Ethnohistory's Ethnohistory: Creating a Discipline from the Ground Up

Michael E. Harkin

Social Science History, Volume 34, Number 2, Summer 2010, pp. 113-128 (Article)

Published by Duke University Press

For additional information about this article
http://muse.jhu.edu/journals/ssh/summary/v034/34.2.harkin.html
This article examines the first decades of the field of ethnohistory as it developed in the United States. It participated in the general rapprochement between history and anthropology of mid-twentieth-century social science. However, unlike parallel developments in Europe and in other research areas, ethnohistory specifically arose out of the study of American Indian communities in the era of the Indian Claims Commission. Thus ethnohistory developed from a pragmatic rather than a theoretical orientation, with practitioners testifying both in favor of and against claims. Methodology was flexible, with both documentary sources and ethnographic methods employed to the degree that each was feasible. One way that ethnohistory was innovative was the degree to which women played prominent roles in its development. By the end of the first decade, the field was becoming broader and more willing to engage both theoretical and ethical issues raised by the foundational work. In particular, the geographic scope began to reach well beyond North America, especially to Latin America, where archival resources and the opportunities for ethnographic research were plentiful, but also to areas such as Melanesia, where recent European contact allowed researchers to observe the early postcontact period directly and to address the associated theoretical questions with greater authority. Ethnohistory is thus an important example of a field of study that grew organically without an overarching figure or conscious plan but that nevertheless came to engage central issues in cultural and historical analysis.

The field of ethnohistory has a past that is both relatively brief and conflicted, as befits what is commonly thought of as the illegitimate product of two sometimes antagonistic disciplines, anthropology and history. United in
what might be described as a marriage of convenience, both fields have produced a long line of defenders of the faith, who make periodic maledictions against the other. Thus E. E. Evans-Pritchard (1962: 190–91) ironically quotes Frederic Maitland’s dictum that “anthropology will become history or it will become nothing,” while E. P. Thompson, who had in fact tired of similar dire predictions, wittily imagines Clio in a swoon, ravished by the social sciences (quoted in Stone 1981: 31). However, as we anthropologists know from reading our Hayden White (under the covers, with a flashlight), much depends on emplotment; perhaps the two antagonists might be imagined under the sign of comedy as Beatrice and Benedick or as the underdressed principals of a supermarket romance novel. We know, certainly, that anthropologists such as Franz Boas and Claude Lévi-Strauss were possessed of a strong fascination for history, albeit after their own fashion. Lévi-Strauss, in particular, reimagined anthropology as a historical science but one that would supersed history (in part by its ability to project into the future) as queen of the human sciences (see Harkin 2009). On the other side, historians from the 1920s onward, such as Marc Bloch, Lucien Fèvre, and Fernand Braudel in France and later, under the sign of the New Cultural History, Robert Darnton, Natalie Zemon Davis, and Dominick LaCapra in the United States, gladly appropriated many of the methods and theories of ethnology.

While arguments can be made for both enmity and amity between the two social sciences, I would make two points about their structural relationship. First, the traditional division of labor between them in the North American context has very much been a product of the ideology of settler colonialism. Thus anthropology was envisioned as a means of getting to “know” the indigenous people of the land and, by extension, the land itself, bolstering otherwise shallow claims to proprietorship (Yanagisako 2005: 82–83). Archaeology, ethnology, and physical and linguistic anthropology all arose, and became linked, out of a desire to gain knowledge about, to categorize, and to control American Indians. While it is true that anthropologists from the 1880s onward frequently took the “liberal” position in the political debates of the age (e.g., whether American Indians were bound to disappear in the face of a superior civilization or whether they could cope in some fashion), they nevertheless tended to frame their arguments in terms of the ideology of settler colonialism. (Even Boas resorted to this discourse in his defense of the potlatch as a banking system.) History, however, was equivalent to documentary history, perforce that of “civilized” peoples. American
Indians are not entirely absent from the picture, as the works of George Bancroft and other nineteenth-century historians attest, but they are secondary to the story of Euro-American settlement of North America.

