



# Why Do They Do It?—A Brief Inquiry into the Real Motives of Some of the Participants in the Recording, Transcribing, Translating, Editing, and Publishing of Aboriginal Oral Narrative

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## Article abstract

This article inquires into the motives of the participants in the recording, transcribing, translating, editing and publishing of Aboriginal narrative. The motivation of Aboriginal communicators, at the outset simple altruism, has evolved onto a pressing need to bear witness to past and present wrongs perpetrated against them by various agents of the dominant society. Social scientists' motivations are equally complex. Most of the social sciences, and particularly anthropology, practice translation. Anthropology has elaborated translation theories that betray a general unease with how and why anthropologists translate. Anthropological translation differs from that of other disciplines in that when anthropologists translate oral and written "texts," their ultimate aim is in fact the "translation" of the cultures that produced them.

**Keywords:** anthropology, translation, Aboriginal, oral narrative, cultures.

# Why Do They Do It?—A Brief Inquiry into the Real Motives of Some of the Participants in the Recording, Transcribing, Translating, Editing, and Publishing of Aboriginal Oral Narrative

**Philippe Cardinal**

It is not entirely clear why Aboriginal communicators, generally elders, chose to tell the traditional stories of their nation to non-Aboriginals (generally social scientists) who came calling on them in the past. It is moreover by no means clearer what precisely their motives and their expectations were in telling those particular stories to those particular individuals at that particular time. Nevertheless, we can safely speculate that until fairly recently their motives were probably altruistic. A tactic commonly used by anthropologists and other social scientists seeking the collaboration of Aboriginals when “in the field” doing “participant observation,”<sup>1</sup> consists in pretending to be almost as ignorant of the subtleties of their host culture as a small child or even an infant of that culture would be; this, in the hope that knowledgeable adults in the society might be moved to instruct them in what they would need to know in order to become

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<sup>1</sup> **Participant Observation:** Direct field observation that can lead to a fuller understanding of the sociocultural realities than could be gained by reliance on secondary sources. (from Glossary of Anthropological Terms: [http://www.wadsworth.com/anthropology\\_d/special\\_features/anthro\\_glossary.html](http://www.wadsworth.com/anthropology_d/special_features/anthro_glossary.html) - consulted May 12, 2006)

fully-functional adult members of that society. The reasoning behind this tactic is that once so instructed they would be able to speak and write knowledgeably about that society. In most Aboriginal societies the task of instructing the young traditionally fell to the elders, no doubt because, of all the adults, they had the most free time to devote to that particular task, and also because, as the eldest, they were usually also the most knowledgeable about all aspects of their culture. This is why in most past cases we probably need look no further into those elders' motivations in telling their traditional stories to visiting strangers. In oral cultures, after all, the telling of traditional stories is generally the means by which children are educated. What is not clear, however, is how aware those elders were of the ultimate goal of their foreign "pupils," which was generally not so much a matter of becoming proficient in the culture per se, as to "mine"<sup>2</sup> the elders for knowledge about their culture; knowledge which could then serve as raw data, or ore, that could eventually be processed or refined into learned academic articles and books, as well as serve as the raw material to be transformed into courses taught to university students.

If the motives of the elders were most probably largely unselfish in nature, those of the social scientists whom they instructed, however, are not so easily defined. Mary Louise Pratt suggests that the motives of social scientists in recording, textualizing, translating, editing and publishing Aboriginal narratives, though sometimes no doubt partly altruistic, may often be considerably more besides. As an example Pratt cites Marjorie Shostak's famous book *Nisa* (1981), which Shostak wrote following her participant observation field work among the people whom she and her Harvard Kalahari Project colleagues call the !Kung (the better to set themselves apart from the 300 years worth of travel writers and "amateur" ethnographers who have been writing about and studying the same people, otherwise, and better known, as the Bushmen). She notes that Shostak naively states that she wanted to study the !Kung because she hoped that the !Kung women would be able to,

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<sup>2</sup> For this metaphor of Academics « mining » Aboriginal Elders for nuggets of traditional knowledge that they can then "refine" into theses, articles and books, I am indebted to linguist André Bourcier of Whitehorse's Yukon Native Language Centre (personal communication, May 4, 2006).

clarify some of the issues raised by the American women's movement [because] their culture, unlike ours, was not being continuously disrupted by social and political factions telling them first that women were one way, then another. Although the !Kung were experiencing cultural change, it was still quite recent and subtle and had thus far left their traditional value system mostly intact. A study revealing what !Kung women's lives were like today might reflect what their lives had been like for generations, possibly even for thousands of years. (Shostak 1981, p. 6—quoted in Pratt, 1986, p. 48)

Shostak's words betray a belief in the notion of "primitive," "timeless," "missing link" peoples, or "people without history" (Wolf, 1982). It is a belief that persists (though probably unconsciously) among today's social scientists who study far-off cultures hoping to shed light on the ways of our own remote ancestors. Their ultimate goal is to determine what we would naturally be like if our culture, like that of the American women whose lives Shostak hopes to enlighten through her work, "was not being continuously disrupted by social and political factions." They hope to accomplish this by studying the ways and culture of peoples who have "failed to evolve." They almost always characterise their work as extremely urgent, arguing that nearly pristine traditional cultures such as that of the !Kung cannot long survive in today's bewildering world and must therefore be studied quickly before they disappear forever. As Mary Louise Pratt points out, however,

