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Résumé de l'article

La tradition anthropologique nord-américaine, dans son ensemble, contraste vivement avec les anthropologies qui se sont formées ailleurs en Occident, et constitue une partie distincte de l'anthropologie contemporaine à l'échelle mondiale, partie qui nous est accessible par le biais de l'anthropologie historique. Dans cet article, je plaiderai pour une approche historique comme partie intégrante de la théorie anthropologique, et examinerai un moment spécifique de l'histoire de l'anthropologie nord-américaine tel un microcosme des différences entre les traditions britannique et nord-américaine.

The Boasian Text Tradition and the History of Anthropology

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La tradition anthropologique nord-américaine, dans son ensemble, contraste vivement avec les anthropologies qui se sont formées ailleurs en Occident, et constitue une partie distincte de l'anthropologie contemporaine à l'échelle mondiale, partie qui nous est accessible par le biais de l'anthropologie historique. Dans cet article, je plaiderai pour une approche historique comme partie intégrante de la théorie anthropologique, et examinerai un moment spécifique de l'histoire de l'anthropologie nord-américaine tel un microcosme des différences entre les traditions britannique et nord-américaine.

The North American anthropological tradition, as a whole, contrasts sharply with the anthropologies which grew up elsewhere in the Western world, and forms a distinct part of contemporary anthropology on an international scale, a part which is accessible to us through disciplinary history. In this article I first argue for an historical approach as integral to mature disciplinary theory, and will then examine a specific moment in the history of North American anthropology as a microcosm of the differences between the British and North American traditions.

The Canadian Anthropology Society¹ has come a long way toward professional maturity since I first read a plenary paper at its annual meeting more than fifteen years ago. Back then, I attempted to argue that Canada had a distinct and distinguished national tradition of anthropology (Darnell 1975).² The argument was not well received, particularly by students. Apparently in those days, Canadian anthropologists were so threatened by imported American theories and academic personnel that they were willing to consider themselves as merely ethnographers. Indeed, the national association then called itself the Canadian Ethnology Society.

A year later, at the annual meetings, I read a paper detailing Edward Sapir's contributions to the institutional development of Canadian anthropology (Darnell 1976). Again, much of the response centred around Sapir's training by Franz Boas in the United States, his return to the United States in 1925, and his consequently assumed non-Canadianness. The implication seemed to be that a non-Canadian in this sense could not contribute to the institutional and theoretical development of Canadian anthropology; the question of the data base in the study of aboriginal peoples of Canada was not addressed.

In fact, however, Boasian anthropology developed without much reference to the national boundary. Both Boas and Sapir were born in Europe. The Division of Anthropology in Ottawa, located in the Victoria Memorial Museum and administratively under the umbrella of the Geological Survey of Canada, flourished under Sapir's founding leadership from 1910 to 1925. The Division was inextricably part of the Boasian network of North American anthropology. Its fieldwork mandate was the native peoples of Canada. Many of its employees under Sapir's leadership were American and/or American-trained and many students of Canadian native peoples outside of the Ottawa organization were also Americans. Boas's own fieldwork was done on Baffin Island and in British Columbia. There was a Canadian on the initial board of the *American Anthropologist*. Both in Canada and in the United States, anthropology developed around the study of the aboriginal peoples of the continent. Much of the canonical fieldwork, and reasoning from it, developed on the Canadian side of the border. I want to examine this North American tradition as a whole and to argue that it contrasts sharply with the anthropology which grew up elsewhere in the Western world, that it forms a distinct part of contemporary anthropology on an international scale, a part which is accessible to us through disciplinary history.

