

Anthropological Participatory Research among the Innu of Labrador

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This paper is a discussion of research with people rather than on people. The author reviews the principles of advocacy work in anthropology, and participatory research, in terms of her work with Innu people in Labrador. The methodology she adopted had the important goal of involving community members in the research process, and the more questionable aim of raising their consciousness. Kurelek concludes that good participatory research allows community members to raise our consciousness.

Introduction

We were talking, as usual, about research proposals and dissertation research. The conversation got around to the obvious contradiction between our approaches to education and research. He mentioned something called participatory research where you did research "with" rather than "on" people. I've got to find out about this. [Personal Journal, September 1983, Maguire, 1987, p. 28]

This description from Maguire's book, *Doing Participatory Research: A Feminist Approach*, is similar to my own entry into the realm of participatory research. Although I was trained in the tradition of anthropology that does research "on" people, my introduction to feminist methodologies and participatory research in a graduate seminar taught me a different approach – one that sought to empower rather than exploit those studied.

Within this paper I will discuss the potentials and possibilities for participatory research based on a review of the literature on this topic, and by referring to anthropological writings on advocacy work. This discussion will be couched in the story of my experiences with Innu people in Labrador, with whom I have worked for the past three years. The aim of

this paper is, first, to promote an approach to research that seeks to improve the lives of those under investigation, and to provide them with control over the research process — participatory research; and, second, to describe to those already "converted" to this methodology the difficulties and successes I experienced in my application of participatory research principles.

What follows is a discussion of participatory research and my work with the Innu people to implement this research approach. This will be done to clarify some of the difficulties of doing research with people rather than on them.

Anthropology and Participatory Research

Although participatory research was made famous by educators and is more often used in the Third World, there are some researchers who apply it in the West. There are even a few anthropologists in Canada who believe that participatory research is the correct approach to working with others (Ryan and Robinson, 1990; Warry, 1990).

Certainly, anthropology has a history of doing work for those studied, but this work has varied in extent and fashion. While some claim we have been doing such work since the beginnings of anthropology (Jacobs, 1974; Schensul, 1987), others claim the opposite, and accuse the discipline of either working *against* those we study, or of being oblivious to them (Kayongo-Male, 1979; Wright, 1988; Trinh, 1989). Increasingly within the past few decades, however, there have been calls by anthropologists for their colleagues to produce work that will benefit those they study.

During the 1960s an outcry arose, stemming from the politicization of academics during the Vietnam era, and in response to the realization that some scholars were working with Western governments against Indigenous peoples. In December 1968, for example, *Current Anthropology* published articles by Berreman, Gjessing and Gough that condemned the discipline for failing to do ethical work and work that would be of use to those studied. The responses to their criticisms, which ranged from commendation to condemnation, were also published in that issue. While some respondents applauded their efforts to evoke work that could be used by more than ivory-tower scholars, others viewed their ideals as problematic. Those who criticized the authors accused them of being naive and narrow-minded for assuming that doing non-objective work would solve the world's problems. These reviewers pointed out that merely choosing a cause or adopting a group to work for did not guarantee beneficial results (see also Chambers, 1987).

Applied anthropology is a case in point. Schensul describes applied anthropology as research designed to improve the lives of the people studied (1987, p. 211). Chambers' definition of one type of applied anthropology – action anthropology – is perhaps more enlightening. He says anthropologists adopting this approach “make themselves available to appropriate clients who determine how anthropology might best contribute to their needs” (p. 321). For whom the researchers work is not specified.¹ No stipulation is placed on the ethical considerations of this work. Ethnographic work at the turn of the century, for example, was often commissioned by government agencies who provided information to colonial administrators.

Anthropology, when applied (i.e., used for more than intellectual theorizing), has a history of providing support for the status quo. Our already bad reputation among Indigenous peoples spread during the 1960s, when intelligence information was collected for the “Establishment” at the peril of those studied (Chambers, 1987).

