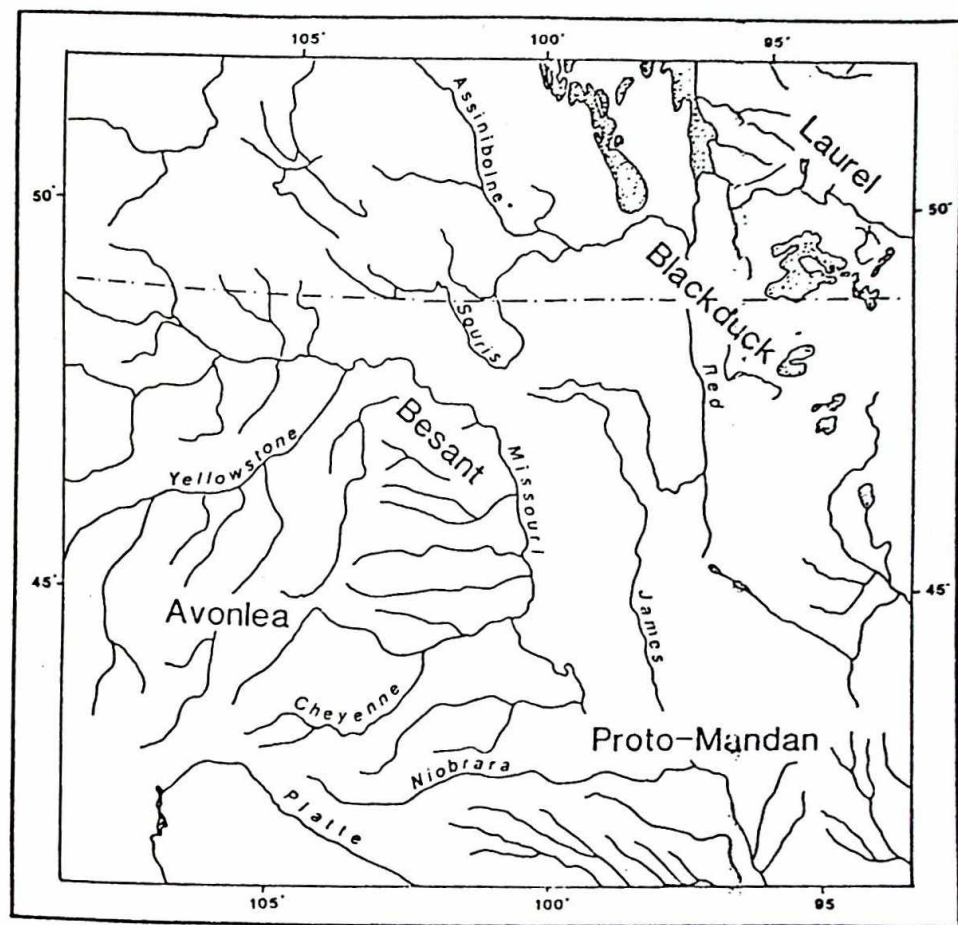


Figure 3. Archaic cultures known from the archeological record, from Schlesier 1987: 113–115.

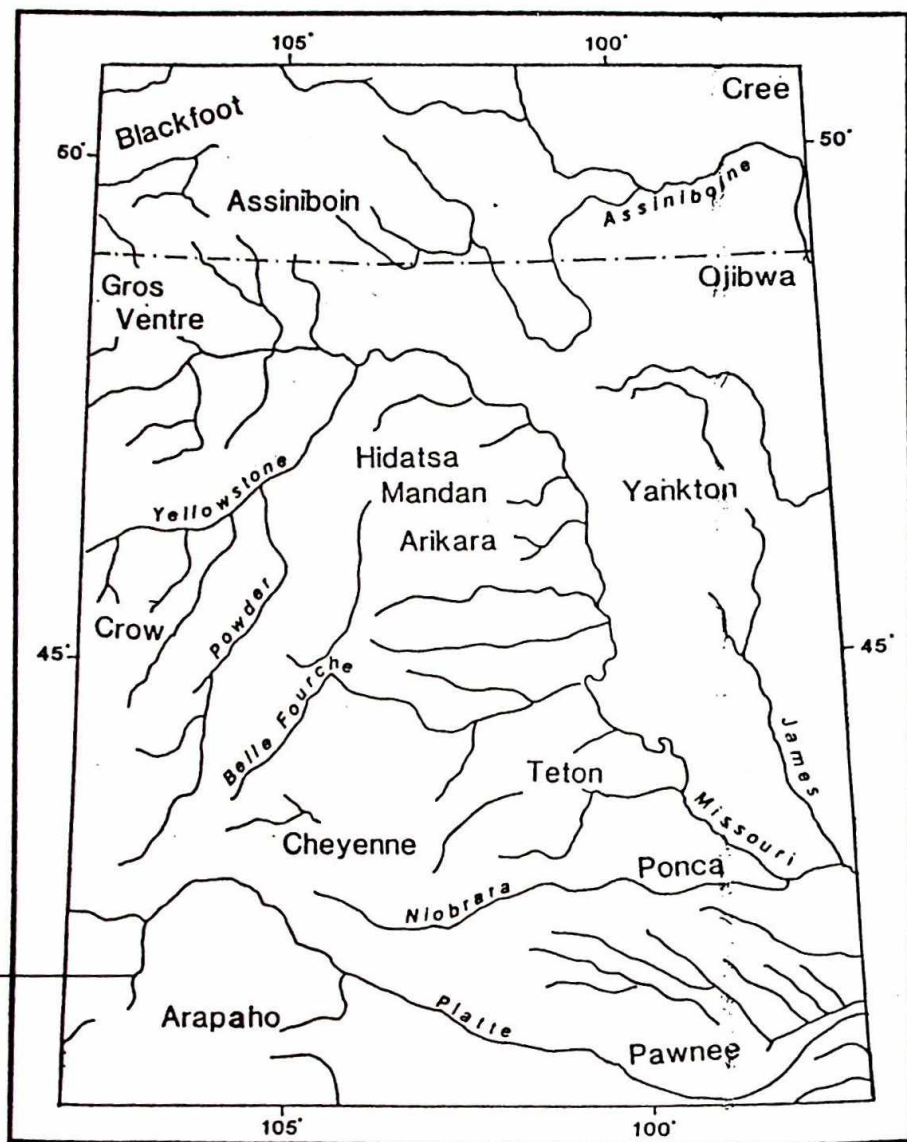


Figure 4. Historically-known Plains societies, according to Murdock and O'Leary 1975: 2.



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