'Recent' and 'subtle' are not the adjectives that come to mind when one ponders the grim history of the Bushman conquest. [...] Is it not worth even asking the question whether 300 years of warfare and persecution at the hands of white settlers (to say nothing of the competition with indigenous pastoralists) have had an impact on the life-ways, the consciousness, the social organization, even the physiology of the group undergoing these traumas? Did the long-term practice of massacring men and enslaving women have no impact on 'what women's lives were like' or how women saw themselves? What picture of the !Kung would one draw if instead of defining them as survivors of the stone age and a delicate and complex adaptation to the Kalahari desert, one looked at them as survivors of capitalist expansion, and a delicate and complex adaptation to three centuries of violence and intimidation? (1986, pp. 48-49)

Mary Louise Pratt further argues that “Modern ethnography obviously lies in direct continuity with this tradition [of literary travel writing], despite the disciplinary boundary by which it separates itself off from travel writing” (1986, p. 35). Anthropology, contends Pratt, strives rather desperately to give a “scientific” tone to its writing, which, to many people, makes it just plain boring, and that is why many anthropologists add a sometimes lengthy foreword or introduction to their ethnographies in which they indulge in more literary narration explaining how they came to be with this other culture, the early difficulties they encountered getting accepted within the culture, and something of their adventures. That is usually the part of their book that is most memorable. After this initial exercise, however, they must settle down to “serious (and dull) science,” or risk the chastisement of their fellow ethnographers. Alternatively, ethnographers write two books: the serious, scientific, ethnography, and, later, a second and more literary book narrating their field adventures. According to Pratt, anthropology as a discipline somewhat frowns upon the second book, but, as far as she could discover, no one has ever been formally disciplined for it (1986, pp. 27-35).

James Clifford argues that Shostak shapes Nisa’s life experience in such a way that, “this shaped experience soon becomes a story of ‘women’s’ existence, a story that rhymes closely with many of the experiences and issues highlighted in recent feminine thought” (1986, p. 104). Clifford argues that books like *Nisa* are in fact allegories “of scientific comprehension, operating at the levels both of cultural description and of a search for human origins...” and that “*Nisa* is a Western feminist allegory, part of the reinvention of the general category ‘woman’ in the 1970s and 80s. *Nisa* is an allegory of ethnography, of contact and comprehension” (*Ibid.*). But then, we might well ask, whose culture is actually being described in these allegories? Is it the (foreign, exotic) culture under study or is it that of the social scientist conducting the study? We might also ask in whose interest these narratives are being recorded, textualized, translated and eventually prepared for publication? Reflecting upon Shostak’s own stated reasons for recording, translating, editing and publishing Nisa’s life story, one may readily concede that her motives may well have been altruistic, but if so, her altruism appears to have been directed squarely toward her own society, not Nisa’s.

Clearly, the way individuals conduct social science, the precise lessons they draw from contact with another people, and the people who stand to benefit from such lessons very much depend upon the particular axe each social scientist chooses to grind.

When Aboriginal Elders “educated” social scientists in the subtleties of their culture, they were of course speaking in their own native language, and thus it was necessary for the scientists to either enlist the help of a bilingual interpreter or to actually learn the language. When Nisa, the !Kung woman, told her life story to Marjorie Shostak, she spoke in her own native language and Shostak had to translate her words as a necessary step toward publishing them. In doing so, she was following in the footsteps of generations of other American ethnographers who, ever since the days of Franz Boas, have routinely recorded, transcribed, translated, edited and published the words of Aboriginal spokespersons. To Boas, whom some call the “father of American anthropology” (Anonymous, Wikipedia, 2006), and his followers, the potential difficulty of not knowing the language of their informants, far from being considered a drawback, was rather seen as a golden opportunity since it allowed them both to study the society’s language and its culture at one and the same time. According to William Clements, the generation of serious students of North American Aboriginal cultures who laid down the principles of the new (new in the late 19<sup>th</sup> century, at any rate) “science” of anthropology were primarily interested in recording, translating and textualizing Aboriginal narratives for the data that this could yield. “Their approach to the process of textualization, which became formalized under the auspices of the Smithsonian Institution’s Bureau of American Anthropology and in Boasian academic anthropology, yielded a healthy commitment to accuracy in recording and transcribing the verbal component of the material they encountered” (1996, p. 130). Although there is no doubt that Boasian anthropologists have added and continue to add much to our knowledge of non-Western cultures and languages, they have nevertheless come under considerable criticism of late, among other things for neglecting to account for or to convey any real sense of the “artfulness” of Aboriginal “oral performance” (Berman, 1992; Clements, 1996).

Fellow social scientists, however, are far from being the only critics of Boasian-style anthropology. For some years now, many Aboriginals have also been highly critical of the discipline, and well-known American Indian activist, lawyer and academic, Vine Deloria Jr., has even accused anthropologists of being little more than “ideological vultures” (1969, p. 95). Stung by such charges, some anthropologists have reacted publicly. Writing in 1997, Elizabeth S. Grobsmith actually expresses a certain amount of gratitude towards Deloria because his writings have both helped her to acquire the “thick skin” that allows her to weather the occasional “unjust” criticism, and moved her to resolve from the very start of her career always to endeavour to determine what the most ethical possible way of proceeding is before posing any act in a professional capacity, and, should ethical proceeding ever prove impossible, never to proceed anyway. Grobsmith considers herself indebted to Deloria and argues that anthropology is, too, but she also clearly sees a need for tempering his critique and for developing a more nuanced assessment of anthropology’s relation to Aboriginal communities (1997, pp. 35-49). Writing in the same publication, also in reaction to the writings of Vine Deloria Jr., Murray L. Wax explores both how anthropology has come to claim the authority to represent Aboriginal peoples and why that authority has become problematic. He also emphasizes Deloria’s insistence that only a researcher whose interests are tied to those of the community being studied will be able to produce work that is useful, desired by the community, and not colonial in nature (1997, pp. 50-60).