The second structural condition is the reimagining of the American academy that took place after World War II. The great expansion occurring in the United States created opportunities to redefine the nature of the social sciences and the boundaries between individual disciplines. Most famously at the Social Relations Program at Harvard University, founded in 1946, but elsewhere as well “it was . . . social science in full cry; headier and more confident than before or since” (Geertz 1995: 100). Notably absent from this heady mix was history, which had traditionally held a superior position in the academy (one that it has arguably recaptured) but was at that moment left out of the push to create an integrative social science. *Science* is of course the operative word here, as that was clearly the preferred side of the fence dividing C. P. Snow’s “two cultures” in the postwar period. The situation is thus very close to that in France in the same era, when an anthropology holding aloft the mantle of science sought to overthrow a humanist history, increasingly seen as losing relevance in a modern world (Kambouchner 2009).

However, these considerations provide at best a broad context for understanding what I take to be my real object of analysis, a field that identifies itself not as historical anthropology, anthropological history, or the like but specifically as “ethnohistory,” a term that serves as the name of a society and a journal. Obviously, such distinctions are arbitrary and difficult to maintain. Marshall Sahlins, who would be considered by most anthropologists a great practitioner of historical ethnography but not an ethnohistorian, served as president of the American Society for Ethnohistory (ASE) in the late 1990s. Lévi-Strauss has many works of ethnohistory (both the journal and monographs) on his shelves. Perhaps such examples belong to the same class of phenomena as African lineage politics, as described by Evans-Pritchard and Meyer Fortes: the greater the ancestor, the more people he or she unites as people work harder to form a connection to him or her. It is also a product of ethnohistory’s own coming of age in the 1980s, as described so well by Shepard Krech III (1991). As a younger generation—removed from the field’s origins in the days of the Ohio Valley Historic Indian Conference (OVHIC) and trained in a more theoretical and interdisciplinary manner—emerged, it brought its own set of symbols and imagined ancestors.

The question of the boundary between ethnohistory and other forms
of historicized anthropology or ethnologized history is not trivial, and it is a theme to which I will return. However, the best way to understand the particularity of ethnohistory is to examine its own early history and institutional context. I would argue that this is located not in the academy but in the field and, particularly, in courts of law. The interests of the founding generation were overwhelmingly shaped by the pragmatic concerns of claims in the context of the Indian Claims Commission (ICC), established in 1946 to adjudicate indigenous land rights (see Shoemaker 2002: viii). Moreover, this specialization represented a creative space for those who were not such partisan practitioners of their own field but who were united in an intellectual and often ethical interest in American Indian cultures and the political position of American Indian communities in the American (and, to a lesser degree, the Canadian) polity. These scholars tended to be empirical rather than theoretical, practically minded rather than mandarins. In a period when working for the federal government was the norm for social scientists, but as a sideline to their academic appointments, ethnohistorians took on the engagement with the federal political-legal system as their calling. Moreover, unlike the often cozy relationships that mandarin scholars such as Clyde Kluckhohn enjoyed with their government employers, ethnohistorians frequently took the opposite side in court, arguing against the government and for American Indian land claims (see Geertz 2002: 4).

In *Making Indian Law* Christian McMillen (2007) claims to have identified “the birth of ethnohistory,” as his subtitle perhaps overdramatically has it. It is to be found, in particular, in the research and litigation on the Hualapai land claim case, which preceded and led directly to the establishment of the ICC. While it is really the ICC proper and not the Hualapai case that defines the gestational period of ethnohistory as a discipline, McMillen correctly identifies the defining case in which the DNA of the field is established. The Hualapai, one of the groups occupying the lower Colorado basin, including the Grand Canyon, were seminomadic and, as such, poorer candidates for having their claims recognized than were groups such as the Cherokee, who, despite practicing sedentary agriculture, in fact lost most of their aboriginal lands in the 1830s. The Hualapai, however, benefited from the more enlightened government policies of the New Deal era, personified by Felix Cohen of the Bureau of Indian Affairs, and the evolving body of scholarship at the intersection of anthropology, history, and law in the 1930s. Armed with this new scholarship, one could argue what once had been con-
sidered absurd: small egalitarian bands of seminomadic American Indians, traditionally lacking both writing and a concept of property cognate with the Anglo-Saxon one (both of which the Cherokee, by contrast, possessed), could establish legal claim to traditional territory.