For this and other reasons, the work of applied anthropologists has been marginalized by the discipline. Because of its more pragmatic rather than esoteric approach to data collection, applied work is believed to be “lacking in intellectual rigor, ethically suspect, unimaginative [and] bereft of theoretical sophistication” (Chambers, 1987, p. 309). Schensul (1974), himself an applied anthropologist, explains that part of the difficulty stemmed from a belief that applied work contradicted two of the basic tenets of anthropology – objectivity and value-free research: “We learned to suspect that applied anthropologists were people who couldn't ‘make it’ in pure research. They were seen as tools of colonial oppression, government spies, incompetents, or worse, as atheoretical” (p. 203).²

This perspective harkens back to the days of anthropology when what we wrote about those we studied was believed to be inconsequential to them. The primary concern then was to contribute to the theories of culture in vogue at the time. Each anthropologist had his (rarely her) own “tribe” to study (Cassell, 1977), the members of which were unlikely to read the material produced about them.

Today the situation is different. It is now not uncommon for those we study to read and critique our work. These critiques condemn work that is irrelevant and/or exploitative (see Richer, 1988, or Wright, 1988, on this topic). Some scholars attribute this realization that our work can be exploitative to a rise in Native consciousness and the move towards self-determination. Others attribute an awareness of the need for useful research to the work of action researchers who worked closely with

community members to improve their lives (Wright, 1988).

Action anthropology (using Tax's definition — see note 1) is accorded more respect, but it also plays a marginal role in the discipline. Made famous by the Fox and the Vicos projects, here was an anthropology that sought to produce results not only on paper but also in the lives of those studied. These and other projects were long-term and sought to involve those studied in the research process with the understanding that their conditions could be improved.

Interestingly, Ryan and Robinson (1990) view this understanding of action anthropology to be similar in nature to the principles of participatory research. In both approaches a problem that originates in the community is addressed with the ultimate goal of transforming social reality to better the lives of those involved. "The beneficiaries of the research are members of the community itself" (1990, p. 58). Community members, who have been defined as powerless people, are actively involved to facilitate self-reliant development. And finally, the researchers are militant, not detached in their involvement. Although the participatory researcher Budd Hall does not credit the action anthropology of Sol Tax (famous for his work on the Fox project), Ryan and Robinson state that they are "brothers in thought."

Unfortunately, the type of action-oriented work seen in the Fox and Vicos projects is the exception to rather than the rule in mainstream anthropology. During the 1970s our focus on the needs of those we were studying was distracted by the navel-gazing of reflexivity (Ottenberg, 1990). This approach has been developed and replaced by the ineffectualness of post-modernism (Marscia-Lees, Sharpe and Cohen, 1989; Goetz, 1991). Even feminist work has been problematic for its focus on the values of middle-class Western women (Mohanty, 1988; Trinh, 1989; Goetz, 1991).

While there continues to be work done by applied anthropologists — work that has practical rather than esoteric value — it is often buried away in reports that fail to have any impact on the atmosphere of the discipline (Wright, 1988; Tough, 1990). And while some anthropologists continue to do work in the spirit of the Vicos and Fox projects (i.e., *with* people rather than on them), this work too is marginalized.

Ideologies that state that all work done should be in consultation with and of benefit to those we study have failed to revolutionize our discipline. I, and others (Marscia-Lees, Sharpe and Cohen, 1989; Warry, 1990), argue that anthropology continues to be an exercise in which data is collected primarily for the benefit of those who do the studying, not for those who are studied.

Experiences with the Innu

Despite the follies of the discipline, I entered a graduate program in anthropology with the intention of doing "something different" and "something useful." I hoped that, by using the methodologies of feminism and participatory research, I would be able to avoid the bad reputation my discipline had among Indigenous peoples, while still working with those traditionally studied by anthropologists — Native people.

My initial interest, however, was in the study of women. Once I had discovered the work of feminist anthropologists, which showed me that I did not have to limit my studies to the study of men by men, I was hooked. At the graduate level I learnt not only that the study of women's lives was valid, but also that a different approach to research often accompanied these studies.