Historians of anthropology often do not formulate their arguments in abstract terms, a detriment to those who question the relevance of their work. This paper will address itself to the maturity of contemporary disciplinary theory and the need for an historical approach to it as integral to praxis. Mature theory, I would argue, presupposes an undistorted historicism if it is to be adequate, even synchronically. That is, we must think historically while we are thinking theoretically.³

At one level, the contrast which George Stocking (1968) imposed on the history of anthropology more than two decades ago between "presentism" and "historicism" has to do with the willingness of the historian of science, whatever his/her disciplinary affiliation, to deal — in their own terms — with intellectual debates that were significant in the past but whose resolutions may be less than congenial to latter-day practitioners.

The ambiguity with which historians of anthropology have treated racism in cultural as well as biological anthropology, or the lack of feminist consciousness in cultural descriptions, illustrate the

queasiness with which we sometimes wish not to claim ancestors distinguished in their own time. It is sobering, though historically undeniable, to recall that Boas systematically and unrepentantly looted Northwest Coast graves to enhance his anthropometric collections, or that Malinowski's diaries make it clear that he lusted after dusky Melanesian ladies.

There can be no denial that historicism in the sense of adequate explication of prior ideas, the more necessary as the more alien to present practice, is the *sine qua non* of adequate disciplinary history. The presentism of an argument like that of Marvin Harris (1968) that all anthropological thought must be interpreted in relation to the emergence of a nomothetic techno-environmental determinism presumably raises qualms which transcend theoretical persuasion. But more subtle examples are also rampant. Elvin Hatch's treatment of ten great men (1973) (one of whom is a woman) declines to treat these anthropologists in chronological order and compares their ideas without regard to chronology and mutual inter-influence. The resulting distortion obscures the very real interaction of ideas among contemporaries and the relation of each to the then-past of his/her discipline.

To move closer to the present, there seems no compelling reason for Clifford Geertz, in *Works and Lives* (1988), to treat Claude Lévi-Strauss, E.R. Evans-Pritchard, Bronislaw Malinowski and Ruth Benedict in that order. Insofar as there is such a rationale, it is presumably rhetorical rather than historicist. Indeed, Geertz utterly fails to address the question of the chronological emergence of the four narrative stances represented by these canonical ethnographers. There is a dislocation of historical context which jars. It wouldn't take much to make a historicist disciplinary historian a lot happier. To distinguish between history and theory is not, however, enough. Rather, the theoretical point of Geertz's rereading would be more powerful if it were historically as well as theoretically sophisticated. Indeed, theory, rhetoric and historical accuracy are all too often ambiguously intermingled in contemporary anthropological writing.

At another level, the issue of reading the past in its own terms has been confused with a different kind of presentism which involves the tracing back to their original context of ideas which are of considerable concern to practitioners today. I would argue that this can and should be done in a historicist manner and that it is a valid part of the history of our

discipline. In practice, such concerns are usually part of the framing whereby the serious disciplinary historian attempts to engage an audience within the discipline. George Stocking, the ultimate historicist, repeatedly makes such arguments, e.g., in his recent book on Victorian anthropology (1987).

In spite of its ostensible concern with historical reconstruction, the Americanist tradition (by which I mean that dealing with native North Americans) all too often takes for granted a timelessness within which the ethnographic present comes to be. Yet, there is a simultaneous commitment in principle to historicism and diachrony, which applies to the way North American anthropologists approach disciplinary history as well as to the way they construct ethnography.

In contrast, most of our British-trained colleagues do not assume the need to know history in order to understand (i.e., to theorize about) the present.⁴ Many are more comfortable with approaching their professional history in terms of the distant intellectual ancestry of the Scottish Enlightenment or the Classical Evolutionists than with the more direct and immediate roots of British social anthropology in the work of Bronislaw Malinowski and A.R. Radcliffe-Brown and their intellectual progeny. This synchronic myopia is long-standing. A theoretical bias away from history in the writing of ethnography has been carried over to the writing of disciplinary history.