One recent case in which two Canadian First Nations actually enlisted the help of several social scientists in a joint effort to bring about social and political change occurred in the *Delgamuukw* case. *Delgamuukw* refers to a 1991 judgement handed down in the British Columbia Supreme Court by Chief Justice Allan McEachern. The stakes of the trial were high for Aboriginals, involving as they did a bid by two north-western British Columbia First Nations—the Gitksan and the Wet’suwet’en—to not only force the Canadian federal and the British Columbia provincial governments to officially recognise their full legal ownership of over 22,000 square miles of BC’s north-western Interior that they were claiming as their unceded ancestral lands, but that their inherent right to self-government be recognised as well. For a detailed, almost “blow-by-blow,” description of the

trial, Culhane, 1998, is an excellent starting point. My present interest in *Delgamuukw*, however, stems from the fact that the evidence presented in court by the Gitksan and the Wet'suwet'en was almost entirely based on oral traditions that they performed in court in their own traditional languages, forcing the Court to rely on translators and interpreters to follow their arguments. Needless to add, *Delgamuukw* provides considerable insight into the nature of traditional Aboriginal oral narrative, and therefore adds much to our understanding of what those narratives mean to at least some Aboriginal societies. One of the major stakes involved for the Gitksan and the Wet'suwet'en was getting the British Columbia Supreme Court to recognise the validity of their own oral-based traditional legal system as a duly constituted legal entity, empowered to recognise ownership of land long before the institution of the British legal system in colonial times. They hired their own legal counsel, which they directed throughout the trial, as well as a battery of social scientists who served as expert witnesses on their behalf—corroborating evidence, as it were, for what was the main evidence of the traditional songs and narratives they performed before the Court.

The Gitksan's and Wet'suwet'en's traditional legal system is based on witnessing (Yagalahl, 1992; Cruikshank, 1992; Culhane, 1992, 1998). It is worth quoting part of anthropologist Julie Cruikshank's explanation of how their oral traditions and the land are connected, and of how they performed their traditions as a form of witnessing before Justice McEachern's court:

*Adaawk* are central to the social organization of Gitksan Houses just as *Kungax* are central to the Wet'suwet'en Houses. They are the oral histories that document House ownership of land and resources and their performances at feasts publicly validate those claims. In performing their *adaawk* and *Kungax*, the hereditary chiefs were offering narratives with roots in two distinct traditions of storytelling. The Gitksan describe their *Adaawk* as a collection of sacred reminiscences about ancestors, histories and territories. The Wet'suwet'en speak of the *Kungax* as 'a song of songs' about trails between territories, the songs tying them to the land and impressing on listeners the importance of place. These songs, dances, and performances, lacking arbitrary beginnings or endings, may flow into one another, like a trail or a stream. The statement of Claim made to the court asserts that the *expressions of ownership* of land come through the *Adaawk*, *Kungax*, songs and



ceremonial regalia; that the *confirmation of ownership* comes through the totem poles erected to give those expressions a material base; and that the *assertion of ownership* of specific territories is made to the court through specific claims. In other words, there exists a complex relationship linking history, the performance of *Adaawk* and *Kungax*, and the land. (Cruikshank, 1992, pp. 34-35)

By performing their *Adaawk* and *Kungax* before the court the Gitksan and Wet'suwet'en were attempting to reconcile their own traditional legal system with the Canadian legal system. In effect, they were asking the Court to validate their claim to the land and to their inherent right to self-government by having it witness their performance of the *Adaawk* and the *Kungax* by which their claims are proven in their respective cultures, in the same way that, say, a Gitksan or Wet'suwet'en heir to a title and to the land that goes with it would validate his or her claim before their assembled community at a feast. That Justice McEachern ultimately rejected the Gitksan and Wet'suwet'en arguments outright (refused to acknowledge their claim as made through the performance of their *Adaawk* and *Kungax*, in the Gitksan's and Wet'suwet'en's own legal terms), according to Yagalahl, a member of the Gitksan nation, was partly because their nations' histories, which they presented as evidence to his court, are not written down like those of Western nations, and because they were only transmitted orally: "And I guess this is one of the arguments that was (sic) used against us;" she said, "that there's oral history and nothing is written. I guess it's fine if anthropologists get this history and write it down and then it can be recognized" (1992, p. 9). Speaking in 1992, long before the Supreme Court of Canada overturned part of the McEachern decision, Yagalahl was still understandably bitter over this rejection of her people's legal system by the Canadian State as personified by the Chief Justice. Nevertheless, she felt that, far from weakening the Gitksan and Wet'suwet'en's resolve, it would actually strengthen their determination to continue bearing witness to their grievances so as to have them in due course acknowledged, and ultimately righted by the official representatives of mainstream society. But there was also a lot of pain that needed to be expressed:

When the oldest person was being cross-examined on his evidence he was 104 years old at the time. (...) And there he sat in the

witness stand, tiny, but sat very proud and answered with a very strong voice. And it was at this moment that I realized why we were there in court—when I saw him and the way he was answering. He was very strong, yet they were treating him like he was a criminal, sitting in the witness box. The only crime that we committed was being born Aboriginal people, descendants of Aboriginal people in this country. And it hurt. (1992, p. 10)

And yet, in spite of the court's rejection of her people's claims and in spite of the pain, Yagalahl still felt that some measurable progress had been made:

To me, it was a sad day when I heard that decision. And yet, in a way, I was happy because in a way it was a victory. A victory in a way that yes, our oral history was slammed around as we were witnesses on the witness stand, but we have it written in black and white now for anyone to see in those transcripts, in those 374 volumes of transcripts. (*Ibid.*, pp. 10-11)

Here Yagalahl voices a dilemma that many Aboriginal communicators face. On the one hand, they have never before felt the need to have their oral traditions written down, and so they often have little interest in collaborating with social scientists to record, translate, textualize and ultimately publish their traditional narratives. On the other hand, they live amid a literal sea of non-Aboriginals, most of whom do not know the first thing about the traditions of any Aboriginal nation, and not a few of whom believe that Aboriginals have no true tradition of any sort anyway, that they are just "primitive" people who have no written traditions of their own for the simple reason that they never had any to write down in the first place. Here then are at least two recent motivations for some modern Aboriginal communicators to seek collaboration with social scientists in having their traditional narratives recorded, translated, textualized and ultimately published: one, so that the mainstream society knows that they and their traditions really do exist, and, two, so that their right to control and use their traditional territories as they see fit and so that their inherent right to govern themselves may both be acknowledged by mainstream officialdom.

The McEachern decision was finally overturned on appeal by the Supreme Court of Canada, but the highest court's ruling did not by any means settle the question of Aboriginal land claims or that of

Aboriginal self-government in Canada. After hearing both sides' arguments in the case on 16 and 17 June 1997 the Court released its decision on 11 December 1997. It ruled, first, that the land entitlement issue had been marred by certain procedural defects at the original BC Supreme Court trial and therefore ordered a new trial; second, that a new trial was needed in any case,

so that the aboriginal perspective on their practises, customs and traditions and on their relationship with the land, are given due weight by the courts. In practical terms, this requires the courts to come to terms with the oral histories of aboriginal societies, which, for many aboriginal nations, are the only record of their past ... [and which] play a crucial role in the litigation of aboriginal rights. (Hurley, 1998, par. 84)

In other words, the Supreme Court of Canada advised that a new trial be conducted in order to determine the merits of the Gitksan and Wet'suwet'en land claims. And at this new trial, the Supreme Court expects lower courts to make special allowances for and not to act prejudicially toward those nations because their historical records exist only in oral form. This, of course, amounts to the Canadian State's official recognition of the Gitksan's and Wet'suwet'en's oral traditions, and, by implication, of the oral traditions of all Canadian Aboriginal Nations as well. But if the Supreme Court did order a new trial, it did not "encourage a resumption of litigation, however, advising the parties to settle their disputes through negotiations instead" (*Ibid.*).

Oral testimony as a way of witnessing, of saying or naming and sharing one's and one's people's pain and negative life experiences in the hope of ultimately obtaining some sort of redress by bringing them to the attention of the mainstream society is also fairly common. This may involve the telling of traditional narrative, it may involve the telling of one's life story, and in some cases it may involve a combination of both.

Lee Maracle is one of Canada's best-known Métis writers. Her life story narrative, told in the first person to political activists Don Barnett and Rick Sterling in 1972, was published, first, in 1975 under the pseudonym "Bobbi Lee," and later republished with an update in Maracles' own name in 1990. Though it does not involve

translation (Lee Maracle's native language is English), it is nevertheless well worth describing briefly here because of the insights it provides on the process of recording, textualizing and publishing life story narratives, which more generally do involve translation. To quote the introduction to the 1975 edition by Rick Sterling of the LSM<sup>3</sup> Information Centre:

Early in 1972, when Bobbi was working in the LSM Information Center, several of us were preparing for work in Africa with liberation movements. As practice for doing life history documentation with African peasants, workers and guerrillas, we began recording each other's life stories. Thus I recorded Bobbi's story, realizing as we proceeded that its publication could greatly enhance our understanding of racism and the struggle of Native people. However, because of problems and contradictions in LSM and its Native members at that time, coupled with my own inexperience, it was impossible to complete the story in the same penetrating vein with which it was begun. Then, when the idea of publishing the story was seriously taken up again last year [1974], our late Information Center director, Don Barnett, re-recorded the latter years of Bobbi Lee's life and brought it up to 1975.

In her 1990 prologue to the same narrative, Lee Maracle writes: "There are two voices in the pages of this book, mine and Donald Barnett's." She also writes that the original Bobbi Lee narration was distilled out of a transcription about twice as long as the final published version. She recalls having had many arguments with the editors as to what should be included in the book and what left out. She writes that she did not always entirely approve of the style of the book's passages, but that she eventually bowed to Don Barnett's judgement. She states that when preparing the 1990 manuscript she considered rewriting the whole thing, but eventually opted not to. But recall Maracle's words: "There are two voices in the pages of this book, mine and Donald Barnett's." What is particularly interesting in this publication is that Barnett's and Sterling's voices have been obscured behind a first person narrative style designed to give the

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<sup>3</sup> LSM stands for *Liberation Support Movement*, a Canadian West Coast more or less radical socialist collective that, among other things, published a number of books by African left-leaning political agitators. It appears to have largely ceased activities after Don Barnett's death in 1975.

impression that the voice is Maracle's alone. This, as I will show, is typical of a great many Aboriginal life history narratives.