These two fundamental problems—documentation and property—defined the conflict between Anglo-American property law and native land claims. The first applied to all native groups: the lack of documentation of any claim that predated European contact (Cherokee and Cree syllabaries were established after contact). A subsidiary problem was the inherent ethnocentric bias in the documents that did exist. Such documents frequently underreported the degree to which land occupied by American Indians was being actively managed, either through agriculture or the maintenance of fisheries game preserves (with fire often used to modify landscapes), the structures placed upon the land, and the vitality of the groups themselves. All of these points suggest the availability of land for settlement, land which was barely used by groups who were, in any case, inevitably disappearing with the advance of civilization. The second problem was the concept of property itself and its limitations when applied especially to nomadic or seminomadic native groups. It is not surprising to discover that the Hualapai, unlike the Cherokee of the 1830s, possessed no cultural concept analogous to fee simple in English common law. However, that is not to say that a concept of rights to territory and resources was entirely absent, even if it was vested in a group, not an individual, and even if it at times allowed for sharing with other groups. Both of these problems were amenable to ethnohistory’s tool kit, which understood land use and occupancy to be a complex and culturally specific phenomenon and which had the means to push back the horizon of what could be known historically.

The cases heard by the ICC, which existed from 1946 through 1978, often turned on definitional questions, such as what constituted an “identifiable group” and whether sufficient historical continuity existed between such groups that signed treaties, often in the distant past, and present petitioners. For instance, in the Indians of California case, it was found that, although the petitioners were in part descendants of the signatory tribes mentioned in the Treaty of Guadalupe Hidalgo, they were a different entity and not an “identifiable group.” That is, the dislocations of the past century—missionization and subsequent settlement on Rancherias—disqualified descendant groups from land claims, because they did not constitute an “identifiable” Amer-
can Indian group (ICC n.d., 1: 372). American Indians, to remain such, must largely exist outside history or be seen to be so. Ethnohistorians were thus placed in a difficult position of mediating between American Indian and Euro-American notions of history.

To get a sense of how formidable this challenge was, one can recall that many American archaeologists continue to this day to speak of the period before European contact for any group as “prehistory.” Depending on the place, this could mean pre-Columbian or pre–Alaska gold rush. This profoundly ethnocentric formulation in essence denied history and historicity to indigenous peoples. Only once they had been observed were they considered to be in the main current of history, albeit as minor participants. Oral testimony of any sort has been considered much less reliable in both history and law. This influenced the first generation of ethnohistorians, who based their analyses primarily, if not exclusively, on documentary sources. This research in fact opened new horizons, as much of this material came from archives that had only begun to be exploited. Thus exemplary work by scholars such as William Fenton (1949) began in the 1930s and 1940s to examine explicitly historical topics using a range of previously unknown sources. Indeed, one important strain of ethnohistory in the 1950s and early 1960s was the location and publicizing of collections of documents in local courthouses, historical associations, and the like. Ethnohistory published both primary documents themselves and, more frequently, summaries of the sorts of documents likely to be found in various locations. Conferences and symposia seemed largely taken up with these matters. Thus in his 1960 address to what was then called the American Indian Ethnohistoric Conference, James H. Rodabaugh (1961), a historian with the Ohio Historical Society, gave an overview of depositories likely containing ethnohistorical documents in Ohio.

At the same time, the journal began to have a broader reach in terms of both theoretical and geographic topics. In the same volume as Rodabaugh’s piece, Andrew P. Vayda (1961), who would become an important figure in ecological anthropology, published a work of historical ethnology on the topic of Maori war captives. Although one might be tempted to consider this an example of blatant opportunism in publishing, it instead reveals the paucity of outlets for any sort of historical anthropology, a situation mirrored in the discipline of history. In this era we begin to see the trajectory that the field and the journal will take. Founded on the rock of pragmatically oriented
Ethnohistory’s Ethnohistory 119

American Indian research, it becomes a safe haven for all sorts of scholarship operating at the intersection of history and anthropology.