For example, at a Wenner-Gren sponsored conference on national traditions in anthropology which I attended in 1968, Meyer Fortes, self-consciously representing the British tradition, boasted that he had forbidden historian John Burrows to sit in on his anthropology courses because the latter's history of Victorian evolutionary thought would suffer if he tried to decide whether Malinowski or Frazer were right or not. To my mind, Burrow's excellent book (1966) would have been even more compelling had it been more deeply engaged with anthropological issues as understood by anthropologists, had it explored the continuity from evolutionary theory to British social anthropology.

Interestingly, the synchronic tradition of British social anthropology and the more diachronic one of Boasian cultural anthropology came into contact on North American soil during the formative years of professional academic anthropology. Edward Sapir, perhaps Boas's most brilliant student, served as the

central theoretician of anthropology at the University of Chicago between his departure from Ottawa in 1925 and his appointment at Yale University in 1931 (see Darnell 1990a for further details). When Sapir moved to Yale, he was succeeded at Chicago by A.R. Radcliffe-Brown. Sapir left behind his unofficial student and collaborator on Navajo, Father Berard Haile, but did not renege on his commitment to help Haile see his extensive Navajo texts through to publication. The resulting polarization of priorities — focused around the fate of Father Berard's texts — stands as a microcosm of the differences between the British and North American traditions.

Radcliffe-Brown professed his mystification that texts should be published, and by implication valued, at all. In a memorandum, presumably written to Fay-Cooper Cole as chairman of the Department of Anthropology (n.d. May 1932: University of Chicago Archives, Department of Anthropology files), Radcliffe-Brown threw out a challenge to the Americanist tradition as a whole:

What are such texts as these for? I wish Sapir had enlightened me on this. I read his letter over without finding out just what one does with such texts.... Authoritative it will certainly be. But just what will be done by scholars with these texts? ...I am clear on this: that if they are to be treasured merely because they are disappearing, and because they are accurately transcribed... their publication would then be supported by mere antiquarian sentiment.

Sapir, unsurprisingly, was appalled. Radcliffe-Brown's position posed a challenge to the most cherished tenets of the Boasian programme, both theoretically and methodologically. Texts, to the Boasians, were virtually sacred artifacts. Sapir insisted to Cole (22 May 1932: University of Chicago Archives) that the Navajo texts were "a priceless linguistic document" and that he would not permit "a beautiful piece of work [to be] made hash of because of the hostility of a supercilious gentleman." In Sapir's opinion, Radcliffe-Brown lacked the Americanist experience to challenge established North American methods of working on the basis of linguistic and ethnographic texts.

Cole attempted to mediate the rather acrimonious dispute between the *prima donnas* of the two national traditions. He successfully persuaded Sapir that he had over-reacted to Radcliffe-Brown's memo and proposed a compromise in which Haile would provide supplementary explanatory commentary

about the meaning of the ritual to the Navajo themselves.

Sapir had no objection to ethnological notes, as long as the texts themselves were published in their integrity. He explained to Cole (2 June 1932: University of Chicago Archives) that "Brown"⁵ had given the impression that a general treatment of Navajo ritual was more important than the texts themselves (which is undoubtedly precisely what Radcliffe-Brown meant). Sapir, reassured that Cole was on his side, continued:

You see, from my standpoint that meant not only was the priceless linguistic material as such to be disregarded... but that all my own Navaho⁶ fieldwork was, by implication, judged a waste of time, that I might, so far as he was concerned, never have trained Father Berard..., that nobody cared for elaborate accounts of specific Navaho rituals anyway, and that we in America had better get busy and learn something from functionalism as to how a truly readable volume should be prepared.... It was as if some Smart Aleck were to put the proffered texts of the Homeric poems aside with a supercilious remark.