Strangely reminiscent of the textualization of Maracle's life story narrative by social activists Sterling and Barnett is Elizabeth Burgos-Debray's recording, translating, textualizing and publishing of Rigoberta Menchú's telling of her life story. A member of Guatemala's Quiché First Nation, Rigoberta Menchú is active in the struggle to improve her people's lot in the face of the Guatemalan government's often brutally repressive treatment. Her primary reason for confiding her life history to Burgos-Debray's tape recorder appears to be a bid to embarrass the Guatemalan government into easing up on its treatment of the Quiché. Menchú spoke her story in Spanish and Burgos-Debray had it translated into English by Anne Wright, who writes that Rigoberta Menchú speaks:

a mixture of Spanish learned from nuns and full of biblical associations ... Spanish learned in the political struggle replete with revolutionary terms; and, most of all, Spanish which is heavily coloured by the linguistic constructions of her native Quiché and full of the imagery of nature and community traditions. ... The problem of translation was how to retain the vitality, and often beautiful simplicity, of Rigoberta's words, but aim for clarity at the same time. (From "Translator's Note," in Burgos-Debray, 1984, p. viii)

Although I agree that Menchú's language (in translation) is not complex, it is nevertheless perfectly adequate to express her often complex thoughts and descriptions. This is a very target-oriented translation—meaning that it retains virtually no trace of the original Spanish. In fact, Ann Wright's translation makes Rigoberta Menchú sound like an average North American woman with above average education—perhaps someone not unlike Anne Wright? Be that as it may, what Rigoberta Menchú, Lee Maracle and Delgamuukw all have in common is storytelling, and (except in Maracle's case) the use of translation in a bid by the narrators to free themselves and their people from an imperialistic government. Unlike the *Delgamuukw* case, however, the published versions of both Menchú's and Maracle's life history narratives are characterized by a typical obscuring of their recorders / transcribers / textualizers / editors / publishers' own considerable part in the process.

Sophie McCall argues that editors of Aboriginal first person narratives routinely achieve an appearance of pristine, exactly as told, narratives by obscuring their own active participation in the process. McCall cites Bakhtin's insight that "still current in linguistics are such fictions as the 'listener' and 'understander'.... These fictions produce a completely distorted idea of the complex and multifaceted process of active speech communication" (quoted in McCall 2002, p. 70). This is a crucial insight because the narrator does not sit alone in front of a tape recorder to which she tells her life history; she sits facing the ethnographer / linguist / historian / etc. The narrator tends to be intimidated by the process, and so her performance is usually anything but natural. She seldom speaks unprompted, but almost always in answer to her interlocutor's direct questions. The recording usually takes place over the course of multiple sessions that can stretch over days, months and even years. Following each session the recorder / interviewer goes back to her home / office / hotel room / trailer / tent, where she transcribes the day's tapes, analyses them, notes inconsistencies, and selects points for further elucidation. At the next recording session the very first thing that the narrator is asked to comment / elaborate on are precisely the points selected by the interviewer. The agenda is thus almost always entirely that of the interviewer, and even when the informant manages to retain a certain control over the recording process, the interviewer, by selecting and reordering the narrative after the fact, by controlling the translation process, and by doing the final editing without consulting with the narrator, still retains full control over what actually gets published and in what form. And yet, as McCall argues, this is routinely obscured, if not elided entirely.

In the introduction to her 1984 edition of Rigoberta Menchú's life history narrative, Elizabeth Burgos-Debray moreover betrays a good deal of sentimental attitude towards Aboriginals in general and Rigoberta Menchú in particular. This is the kind of anthropological writing (Burgos-Debray is an anthropologist) that James Clifford cautions against. He argues that virtually all ethnographic writing and the ethnographic life history narrative in particular is not only an allegory or a parable, but "a parable, almost always, in the pastoral genre." It is a harkening back to a time and place where human relations were / have always been / still are more

humane; it is, argues Clifford, symptomatic of a yearning for a time before the fall, a yearning for Eden (1986).

Consider ethnographer Burgos-Debray's assertion about Rigoberta Menchú's Quiché culture:

Within that culture, everything is determined in advance; everything that occurs in the present can be explained in terms of the past and has to be ritualized so as to be integrated into every day life, which is itself a ritual. As we listen to her voice, we have to look deep into our own souls for it awakens sensations and feelings which we, caught up as we are in an inhuman and artificial world, thought were lost for ever. (1984, p. xii)

Strangely, in Menchú's narration (in Anne Wright's English translation at least), there are virtually no traces of this pristine, "timeless" culture that Burgos-Debray describes. The woman that comes across the page is a very determined one who has no time for romantic notions of timeless pastoralism—she's far too busy describing the harshness of her environment and struggling to make life better for herself and for her people to waste time on such fancies.