I read the works of scholars such as Oakley (1981), Duelli Klein (1983), Mies (1983), Reinharz (1983) and Mbilinyi (1989), who spoke of avoiding positivistic research in favour of work with women. They explained that adhering to the tenets of positivistic research — value-neutrality and objectivity — failed to improve the lives of women who were oppressed the world over. The problems stemmed from the fact that women's issues were not taken into account and the nature of women, as defined by social scientists, was limited.

Mies and others spoke of doing work in conjunction with women to bring about change in their lives. Her technique, about which I later learnt more from Maguire (1987), involved a feminist adaptation of participatory research.

During my readings of these scholars and my investigation into "alternative" subject matter and methodologies, I was formulating a thesis topic. Initially I thought I would explore the link between prostitution and militarism in Southeast Asia. After attending a presentation by several Innu men,³ who voiced their concern over young Innu women "prostituting" themselves with military men from a nearby military base (see also Foulliard, 1989, and Canadian Peace Alliance, no date), I realized I could research the issue much closer to home.

This course of action was halted, however, when I was told by one of the feminist scholars in my department about the inappropriateness of such a topic for a White researcher. She believed that investigating a topic that linked Native women to prostitution would only serve to reinforce a stereotype of Native women as "whores." I decided to redirect my focus to participatory research and offer my services as a researcher to the Innu people.

As touched on earlier, my first introduction to some of the principles

behind participatory research came from scholars such as Mies (1983), Reinharz (1983) and Maguire (1987). They advocated an approach to research in which the people they studied determined what needed to be done, how the work would be done, and what would be done with the results.

Based on these readings my interpretation of the approach was to let the Innu decide how my work should proceed. I did, however, have a few ideas of my own as to what might be useful research.

The Innu people (Naskapi-Montagnais) with whom I was interested in working were those who were famous for their protests against the building of a NATO base at Goose Bay, Labrador. They saw the proposed base and the low-level flying that was taking place over their camps (and which continues today) as another form of abuse imposed on their life-style and livelihood by the Canadian state (Ashini, 1989; LaDuke, 1990, 1991).

With a population of 15,000 in total, approximately 2,000 Innu people live in Labrador, while the rest are in Quebec. Although the Innu people of Sheshatshit do not recognize themselves as citizens of Canada, they see themselves as the inhabitants of *Nitassinan*, and have entered into land claim negotiations with the Canadian government to halt further development of their land (*Innu News*, 1991).

In addition to their protests against NATO, for which they gained world-wide notoriety, the Innu people in Sheshatshit have successfully used protests and civil disobedience to halt logging on their land and to gain control over their children's education.

The women of Sheshatshit played a key role in the protests (Manning, 1989; LaDuke, 1991), and because of my feminist values, I hoped to work with these women in some manner. I thought I might record the effects of NATO on Innu women or perhaps explore how they had organized their protests. Once again, however, my plans were rerouted – this time by an Innu woman. Rose Gregoire informed me that her community did not need research of this nature. She suggested, instead, that I spend time in her sister's camp in the bush (my first visit was in 1990).

The Innu spend up to six months of the year living in camps and engaging in the hunting and gathering life-style they lived year-round until thirty years ago. During the spring and the fall (money permitting) families are flown out to camps many kilometres to the north and south of Sheshatshit. The time spent in the bush is called "going to the country." The Innu also "go camping" in the bush surrounding Sheshatshit for briefer periods of time during the rest of the year. Family unity, serenity, traditional values and wild food are just a few of the benefits the Innu

people receive when they leave the community for the bush (Armitage, 1990).

It was in this context that I first met the Innu people. As I agreed to Rose Gregoire's suggestion, I promptly flew out to her sister Elizabeth's camp. There I met her family (the Penashues) and two others while I learned, among other things, to fish, make bread, collect boughs and scrape caribou skins. I got to know people who laughed a lot, who were close-knit and who lived in harmony with their surroundings, a sharp contrast to life in the community.

While I was in the country, I sought to avoid the traditional role of anthropologist. My intention was to avoid objective work in which the people with whom I lived were my subjects. I decided not to "study" the Innu by taking field notes. Instead, I merely kept a journal. I think, however, that even my journal writing made those whose tent I shared nervous. It was later explained to me that they had no way of knowing what I would do with what I learnt. Was I a CSIS spy?⁴ Would I write a book similar to *A Poison Stronger than Love* (Shkilnyk, 1985), which described Ojibwa people in a very unfavourable light?