Over the years that the fate of Haile's texts remained unresolved, Sapir's most articulate statement of his passionate commitment to the text method came in a letter to Cole in 1938 (25 April: University of Chicago Archives):

I'm not particularly interested in "smoothed-over" versions of native culture. I like the stuff in the raw, as felt and dictated by the natives.... The genuine, difficult, confusing, primary sources. These must be presented, whatever else is done... There are too many glib monographs, most of which time will show to be highly subjective performances. We need to develop in cultural anthropology that anxious respect for documentary evidence that is so familiar to the historian, the classical scholar, the Orientalist. We'll **have** to do this, willy nilly, if we are to keep the respect of our colleagues.... If we're not careful, thoughtful and essentially not unfriendly colleagues will be getting more and more restive and saying, "Yes, this is all most interesting and I admire the beautiful synthesis that you have made, but where is the raw evidence? I can't tell whether a given statement is common native knowledge or is merely your interpretation of one man's say-so."

For Sapir, more was at stake than the publication of a collaborator's work. His lyrical defense of the method of using texts as the basis for linguistic and ethnographic description crystallized the essence of the Boasian Americanist tradition — as understood by Sapir, one of its most distinguished practitioners.

My own work gives a particular construction to the history of anthropology (though I have never claimed that other things are not history of anthropology). It focuses, I would claim in a uniquely North American way, on the dialectic between the articulation of practitioners — anthropologists as ethnographers and as theoreticians — with their intellectual ancestry and the work that they do. And here, finally, I get to my thesis that the uniqueness of North American anthropology is to be found in this very reliance on the text method of ethnographic presentation.⁷ Boas taught his students to argue that the data-base for both ethnography and linguistics ideally was to be texts in the words of native speakers of aboriginal languages, in those languages, presented with a translation which, whatever its literary merits (usually minimal), preserved as much as possible of their original structure.

In linguistics, this followed from and elaborated upon the emphasis on suppression of preconceived Indo-European grammatical categories. Direct elicitation was tied necessarily to observer bias, whereas spontaneous speech by a native speaker naive in the analysis of grammar would allow the "emics," in our more modern jargon, to emerge.

In ethnography, the textual premise had even more dramatic ramifications. The collection of texts required the anthropologist to work intensively with a small number of "informants." I maintain the term "informant," now relegated to the early imperialist phases of our discipline, along with "savage" and "primitive," in large part for the simple historicist reason that it was the term used by the anthropologists who collected our classic canon of Americanist texts. This increasingly archaic term "informant," however, is further useful in highlighting the change which has taken place in the last couple of decades, as our "informants" have become our "collaborators" or "consultants." This process acknowledges the integrity of the voices we record, allows them not only to speak for themselves, but also to take credit for their own words, transmitting an oral tradition in a new medium of literature. The wave of recent work on American Indian biography, spearheaded by Arnold Krupat (1989) and David Brumble (1988), is

illustrative of this latter-day acknowledgement. Dennis Tedlock's work on texts as dialectic between native speaker and anthropologist (1983) lyrically characterizes the new concerns from within our discipline. To give a concrete example, Sapir's Kutchin texts recorded from John Fredson in 1923 at Red Cloud Camp in Pennsylvania were never published by Sapir (although they will appear in his 16-volume collected works, now in press from Mouton de Gruyter). But a version reworked by contemporary Kutchin speakers has appeared under Fredson's own name from the Alaska Native Languages Center (1982).

The linguist/anthropologist who worked day after day with an old man, occasionally woman, who remembered traditional stories and/or a younger bilingual translator was forced to attend to the construction of a culture in the understanding of a single individual. Moreover, attention to variability of "the culture" as understood by different individuals was entirely consistent with the lack, among most native North Americans, of a cultural canon which could, or at least would, label any particular integration of cultural knowledge as invalid (although, of course, some versions were more highly respected within the culture than others).