Burgos-Debray describes the process of "collecting" Menchú's life story as one of interviews that she conducted by questioning her informant using a running tape recorder. Each evening, she transcribed the day's tapes and formulated a series of questions meant to clarify any ambiguous points before moving on to further life episodes, these last, also prompted by the ethnographer's predetermined questions—hardly an undirected process. And yet Burgos-Debray chose to delete all of her own considerable interventions from the final publication:

I soon reached the decision to give the manuscript the form of a monologue.... I therefore decided to delete all my questions.... I allowed her to speak and then became her instrument, her double by allowing her to make the transition from the spoken to the written word. [...] I had to insert linking passages if the manuscript was to read like a monologue.... I followed my original chronological outline, even though our conversations had not done so, so as to make the text more accessible to the reader. (*Ibid.* p. xx)

Here, we are obviously in the presence of precisely the kind of editorial intrusion that Donald Barnett and Rick Sterling practiced in *Bobbi Lee: Indian Rebel*—same reworking of the transcription, same choosing which parts to include and which to leave out, same appearance of pristine, exactly as told narrative, same deliberate obscuring of the social scientist’s own active participation in the process, and a very similar sort of subversion of the informant’s narrative. In the case of Burgos-Debray this editorial intrusion reflects her own sentimental notions of timeless, pristine culture rather than the narrator’s, while in the *Bobbi Lee* case, the narrative was twisted so as to reflect Barnett’s and Sterling’s own radical socialist agenda, rather than Maracle’s particular concerns.

Anthropologist Julie Cruikshank has spent a considerable amount of time working “in the field,” which in her particular case was the southern Yukon Territory, recording the life histories of three elderly Aboriginal women. She eventually published these in book form in 1990 (Cruikshank, 1990), but earlier, in 1983, she published a small portion of the same narratives by these three women in a “report” issued by The National Museums of Canada (Cruikshank, 1983). She explains that she undertook the research that led to the production and publication of her 1983 report to fulfill a contract with the National Museums of Canada, which called for her to “record, transcribe and translate Athapaskan myths of the Tagish and Tutchone in southern Yukon Territory to (i) analyse the complexity of themes in these stories, and (ii) compare these stories with versions previously collected” (1983, p. 1). Once in the Yukon, however, Cruikshank quickly discovered that the three elderly Aboriginal women who collaborated with her preferred to self-translate their own narratives, and they moreover all insisted in telling only those traditional stories that they wanted to tell, while further insisting that they would only tell these traditional stories as an integral part of the broader narratives of the history of their moieties,<sup>4</sup> of their families, and of selected autobiographical narratives. This is however never clearly and fully stated in either the 1983 report, or the 1990 book, and only by reading both of them together can one begin to understand the circumstances and form in

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<sup>4</sup> Social scientists use the word “moiety” (after the French “moitié”) to designate each of the two descent groups into which the population of certain human groups fall.



which the “myths” and life history narratives on which both publications are based were “recorded” and edited for publication.

Cruikshank does state that these “women seemed to consider these stories an essential component of personal history” (1983, p. 24). And yet, in 1983 she presents all of their traditional narratives in disembodied form, amputated, as it were, from the broader narratives of which the narrators say they are integral parts. It is true that she had a contract to fulfill, which called for her to “record, transcribe and translate Athapaskan myths,” and given that personal history is not generally accepted within the definition of “myths,” she probably had little choice but to surgically separate the traditional narratives from the context of their telling in order to fulfill her contract. Another of the major themes of the 1983 report is also puzzling. Cruikshank states repeatedly (1983, pp. 12-19) that the stories presented in it can be divided into two categories: those with a male protagonist, and those with a female protagonist. She further states that those with male protagonists feature vision quests in the course of which the protagonist acquires a spiritual helper with whose assistance he then saves his community, while those with female protagonists feature women taken against their will on a journey to a parallel world from which they can only escape by performing ordinary domestic tasks exceptionally well. The problem with that assertion is that while some of the stories with female protagonists are similar to what Cruikshank describes; some of the other stories presented do not fall under her two stated categories. For example, the “Mountain Man *Shat’okâw*” story (1983, p. 75), and the “Wolf Man” story (1983, p. 79), both feature women protagonists who go on vision quests from which they return with spiritual helpers with whose assistance they subsequently save their communities.

Cruikshank presents some of these same traditional stories in her 1990 book *Life Lived Like a Story*, though in verse form similar to imagist poetry, and (in a way) accompanied by the life histories of which they were originally an integral part. I write “in a way” because, although the traditional narratives appear next to life histories in *Life Lived Like a Story*, they are not actually restored to their original position in the greater narrative into which they had been embedded by their narrators, but placed separately, in verse form, next to selected sections of life history narratives, some of

which, Cruikshank admits, were not necessarily those within which the tellers originally chose to tell them. To further set them apart from the traditional narrative sections, the life history narrative sections are presented in prose form. Why this difference in presentation? Are the traditional narratives presented in verse to make them appear more poetic? And is the goal in presenting the autobiographical narratives in prose form to make them appear more real? More true? But what of the tellers' insistence that there is no difference between the life history narratives and the traditional narratives, that both are equally true and that they cannot be separated?

Julie Cruikshank also admits to having extensively edited the narratives for publication, but she insists that all the words appearing on the page are those of the contributors. Of one of the narrators, for example, she writes: "I have chosen to edit her account more than the others—not by changing her words, but by rearranging them to meet the grammatical demands of English when such reorganization seems to make her meaning clearer" (1990, p. 268). And, just as did Burgos-Debray, Shostak, and Sterling and Barnett, Cruikshank has also removed all traces of her own, also considerable, part in the process. Upon first reading them, Cruikshank's textualizations of Tutchone and Tagish narratives appear to provide a reasonably good representation of Tutchone and Tagish worldviews. A closer look, however, reveals that they are selective, fragmented and personalized versions (Cruikshank's) of those worldviews. These versions that may in fact, to again use the words of James Clifford, be allegories "of scientific comprehension, operating at the levels both of cultural description and of a search for human origins..."; and which, like *Nisa*, may well be more "a Western feminist allegory, part of the reinvention of the general category 'woman' in the 1970s and 80s..." (1986, p. 104), than a true representation of three elderly Aboriginal women's own worldviews.