My intentions were to do work for and with the Innu. I intended to neither impose a research agenda on the people, nor use my time with them as a source of data to produce "yet another ethnography" on Native people. I wanted to avoid treating them as subjects for the development of anthropological theory as researchers have done in the past (see for example Leacock, 1991, or Henriksen, 1988).⁵

But what was I doing in the country? Why did Rose Gregoire suggest I go there? And once I returned to the community, how would I make myself useful? In Sheshatshit, Gregoire provided me with a topic for research and confirmed the offer of two community members (Jack Penashue and Anne Riche) to work with me gathering information. Gregoire decided it would be useful for the three of us to interview people about problems in the community as opposed to the tranquil life of the country.

My efforts to implement participatory research met with several difficulties. The main problem was that neither I nor those with whom I worked were clear on how to implement the methodology. I understood that I should take direction from the Innu — I did this by getting Gregoire to direct my work, and by getting community members involved in the process, which I was doing by working with those interested in participatory research. There was, however, an additional element to participatory research that I was not sure how to implement — that of education.

Several scholars in their discussions of participatory research mentioned

the educational component of participatory research, described alternately as "consciousness raising" or as "an educational process" (Hall, 1981; Tandon, 1981, 1988; Mies, 1983; Maguire, 1987; Gaventa, 1988). The goal of participatory research that makes it unique, and differentiates it from, for example, action research⁶ is that of stimulating critical consciousness among oppressed peoples so they would take actions to liberate themselves.

Authors vary in their understanding of where this consciousness comes from, i.e., who perceives the problem to be addressed and who facilitates the educational process. Freire (1990), considered one of the fathers of participatory research for his alternative approach to education with Brazilian peasants, claimed that the oppressed need the assistance of teachers and revolutionary leaders to free them from the "internalized image of the oppressor and [his] adopted . . . guidelines" (p. 31). Hall (1981), Maguire (1987) and Gaventa (1988) mention the use of outside consultants or experts who provide impetus for the work and tools to acquire knowledge for power. In most of the popular literature on participatory research there is an assumption that outside expertise is necessary if oppression is to be perceived and overcome.

Yet Tandon (1988; see also Gaventa, 1988, and Small, 1988) claims that participatory research is Indigenous to oppressed peoples: "throughout history, popular systems of knowledge and an alternative system of knowledge production have existed parallel to the dominant system" (p. 6). Although each of the participatory research projects that I reviewed include, at the very least, *consultation* with outside "experts," a few of the projects do describe work initiated by community members, not outside participatory researchers (see for example Hudson, 1982; Jackson and McKay, 1982; St. Denis, 1989). Ryan and Robinson (1990) perhaps best describe the position of outside "experts" in participatory research. They state that one of the goals of the research approach is for the outsiders to eventually work themselves out of a job.

In terms of my work that first summer (1990) with the Innu, I felt that my job as a participatory researcher was to teach Jack Penashue and Anne Riche to do research to make the need for outside researchers obsolete. In this way, I believed, I would fulfil the educational tenet of participatory research. This plan failed to materialize. Riche was too busy with demands on her time as a mother, wife and wage-earner to do more than interview people. Penashue was willing to work more closely with me, but I was both unable and unwilling to work co-operatively with him to arrive at an appropriate approach for interviewing. I perceived that a schedule of questions needed to be written to illicit "correct" answers for the topic Gregoire suggested. I incorrectly assumed that only a social scientist could

provide the appropriate format for this research.

In retrospect, I see that, had I worked more closely with Penashue and other community members to determine how we would acquire the information we were seeking, we would likely have had better results. As it was, the women interviewed complained that the questions focused too much on the individual and failed to allow them room to discuss their families. Open-ended questions or even just an introduction to the topic would have been more appropriate. Penashue explained later that asking direct questions of elders was considered rude. Yet the questionnaire I wrote did just this. I had, however, shown the questions to a few community members and had Penashue and Riche pre-test it on their spouses. No mention was made of an alternative approach, likely because I was the outside "expert" and it was my money that was paying them to do the interviewing. Unfortunately, none of us really understood how to approach participatory research.