I have argued elsewhere (1990b) that the text tradition, in this sense, provides the link for Edward Sapir between his work in linguistics and that in culture-and-personality. Because the words of the native speaker came to stand for the culture itself, it mattered which individual articulated the culture and what he/she brought to that articulation. (It was not always the case, of course, that the Boasian ethnographer explored the implications of such variability.) In a marvellous paper, Jeanne Cannizzo (1983) argues that George Hunt's personal stake in articulating his own experience as a partial outsider to Kwakiutl culture was essential to the character of that culture as it came to be constituted in ethnographic canon by way of Franz Boas. One might compare, for example, Dell Hymes's explication of the stories of Victoria Howard as reflecting a genealogical line of female storytellers and cultural transmission among the Chinook (1981).

This emphasis on texts is not found in the British tradition or the French, or, ironically given the origins of the text tradition, the German. At the American Anthropological Association meetings in 1989, Adam Kuper, historian of anthropology and editor of *Current Anthropology* (where the paper appeared as

an editorial in 1990; see also 1991) said that what he found most alien in North American anthropology was its focus on language. I would like to think that he meant not just the required course in descriptive linguistics within our four-square definition of the discipline but rather the conditioned attention to the words of native speakers which arises from the North American text tradition. In this tradition, culture is understood to be a symbolic form. The consequence of a symbolic definition of culture is that it becomes a particular, individual, unique integration of a *Weltanschauung*. Anthropological understanding of a culture comes from its articulation by people who live it. Such understanding is impossible if the ethnographer speaks for the people he/she studies.

The concern with texts was brought into our North American disciplinary mainstream concurrently with professionalization of its institutions, personnel and research methods by Franz Boas, by way of a German folkloristic and literary emphasis on the continuity of intellectual history reconstructed through texts. This is best exemplified by the linguistic and ethnographic scope of the work of the Brothers Grimm — one a linguist, looking for the expression of romanticized national identity in the vernacular of the peasant, the other collecting and preserving in literary form the stories which were the core of the European oral tradition.

Boas went to the Central Eskimo with a hypothesis about environment and its determinism over cultural forms. Having been forced by his ethnographic experience to reject this hypothesis, Boas turned to texts as a way of getting at what "they" were doing when they weren't being determined by their environment. He deliberately chose the Northwest Coast for his next, lifetime, fieldwork because of its historical complexity. Boas's adaptation of the established, though not in anthropology, text method added a third strand of relevance — the usefulness of story themes and elements for reconstructing the history of culture contact and migration among "peoples without history."

For Boas, the problems of anthropology were twofold: the historical and the psychological. In historical terms, his questions had to do with what you use when you don't have the written documents which "our" tradition privileges as history. He thought that understanding history in this sense was methodologically prior to addressing psychological questions. In the anthropological climate of the late

nineteenth century, the theoretical agenda involved clearly distinguishing the three variables which defined the scope of the discipline — race, language and culture. Boas argued that you got different classifications of the groups of mankind depending on which variable(s) you used.

Mostly by way of Margaret Mead's rampantly presentist readings of Boas's thinking in the interwar period (e.g., 1959, ed.), the history of anthropology has inherited the notion that Boas refused to address psychological questions at all until he had solved the historical problems. In fact, however, in 1911, the year he made the race, language and culture argument in the *Introduction to the Handbook of American Indian Languages*, Boas also published *The Mind of Primitive Man* — in which he saw no contradiction in claiming that the ultimate problems of anthropology were psychological. Boas, however, didn't use the word "psychological" in its modern sense. He was not particularly interested in the Freudian deep structure of the psyche (though he did follow Freud's work closely, earlier than most North American intellectuals). The psychology of Jung appalled Boas by the superficiality of his ethnographic evidence and the rapidity by which he leaped from his private dream imagery to postulation of cultural universals.

For Boas, psychology was inseparable from history. Texts and their contents could be used to reconstruct history because people were not consciously aware of cultural forms. Therefore, such forms persisted as unexamined cultural knowledge. The kinds of explanations people were able to give about cultural phenomena were "secondary" and could be filtered out by the analyst. For the anthropologist whose goal was to convey the reality of an alternative *Weltanschauung*, however, the only legitimate source of evidence was the articulation of the culture in the words of particular individuals.