*Ethnohistory* filled a definite gap; the ignoring, if not active suppression, of historical studies was characteristic of the flagship journal *American Anthropologist* prior to the 1960s. The few diachronic studies published in its pages were tied to paradigms that were themselves ahistorical or antihistorical, such as acculturation or structural functionalism. The two articles that contained the word *history* or *historical* in their titles and appeared in the modern editions of selected papers from the *American Anthropologist* covering the period 1921–70 (Murphy 1976; Stocking 1976) are Fred Eggan’s (1937) Radcliffe-Brownian analysis of Choctaw kinship terminology and A. L. Kroeber’s (1935) discussion of Radcliffe-Brown. Neither of these articles employs the term in a way that would be embraced by either historians or ethnohistorians. Rather than specific history, we have an attempt to recapture certain aspects of diachronic process in synchronic datasets (primarily kin terminology). Of course, one could argue that Anthony Wallace published in the *American Anthropologist* in the 1950s, but the hook in his work was clearly his use of Freudian and ethnopsychological theory and thus its resonance with interdisciplinary social science, not its historical dimension.

Indeed, the very area that interested most of the first generation of ethnohistorians, American Indian cultures, had been proclaimed by the founding fathers of American anthropology to be an unfit subject for historical study. In the preacademic phase, historical studies had been a staple of Bureau of American Ethnology publications, most notably in the work of James Mooney (1975, 1991). However, everything changed in the wake of the migration of professional anthropology from museum to university. Robert Lowie bluntly asserted, very early in the game (1915), that one cannot “attach to oral traditions any value whatsoever under any circumstances whatsoever,” because “we cannot know them to be true” (quoted in Krech 1991: 345). Thus denying a historical consciousness to these people, Lowie precludes the professional scholar from considering what had been of great interest and would again become the essence of the ethnohistorical method. Lowie’s proclamation of course begs the question of when personal memory ends and “oral tradition” begins; for one so dependent on the former, it seems reckless to dismiss the latter. Kroeber, while certainly interested in diachronic process, not just in kinship terminology but in hem lengths and other ephemera of American
culture, scoffed at the ethnohistory of California as “the little history of pitiful events” (quoted in Buckley 1996: 257). Much of this can be traced, of course, back to Boas, whose “historical particularist” theory and method in fact adopted only the concept of contingency from historicism (see Lewis 2001). While employing research methods very similar to those of the ethnohistorians (who themselves often learned them at the feet of Boasians), Boas and his students engaged in “salvage ethnography” were interested in documenting an idealized life that existed before the reservation. The historical and psychological naïveté of this approach must have been evident to many in that era of Sigmund Freud and Oswald Spengler; the idea that one could simply “leapfrog” past traumatic and even genocidal events to an objectively remembered childhood is methodologically far more suspect than trusting in oral tradition. Boas well into the 1930s continued to collect material on the prereservation life of the Kwakiutl while carefully blocking out all evidence of historical change in the living community around him (Harkin 2002). By this point the term *salvage ethnography* takes on a surreal quality, with its undertones of religion (*salvation*) and primitivism (*savage* is equivalent to *salvage* in seventeenth-century usage).

If the period from roughly 1900 through World War II was notable for the ahistoricism of academic anthropology, by the 1940s that was beginning to change within the very bastions of Boasian anthropology. Most notable among the transitional figures was William Duncan Strong, an archaeologist trained at the University of California, Berkeley, who taught at the University of Nebraska before spending most of his career at Columbia University. An innovator who developed the “direct historical approach” in archaeology with his student Waldo Wedel, Strong later offered a highly influential seminar on historical anthropology at Columbia (Solecki and Wagley 1963). His primary field of research was in Peru, but he conducted considerable fieldwork in North America, including a stint of ethnographic fieldwork with the Naskapi in the late 1920s. Although his official position was that historicism and functionalism were compatible, his students took from him a strong historicist bent. Many of these students, such as Sally Falk More, Ralph Beals, and Oscar Lewis, later worked in Latin America. Eleanor Leacock, who was charged with editing Strong’s Naskapi material, conducted fieldwork with the group, by then known as the Innu (see Leacock 1994). Her 1954 dissertation on Innu hunting attracted attention for its historical method and for its recognition of the fur trade as a fundamental factor shaping Innu life.
Ethnohistory’s Ethnohistory

(Leacock 1954). Although later known primarily as a Marxist feminist and an expert on Lewis Henry Morgan, Leacock was an important founding figure of the ASE.