St. Denis (1989) reported not dissimilar findings in her work with Native people in Fairbanks, Alaska. Herself an outsider (Cree-Métis from Saskatchewan) she was, however, hired on the initiative of the Fairbanks Native Association. The association decided to adopt a participatory approach to a community needs assessment project. Because of a lack of familiarity with the approach, people were mistrustful of the project and found it difficult to distribute control over the work. The co-ordinator complained about not wanting to assume too much authority, but at the same time she felt it was thrust upon her. Similar to my work, however, overall there was a successful community response to the work. St. Denis attributes this success to the fact that, except for herself, everyone involved in the project was from the community.

Arratia (1992), based on her examination of participatory research in Chile, found less success when outside intellectuals initiated and managed community research projects. This forcing of the process by outside intellectuals led to problems. Arratia's work examines the "receiving" populations' perceptions of the several types of participatory research implemented in their communities. In general, the people were accepting of projects that sought to improve their material conditions in life. Projects that had less tangible results, like fostering cultural revival, were viewed with scepticism and even criticism. Interestingly, without her having done this review to assess the projects' worth, the intellectuals who did the work would have considered their projects successful. Arratia concluded that:

[T]he vision of the process and its outcome as held by the external agents (and planners) was quite different to that of the members

of the communities. And, these differences were never identified, nor made explicit, as indeed they should have, if the process was to lead to a different (less asymmetrical) kind of relationship. Apparently, everyone performed their expected roles and nobody complained. On the surface, things were going just fine. [pp. 116-117]

Although some of the projects were successful – for instance those that improved the production of cash crops or the marketing of wool – in general there was a lack of understanding as to what participatory methods were.

Indeed, "the recipients" could not understand this new approach, but worse still, "the deliverers" did not fully understand it either. ... They knew that these methods were an "up and coming" trend for those of the "correct" political persuasion. [Arratia, 1992, pp. 116-117]

The projects that were successful took time – years of involvement on both sides.

My experiences with the Innu showed that the longer we worked together the more successfully participatory our work was. During my first summer in Sheshatshit, community involvement and control was evidenced by Gregoire's choice of topic and the flexibility to which I approached my work with Penashue and Riche. They decided who they would interview, using my questions only as a guide. Community support was clearly evidenced by people's willingness to participate without remuneration (only Penashue and Riche were paid). This likely stemmed from the relevance of the topic to people's lives and my assurance that the work was being done for the community resource centre. I had no intention of using the information for my own research purposes.⁷

Maguire's (1987) participatory research with battered women in Gallup, New Mexico, encountered difficulties that stemmed from a lack of community commitment to the work. She found that, although the women she gathered together for consciousness raising found it empowering to realize they were not alone in their suffering, they failed to continue meeting after Maguire stopped organizing the sessions. She was also disappointed that the women failed to make a structural analysis of their oppression. However, they successfully recommended that the area's shelter for battered women not use male volunteers.

Maguire attributes the possible difficulties in sustaining the meetings to a lack of transportation and child-care for the women. She makes no

mention of financial support for the work. An added difficulty arose from her being of a different cultural heritage than most of the participants (Maguire is White, while most of the battered women are Navajo).

Her decision to initiate the work, and the consequently limited success of the project, leads us to question what our role is as non-Natives working in Native communities. While some scholars continue to do ethnographic work to titillate our fascination with "the other" (see Cove, 1987), others do work *for* Native peoples — particularly in the area of land claims. How we approach our relationship to the communities we work in varies from researcher to researcher. Once we bridge the distance that positivistic work encourages us to maintain, we are likely to find our role in the community unclear.

An interesting illustration of the changing role in which anthropologists find themselves is illustrated in Talbert's (1974) account of her experiences at Wounded Knee. She described how her role as an anthropologist became obscured, first, because of the crisis situation into which she entered, the occupation of Wounded Knee, and secondly because of the demands of the Native people with whom she stayed. The role given to her was that of cook. On one occasion, when she was called upon to use her skills as a researcher, the women she worked with were displeased.