All of this is an admittedly leisurely prologue to a reconsideration of post-structuralist (or post-modernist) anthropology. Insofar as it deals with Boasian anthropology at all, this contemporary critique of the anthropological agenda of cultural relativism has been dismissive and stereotypical. Reading the Bible of the movement, Clifford and Marcus's *Writing Culture* (1986), the reader might well conclude that no Boasian ever wrote an ethnography. The critique centers on Ruth Benedict's *Patterns of Culture* (1934), which is a different genre altogether. *Patterns of Culture* is not an ethnography.

I will draw my post-structuralist strawman, however, from a work I consider more sophisticated than most of the papers in Clifford and Marcus, in both theoretical and literary terms: Clifford Geertz's *Works and Lives: The Anthropologist as Author* (1988). Geertz proposes four narrative *personae*, which have been widely adopted by subsequent anthropologists.

In *Tristes Tropiques*, Lévi-Strauss judges the exotic made intelligible to be no longer interesting and thereafter grounds his investigation of the products of the human mind in the mind of one particular French intellectual, himself.

E.E. Evans-Pritchard, writing about the Nuer, persuades the reader of the authenticity of his experience on the grounds of descriptive, usually visually so, exhaustiveness. Whether or not he succeeds in being objective, he is often dull (which is not, of course, to say that the Nuer themselves are dull). Moreover, his questions come from his own society, reflecting a kind of patronizing bemusement at "...how what we take to be the foundations of genuinely human life manage to exist without the assistance of our institutions" (Geertz 1988: 69). If Geertz's point is that Evans-Pritchard represents for anthropologists this style of ethnographic writing, his choice of an obscure wartime report to exemplify the genre seems somewhat perverse.

Malinowski plays off of his own sense of irony about his personal experience. Geertz argues persuasively that Malinowski's participant observation is more reasonably seen as "a literary dilemma, Participant Description" than as a factual one (1988: 83). Interestingly, Geertz relies heavily on Malinowski's diaries to establish his introspection, "reflexivity" in current jargon. Yet Malinowski's ethnographic reputation vastly precedes the publication of his Trobriand diaries in 1967. Sections of *Coral Gardens and their Magic* (1935) make many of the same points in a way more easily arguable to have influenced authorial stances within the discipline.

Geertz arrives at the Boasian tradition, not through the words of the master himself, but in those of the disciple who could write — Ruth Benedict. This is, of course, more convenient for the theoretician who wants to educate anthropologists to write (and to read) ethnographic texts that are multi-levelled in their meaning and sophisticated in their style. Like the contributors to Clifford and Marcus, however, Geertz carefully sidesteps the five-foot

shelf of Kwakiutl ethnology published by Boas and ambivalently co-authored by George Hunt. From Geertz's treatment, the reader might draw the moral that Boas should have written things that were readable, should have digested this mass of texts, interpreting them for the anthropologists who would follow him.

But Boas's point was precisely that the texts themselves constituted the ethnography. What Franz Boas might say about the Kwakiutl was of considerably less interest than what they actually did say about themselves. Boas's approach leaves out the casual reader (for whom he was not writing anyway), but it is hardly a-theoretical, as the history of our discipline has most often claimed.

Without denying the difficult intelligibility of the seemingly endless volumes of opaque texts, however, I want to focus on Boas as ethnographer and theoretician. Boas has been dismissed as a theoretical leader in North American anthropology quite consistently, though he is regularly credited with effective institutional leadership, giving the discipline its modern academic character. There is, in fact, very little difference in historicism between Marvin Harris in 1968 or Leslie White in 1966 arguing that Boas set back the discipline by his failure to engage theoretical issues and Clifford and Marcus or Geertz dismissing the ethnographic rhetorical/literary stance deliberately taken by Boas and his students around the text method. In all cases, the critique of the putative historian of anthropology is motivated by the wish to set a new theoretical agenda. In both readings, texts are "mere" description. Post-modernist readings of Boasian anthropology, in particular, have failed to notice the continuity of their own concerns with text and voice to the Boasian tradition they alternatively criticize or ignore.