*Ethnohistory* was founded by another female scholar, Erminie Wheeler-Voegelin, in 1954 as the official publication of what was then called the OVHIC (Tanner 1991). Wheeler-Voegelin had trained at Berkeley and then, as she and her husband, Carl Voegelin, became beneficiaries of the pharmaceutical mogul Eli Lilly’s interest in Indiana antiquity, at Yale University. At Lilly’s urging, they established themselves at Indiana University in Bloomington, with Wheeler-Voegelin taking on the directorship of another Lilly-endowed project, the Great Lakes–Ohio Valley Research Project. This project arose to fill the gap in ethnohistorical knowledge for the region in the context of the ICC, established in the wake of the Hualapai case. The Ohio Valley and Great Lakes region presented methodological and theoretical problems distinct from but comparable in degree to the Hualapai. In particular, the connecting of remnant populations with eighteenth- or nineteenth-century documented groups was a formidable challenge. Indeed, the vast majority of the research conducted under the auspices of the project, presented at the conference, or published in *Ethnohistory* had direct application to ongoing litigation. It is not an exaggeration to say that the ICC provided a total context for ethnohistorical research in the 1950s. For many, it intersected with Sol Tax’s (1952) call for an “action anthropology”: a politically engaged applied anthropology at the service of disempowered minorities, especially American Indians, which had been exemplified by Tax’s University of Chicago Fox Project. This was the era not only of the ICC but also of congressional efforts to “terminate” tribes, that is, to sever the federal trust relation (Nash 1988: 270).

However, not all anthropologists or ethnohistorians shared this view. Julian H. Steward, a central figure in the development of neo-evolutionism and cultural ecology, was an expert witness for the Justice Department (i.e., the side opposed to land claims), because he believed that the concept of private property could not be applied to nomadic or seminomadic bands such as the Paiute (Rosen 1977: 567; McMillen 2007: 175; Pinkoski 2008). In particular, Steward (1972: 101–8) held that Great Basin indigenous groups operated at the “family level of sociocultural integration,” which fact precluded them from land ownership (see Pinkoski 2008). Indeed, several prominent ethnohistorians took this position, for reasons of political or theoretical conviction, including Wheeler-Voegelin herself (see Tanner 2007: 185–86). This
argument of course reproduces on a social-theoretical level the old concept of *terra nullius* and associated notions of the availability of land that was neither defended nor “improved” (Culhane 1998). This constituted the originary fault line in the society—one that remained well into the 1970s, when it was replaced by new ones involving many of the same figures (Krech n.d.). In two articles in the journal Robert Manners (1956, 1957) set out his position in opposition to the Hualapai land claim, a position forcefully challenged by Nancy Oestreich Lurie (1956), another important figure in ethnohistory. The two attended a symposium on anthropology and American Indian claims litigation that was held during a meeting of the OVHIC in Detroit. Manners bemoaned the state of affairs by which a collegial society and the larger profession of anthropology were being torn apart by the politicized role-playing that accompanied litigation. The witnesses for the American Indians were “self-righteous[,]” while those for the Justice Department were “belligerent[ ]” (Manners 1956: 72). At the same time, he laid out an argument accusing those arguing for the land claims of non-sedentary groups, such as the Hualapai, of intellectual and legal fraud, for how could a “fictional” entity (i.e., a tribe) present a case against the federal government (Manners 1957; McMillen 2007: 176)? Manners accused the pro–American Indian witnesses of being naive and theoretically unsophisticated, self-righteous, and swayed by a vestigial sympathy with the American Indian, inherited from the field’s antiquarian past. Lurie (1955, 1956), commenting on Manners’s commentary from the other side of the fence, saw a much different picture of collegial cooperation, even across the hardening boundaries of expert witnessing. However, her own animosity toward Manners (and the unnamed Wheeler-Voegelin) rather undermines this picture. In fact, several conferences during the 1950s brought representatives of the two sides together, including a symposium sponsored by the American Anthropological Association (Lurie 1956). These events turned less on what would now be called “political correctness” than on methodology and the scholarly value of research done for legal cases.