The above claims, of course, beg the question: have any fair representations of Aboriginals' worldviews been published? The short answer to that question is, perhaps. In 1999, anthropologist Dominique Legros published Northern Tutchone elder Tommy McGinty's self-translation in English of his own Tutchone language

telling of his nation's traditional creation narrative<sup>5</sup>. Legros had been gathering ethnographic data among the Tutchone ever since 1970 when, as he puts it:

At the beginning of my last long fieldwork (1990-91), their band council requested as a condition to my planned study that I now research and write *for* Them, not *on* Them for *Them* in the outside world, "down South." Although worded less abruptly, in short, it requested that I become scribe to Mr. Tommy McGinty, one of the most learned elders in Tutchone culture and a long-time friend and ethnographic consultant. After discussion, part of Mr. Tommy McGinty's project became taping in English all the sacred Tutchone narratives he knew and having me write them down as well as getting them published. (1999, p. 19)

Legros' work in general and his recording, transcription, textualization, editing and publication of Tommy McGinty's narration in particular has yet to undergo the test of full and thorough analysis and therefore, at this point, should still be treated with caution. And whether collaborations such as Tommy McGinty's and Dominique Legros' will prove more satisfying to First Nations in the long run has yet to be determined. Strategies calling for elders to self-translate their narratives orally for ethnographers or other social scientists to textualize and publish on their behalf are however probably only an intermediary step. Once elders become fully literate, why should they require the help of "scribes" to textualize their traditional narratives for them? In a 1995 paper presented at the McGill Institute for the Study of Canada's Sixth Seminar, archaeologist and ethnohistorian Bruce Trigger suggested that they may well do their own "scribing" in a not so distant future when he asked: "Can Non-Native Scholars Write a History of the Native Peoples of North America? Will the Cambridge History of the Native Peoples of North America be the Last Scholarly Account of Native

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<sup>5</sup> I am well aware that traditional creation narratives are more usually called "creation myths;" however, I agree with Dominique Legros that "to use words like *myth* for an Other's creation narrative is as crude, coarse and unacceptable as to address a Christian audience on the *myth* of Jesus or a faithful Jewish assembly on its *myth* of *Exodus*" (1999, p. 20). Accordingly, I am using the phrase "traditional narrative," rather than the word "myth" throughout this article.

Peoples by Non-Aboriginal Scholars?" (Quoted in Legros, 1999, p. 21).

Various forms of self-representations by Aboriginals in which highly controlled self-translation is involved and addressed to both fellow Aboriginals as well as to the non-Aboriginal public at large do in fact exist. I am thinking, for example, of Inuit filmmaker Zacharias Kunuk, whose award-winning feature film *Atanarjuat, the Fast Runner* was projected on screens all over the world in its original Inuktitut language version with subtitles in the local language of the audience appearing on-screen. This subtitling strategy, according to Sophie McCall, was not an innocent choice on the part of the Inuit filmmaker, whose "politics of partial translation" are a deliberate attempt at keeping ultimate control over traditional Inuit narrative in Inuit hands. The first part of the title of McCall's 2004 article ("I Can Only Sing This Song to Someone Who Understands It") is taken from the opening scene of Igloodik Isuma Production's now famous film in which a character (Kumaglak) refuses to sing his song for a mysterious stranger on grounds that the stranger would not understand it. "Kumaglak will not sing because he doesn't know how the listener will receive, retell, and reuse the song for his own purposes. His suspicions prove to be well-founded: the 'up-north stranger' murders him..." (2004, p. 19). McCall sees this scene, which opens the film, and especially the statement by Kumaglak as a metaphor of the way in which Euro-Canadians have plundered Inuit art, stories and songs and appropriated them for their own use with little regard for what they might have meant to those who created them. A film such as *Atanarjuat*, she argues, serves to allow Inuit artists to re-appropriate their own culture. Furthermore:

The subtitled film enables the filmmakers to create two parallel texts that interact and speak to each other in complex and imperfect ways. The gap between what is spoken and what appears on the bottom of the screen can be manipulated strategically, for a variety of effects, enabling the filmmakers to address different audiences. The book version of *Atanarjuat*, by Paul Apak Angirlik and others, which includes screenplays in both languages (Inuktitut and English), film stills, interviews, personal essays, and ethnographic commentary, adds still more layered and variant tellings to the oral script. The film's strategy of partial translation highlights the space of cultural contact and difference in acts of textualizing orature and orality. The filmmakers thus resist the

powerful explanatory impetus of the genre of the ethnographic film, which presumes to elucidate the roles and purposes of cultural practices for outsiders. (*Ibid.*, pp. 26-27)