I am currently engaged in a re-reading of how Boas used texts and how the text method came to be so integral to the North American anthropological voice that we no longer consider it interesting enough to be reportable or theoretical. Yet the words of native speakers/members-of-culture are far from the only possible stance in relation to the cultural "other." There are some implications which we need to draw to mind again, on both theoretical and historicist grounds.

What are texts good for? Boas believed he was recording disappearing cultures so there would be a record of a great part of human intellectual

achievement otherwise to be lost. There is no question that much Native American traditional knowledge has been lost with the death of the last fluent speaker of various languages. We in Canada, however, are heirs to a great percentage of the viable aboriginal linguistic traditions of North America. There are large numbers of speakers of Cree, Ojibwe and Inuktitut. The Dene languages of the MacKenzie Delta, taken together, are alive and well — their persistence encouraged by recent official language legislation in the Northwest Territories. Some of the Iroquoian languages are doing fine, as are the larger languages of the Northwest Coast. Many native communities are active in language maintenance programs, seeing this as essential to maintenance of their aboriginal identity.

Texts collected by anthropologists are being read and adapted and reframed in terms of current cultural dilemmas by native people themselves. The words of the elders are the core of traditional cultural transmission among all native groups. The written texts are a resource, recording the conservative traditional speech of an era no longer retrievable directly. Oral transmission continues, but it is enriched in many areas by the texts from our anthropological libraries and archives.

The community-based work many of us now do in oral history, gathering the words of the elders for further generations of native children, has powerful roots in the text tradition of Boas, Sapir and their contemporaries. Robin Ridington in his *Trail to Heaven* (1989), lyrically muses on the fate of Dunneza children who will never know a dreamer. It is, therefore, crucially important that Ridington record his knowing of dreamers. By implications, others of us — native and white — also have an obligation to record our experiences of aboriginal oral traditions.

The texts are there. They document the inexplicable logic underlying objects of material culture. Boas's letter-diaries from the Northwest Coast (Rohner 1969) repeatedly report the value of collecting a mask for the American Museum of Natural History or the Smithsonian Institution and getting the story that goes with it. The culture is not in the things but in the ideas underlying their construction — ideas which are expressed in the words of the elders and recorded in the texts.⁸

Native people invented literary text theory before Paul Ricoeur. The texts, the words of the elders — whether orally or literarily transmitted, are open-

ended and available for reflection in terms of life experience, which is by definition different for each individual who learns from them.

We need to rethink our professional debt to this kind of work. It is basic to our conviction that cultural ideas can be and are in the course of everyday life, especially because of the need to socialize children, put into words. Moreover, those words can be translated across cultural boundaries. Perhaps the greatest challenge to anthropology in the nineteen-nineties is the dissolution of the boundary traditionally taken for granted between "us" and "them." We blur the boundary in another way when we as anthropologists study our own culture, as members of it, allowing our traditional double vision to operate within a single culture. Our stance here may well harken back to the folkloristic European tradition of *Volk ethnography* and its continuity to modern *Lebenswelten*. And finally, we dissolve the increasingly permeable boundary when not all anthropologists are alien to the cultures they study. One thinks of Alfonso Ortiz's work on Pueblo religion or of native writers like M. Scott Momaday or Leslie Silko.