In this way, ethnohistory was forced to reckon with a set of epistemological and ethical issues, and thus with questions of identity and boundaries, in a more direct way and at an earlier point than other new disciplines. In 1960, less than a decade after the founding of the society and a mere half decade after the first journal issue, a symposium on the concept of ethnohistory was held at the annual meeting in Bloomington, Indiana. A broader,
more potentially destabilizing discourse could not be imagined. However, the results were somewhat less than promised. Lurie 1961a, “one of [her] weaker works” (pers. com., November 1, 2007), is a charming if unambitious call for anthropologists to become more aware of the expanding base of documentary evidence (including photographs) and the theoretical sophistication of contemporary historiography. (What is charming and memorable about this piece is Lurie’s recollection that during her dissertation defense she was asked whether an ethnohistory of Paris was possible.) In a similar vein, Wilcomb Washburn (1961) argued for ethnohistory as “history ‘in the round,’” consisting of a rapprochement between history and ethnology; interpretation of documentary evidence should be more ethnologically informed, while anthropologists should make use of the documentary sources available to them. Washburn (ibid.: 32) explicitly compared the development of ethnohistory to that of philosophical pragmatism in that both represented “merely a method, and not a new one at that, as [William James] pointed out.” At the same time, Washburn noted that ethnohistory must not represent merely the erasure of disciplinary boundaries, as both history and ethnology had well-developed theories and methods. Moreover, history being the province of the Euro-American and ethnology that of the American Indian, their newfound relationship could both mimic and illuminate the relationship between the two groups that shaped much of American history. In a commentary on the symposium, Leacock (1961) commended this rapprochement while urging her colleagues to undertake a more theoretical enterprise, located in what she called the “analytic” middle ground between descriptivism and abstract theorizing.

The year 1961 was also that of the American Indian Chicago Conference (AICC), organized by Tax. This event, generally considered a successful collaboration between academic anthropologists and American Indian activists, nevertheless stirred the ethnohistorical pot some more, introducing the specter of American Indian “militancy.” A quarter of a century later, Washburn (1985) accused Tax of ethical misconduct in organizing the conference and in his conduct of the Fox Project. However, Lurie’s (1961b) extensive report included a range of photographs and primary documents (no doubt with an eye toward future ethnohistorians), making the claim that the conference was productive on many fronts. The AICC certainly represented a new phase in the development of the relations between academics and American Indians, the political climate of American Indian activism, and the field of
ethnohistory. This new era saw the continuation and deepening of many of the fissures that arose in the field’s first decade and, in particular, between pro– and anti–American Indian claims (which went far beyond land claims in the 1960s) and politically committed versus detached styles of scholarship.

Despite these fissures, the field itself was taking shape as a hybrid that nonetheless possessed a strong sense of identity. (One need not be a reader of Homi K. Bhabha to realize that hybrid identities are often the most forcefully defended.) Lacking the institutional resources that history or even anthropology had at its disposal, much of the camaraderie of the meetings of what became the ASE flowed from the very sense of shared marginality. Another decade passed before “Indian history” became an established specialization within American history (with departments at western public universities, such as those in Oklahoma, Colorado, Texas, and Wyoming, providing some of the first positions in this area). The ethnology of Native North America, while still active (and indeed experiencing something of a golden age in Canada, Alaska, and northwestern Mexico), was nonetheless suffering from a sense of being passé. As Barbara Pym (1986), the British novelist who imagines a journal called *Ethno-history* (unaware that such a one did exist in America), put it, all of the smart students were going to Africa (or Melanesia, Indonesia, or Morocco), leaving behind the presumably well-plowed fields in which the discipline got its start.