McCall further argues that anthologies that include Inuit songs—such as Edmund Carpenter’s *Anerca* (1959), Jerome Rothenberg’s *Shaking the Pumpkin* (1971), James Houston’s *Songs of the Dream People* (1972), John Robert Colombo’s *Poems of the Inuit* (1981), Penny Petrone’s *Northern Voices* (1988), Daniel David Moses and Terry Goldie’s *An Anthology of Canadian Native Literature (1992-1998)*—“have constructed the songs as imagist poems and isolated them from their original contexts. The presentation of the songs as isolated fragments ignores the storytelling interactions and the social contexts of the telling” (*ibid.*, p. 21). In her article a good deal of space is devoted to comparing the 1921 film *Nanook of the North* with *Atanarjuat*, which deliberately repeats several shots and scenes from *Nanook*, to “correct,” believes McCall, the false impressions of the culture given by the earlier film. She condemns the re-production of Inuit culture in out of context bits and pieces, and the appropriation by Euro-Canadians (or even by Aboriginal southern Canadians such as David Daniel Moses) of Inuit Orature, which they re-present in modified form that closely approximates forms of Western literary art (imagist poems, for example), or which they use as inspiration to create a Canadian literature that romanticizes the original Inuit Orature into something largely foreign to its original creators’ intents (*Ibid.*, pp. 24-25).

So why then do they do it? As indicated at the beginning of the article, the evidence suggests that in the past many Aboriginal Elders collaborated with social scientists for altruistic reasons. More recently, there is no doubt that Aboriginal communicators’ primary motivation is to bear witness to past wrongs perpetrated against them and their people by various agents of the mainstream society. They often do this in a bid to have those wrongs righted, or at least to bring about their cessation. In some cases, such as the *Delgamuukw* case, the witnessing takes place in a formal courtroom setting, and has had a small measure of success recently, though Aboriginal participants are far from satisfied and their struggles, in and out of the courtroom, are ongoing. As I have attempted to show, however, the motives of social scientists who seek the collaboration of these same Aboriginal communicators, although generally purported to be in the interest of

the advancement of scientific knowledge about “preindustrial”<sup>6</sup> societies, as well as to have the best interest of Aboriginal Peoples at heart, often tend to reflect the worldviews of the social scientists and of the societies whence they sprang, rather than those of their Aboriginal collaborators. These social scientists moreover often routinely obscure their own considerable interventions and input into the process of the collaboration as well.

Sophie McCall proposes a complete reversal of the current process of recording, transcribing, translating, textualizing and publishing Aboriginal narrative. Since the influence of the narrative’s collector / transcriber / translator / editor / publisher can never be eliminated entirely, she argues, the next best thing would obviously be to make it entirely transparent. This means that each and every action of all participants should be clearly laid out in the final publication. At the very least, it should mean publishing all of the interviewer’s questions, interventions and promptings during the recording process, as well as a candid description of all manipulations that the informants’ words have undergone throughout the process. McCall argues that “Bakhtin’s discussion of how ‘heteroglossia’ emerges from rigidly hierarchical social relations is particularly relevant...” (2002, p. 75). A fully transparent publication of Aboriginal life history narratives would then clearly show that their production was a dialogic process, and not the purported monologues that they are made to appear.

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<sup>6</sup> In a bid to do away with formerly commonly used, but connotatively pejorative, terms such as “primitive” to describe societies that have not chosen to adopt modern industrial lifestyles, many anthropologists, including James Peoples and Garrick Bailey in their cultural anthropology introductory textbook, *Humanity* (New York, West Publishing Company, 1994), often use the term “preindustrial” in its stead, evidently failing to grasp that such a term remains potentially pejorative since it seems to imply that such societies have only managed to reach a lower level of development and are bound to eventually adopt a fully industrial life style, which, as the term appears to suggest, would be the culminating, end point of human evolution.

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**ABSTRACT: Why Do They Do It?—A Brief Inquiry into the Real Motives of Some of the Participants in the Recording, Transcribing, Translating, Editing, and Publishing of Aboriginal Oral Narrative** — This article inquires into the motives of the participants in the recording, transcribing, translating, editing and publishing of Aboriginal narrative. The motivation of Aboriginal communicators, at the outset simple altruism, has evolved onto a pressing need to bear witness to past and present wrongs perpetrated against them by various agents of the dominant society. Social scientists' motivations are equally complex. Most of the social sciences, and particularly anthropology, practice translation. Anthropology has elaborated translation theories that betray a general unease with how and why anthropologists translate. Anthropological translation differs from that of other disciplines in that when anthropologists translate oral and written "texts," their ultimate aim is in fact the "translation" of the cultures that produced them.

**RÉSUMÉ : Pourquoi le font-ils? – Une brève enquête sur les motifs de certains des participants à l’enregistrement, la transcription, la traduction, la révision et la publication des récits oraux autochtones** — Cet article analyse les motifs pour lesquels certains des participants acceptent l’enregistrement, la transcription, la traduction, la révision et la publication des récits autochtones. Les raisons d’agir des communicateurs autochtones, au départ caractérisées par un simple altruisme, ont évolué vers un besoin urgent de témoigner des torts, présents et passés, perpétrés contre eux par divers agents de la société dominante. Les motifs des sciences humaines sont également complexes. La plupart d’entre elles, et l’anthropologie en particulier, font appel à la traduction. Les théories de la traduction de l’anthropologie trahissent un malaise général envers ses pratiques et ses motifs traductifs. La traduction anthropologique diffère de celle des autres disciplines du fait que lorsque les anthropologues traduisent des « textes » oraux et écrits, leur but ultime est en fait la « traduction » des cultures qui les ont produits.

**Keywords:** anthropology, translation, Aboriginal, oral narrative, cultures.

**Mots-clés :** anthropologie, traduction, Premières Nations, récits oraux, cultures.

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