In an all-day retrospective session twenty years after the publication of Vine Deloria Jr.'s *Custer Died for your Sins* (1969) at the 1989 meetings of the American Anthropological Association, Lakota ethnohistorian Raymond DeMallie analyzed, in a very "Indian" fashion (beginning with the by now partly apocryphal story of his first meeting with her), the letters of Ella Deloria, a revered elder of Vine's own family but also an anthropologist trained by Franz Boas. In her letters to Boas, Ella Deloria agonized over her efforts to describe the Assiniboine. She was unable to resolve in her own mind whether the neighboring culture was simply "thinner" (à la Geertz) than her own Lakota culture or whether the richness of the Lakota world was obvious to her because she was part of it. There could be no clearer statement of how the anthropologist tries to separate the "emic" from the "etic." Nor had Vine Deloria any comment on that paper.

The great agonizing issues of our generation in anthropology appear to me to be two: First, where do we get the *chutzpah* to speak for others? Secondly, can we resolve the subjectivity which is at some level inherent in all ethnographic work/writing? I submit that a serious rereading of the texts we have and hope to have in the future leads us to a way out of both post-structuralist failure of nerve and the crisis

of conscience in the professional ethics of anthropology as a discipline.

Focus on texts is the core of the historically-constituted heritage of our Americanist tradition. It is simultaneously a resource for our future, both disciplinarily and in terms of the self-definition of the communities with which we interact. Our notion of fieldwork has changed dramatically in recent years. Many of our "best" ethnographers have been hanging around and hanging out in the same cultural contexts for twenty or more years. We have friends with whom we have lived out together much of our mutual life cycles. As to the "us" and the "them," there are a multiplicity of voices within each — our job is to listen to our texts, both oral and written, in an evolving, changing cycle of interpretation. Many voices add up to more than the sum of their parts, to more than subjectivity. This has always been the foundational premise of anthropology as a cross-cultural discipline.

We would not be asking these questions, at least not in this way, without the Americanist text tradition. We cannot retrieve its origins and implications, however, solely on the basis of present praxis, post-structural or otherwise. There is a historicist context which potentially explains to us why some anthropological texts form a kind of canon, why there is an overwhelming citation and reanalysis of Boas's Kwakiutl texts, ranging from Helen Codere's challenge of Ruth Benedict's megalomaniac Kwakiutl (1956) to Lévi-Strauss' rereading of Boas's mask texts in both historical and structural (Boas would have called it "psychological") terms (1982). The texts are what we work with. And they exist, though interpretations of them are variable, open-ended and ongoing.

NOTES

1. A preliminary version of this paper was delivered as a plenary address at the 1990 meetings of the Canadian Anthropology Association. I would like to thank Elvi Whitaker and Pierre Maranda for encouragement to proceed with this reconsideration of the theoretical groundings of Canadian anthropology. I have also benefited from discussions with Judith Berman, Ira Jacknis, Stephen O. Murray and James M. Nyce. I have cited documents from the University of Chicago Archives, Department of Anthropology files.
2. Then, as now, the Americanist tradition as explored here operates in Anglophone Canada. The studies of native Canadians arising from Québec anthropology have a distinct history which is not dealt with here.

3. Historical anthropology, dealing with time perspective in ethnographic theory, must be clearly distinguished from the history of anthropology itself. The latter is of concern here.
4. I am fully aware that there are exceptions to and complexities about the characterization of British social anthropology presented here. In part, my point is rhetorical and calls for the greatest possible contrast between the British and Americanist traditions. Moreover, I have tried to be provocative because I think there are issues here that members of the discipline need to talk about.
5. Various Boasians in this period expressed their lack of affection for Radcliffe-Brown and his attitude toward North American anthropology by refusing to acknowledge the hyphenated form of his name.
6. Sapir used the spelling Navaho, although Navajo is conventional today.
7. This is certainly not the only characteristic of the North American tradition. It is, however, one which explains a lot of the sense that practitioners have of being on different ground when they move between North American and British work.
8. Lévi-Strauss (1982), though in many ways far from a Boasian, takes the argument to its logical conclusion when he moves freely between myth text and plastic form in art to make explicit the system of meaning encoded in the products of Northwest Coast cultures and borrowed widely among them.

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