The shortcomings of the ethnohistory of this era have been well documented (see Krech 1991). Indeed, it seems to have been unnecessarily conservative, especially in its suspicion of oral history (a methodology already well established in folklore studies and Africanist anthropology) and in its lack of critical readings of documentary sources, which tended to be used primarily to establish facts rather than to understand the dialogic dimensions of cultural encounter and exchange. At the risk of sounding overly presentist, I would generally agree with Krech, the editor of *Ethnohistory* who more than anyone else was responsible for the field’s full frontal assault on interpretivism, structuralism, feminism, postmodernism, and other isms of 1970s–80s anthropology, that the era was “relatively quiet. In retrospect, it was on the cusp of an intellectual moment of far greater interest, and it prepared the ground, as it were, for an explosion in interest in the play between anthropology and history, and therefore in ethnohistory, in the 1980s” (Krech n.d.: 18).

This Kuhnian view, flattering though it may be to members of my own intellectual generation, is only a partial truth. While the field proceeded
Ethnohistory’s Ethnohistory

gradually, it did progress. It expanded from being almost exclusively concerned with Anglo– and Franco–North America to having a strong interest in Latin America (facilitated in the early days by collective studies, such as the Harvard Chiapas Project). By the 1970s, 70 percent of the articles published in *Ethnohistory* were on North America, 13 percent on Latin America, and 17 percent on the rest of the world (Krech n.d.). The percentages for the latter categories were much higher than they had been in the 1950s and would expand further. The use of oral history and sophisticated modes of interpretation was similarly present but not yet dominant in the field. In this context Raymond Fogelson emerges; his importance as a teacher and scholar is great, and his positioning at the University of Chicago during its most dynamic era is central (see Kan and Strong 2006). Fogelson (1985) coined the term *ethno-ethnohistory* to highlight those aspects of historical consciousness and interpretation that were rarely considered in the early days. In addition, through Fogelson many ethnohistorians became aware of his Chicago colleagues practicing their own versions of historical anthropology, notably Sahlins, Bernard S. Cohn, Jean Comaroff, and John Comaroff. Cohn (1980) coined the memorable phrase *proctological history*, whereby everything is considered from the bottom up.

The present study has not exactly been history from the bottom up, and one wonders whether such a thing would be possible in any study of a field of academic endeavor, where the elites (many of whom may indeed be proctologically inclined) dominate the discourse. Rather, as a sort of provisional autoethnohistory, it perhaps gives a sense of the identity and historical consciousness of a scholarly society. This seems particularly important in the case of ethnohistory, whose present appearance is that of a globalized, multidisciplinary locus of discourse. Special issues of *Ethnohistory* on Madagascar, Papua New Guinea, and other previously exotic locales, as well as an eclectic and sophisticated theoretical tool kit employed by many authors (both phenomena have raised some hackles among the old-timers), make comparisons with *Subaltern Studies* seem more likely than those with *Western Historical Quarterly* or *Plains Anthropologist*. However, the roots of the society in methodological but not theoretical innovation, pragmatism, applied study and expert testimony, and American Indian political activism must be acknowledged and reckoned with.
Notes

1 It is important to realize that the present-day testimony of anthropologists and historians about what many perceive to be a “golden age” of ethnohistory is likely to be biased toward a picture of cooperation rather than conflict. Additionally, the structural conflict between history and anthropology in the context of the post-war academy is no longer relevant in the United States. Interestingly, however, the French government has proposed locating anthropology in history departments.

2 The ICC did award about $2 billion to plaintiffs. However, it did not award land (Nash 1988: 270).

3 In addition to Wedel, who became the dean of Plains archaeology, Strong’s students at Nebraska included Loren Eiseley, the great anthropological essayist.

4 This represents an interesting reversal of the commonplace assumption in the social sciences since the 1970s that high theory and progressive political causes go hand in hand.

5 Lurie states that “the antagonism died away” after the early years of ICC litigation and that both sides found common ground in their frustration with the process, lawyers, and so forth (pers. com., November 1, 2007).

References