Talbert's situation is not unlike my experience in Sheshatshit, where I found myself doing household chores much more than I do in my home in Ottawa. One could argue, of course, that this is the nature of participant observation, the traditional tool of anthropologists today (at least for a female anthropologist). Perhaps, however, I know I would not feel comfortable (nor could I afford) being waited on as the classic anthropologists were. While I may have unintentionally been doing *participant observation*, I would argue that, for an outsider, to do effective *participatory research* you need to get to know the people with whom you work and allow them to know you.

As this understanding grows, so too do friendships. Whether this by-product is beneficial or not in terms of participatory research has not been discussed. Jacobs (1974, p. 213), doing action-advocacy anthropology in an impoverished Black community seeking to establish a health centre, found a lack of distance between her research team and community members:

At some point, we each understood that the tradeoffs were worth more than could be summed up in academic language, or passed off lightly. We had formed bonds of mutual caring, and for some of us, deep friendships.

These relationships facilitated her working in conjunction with people in the community. Others argue these friendships contradict "appropriate," i.e., positivistic, research principles (see Tough, 1990).

I believe that the friendships I have in Sheshatshit facilitate my work there. After my first summer in the community, I made a return visit around Christmas for purely social reasons, which helped develop my friendships there. Had it not been for these relationships, I would not have learnt of the dissatisfaction people in the community had with the topic I proposed for my second summer (1991) in Sheshatshit.

My plan had been to question people about the methodology we used the summer before to see where I had gone wrong and in what ways the work was a success. I questioned the usefulness of our work when the tapes we collected were misplaced for six months without anyone noticing. I felt, however, that the work was appropriate in some ways, since community members had selected our topic, done the interviews and then retained the information. By this time I had decided to write my thesis on methodology rather than produce yet another ethnography on the Innu.

When I mentioned that I hoped to return and interview people about our "participatory research," several people expressed disappointment. They were not interested in me returning as a researcher. They wanted me to come as a friend. I felt, however, that with the generous funding I had received from the Northern Science Training Fund, a thesis still to produce and a lack of identity in the community, I needed a project. What we settled on, to the satisfaction of everyone involved, was a life history project.

I thought it would be interesting to contrast the story of Rose Gregoire, who spent most of her life in the community, with that of her sister Elizabeth, who grew up in the country. They expressed interest in a life history project but suggested that more women be included in the book. As well, they wanted the focus to be on life in the country, rather than on the contrast that I had suggested. During the process, a second theme arose – the protests against NATO. The result was the beginning of an exciting process during my second summer in Sheshatshit.

I worked with several women to organize meetings in which their individual stories and their group discussions were recorded. As well, we worked toward getting the recordings translated. Since their first language was not English, they told their stories in Innu-eimun.⁸ I was inspired by their support of the project as they co-operated to arrange for child-care, transportation and participation with each other. At times, my preoccupation with housework and socializing led me to abandon the work for a while. This stemmed from my responsibilities in the home in

which I stayed and my doubts that the women were really interested in the work. When they were too busy to participate, I worried I was imposing my agenda on them and they were involved only to oblige me. However, when I was away from the work for too long I was reminded, by several of the women, to keep at it.

Last summer (1992) (precipitated by another social visit in March) I had similar doubts because I was having difficulty meeting with the women to arrange for more recordings. Also, looking back, I realized that despite my many months in the community we had only collected a few hours of recordings. The translations were proceeding even slower for lack of time, money and available translators. Rose Gregoire, however, assured me that the women were interested in the work, but it would take time. I needed to be patient, she said, and not assume the work would be done in the next couple of years.

My feelings were mixed. It was good to know the women were interested but it also meant I was making a commitment to a project that would last many years. Not only patience, but a continued diplomacy was required on my part as I struggled to be sensitive to community values and people's needs while carrying out the work.

This was the challenge I had to accept if I was to continue living and working in Sheshatshit. I feared stepping on toes and arousing disapproval by imposing my agenda, my values and my time schedule on the work.

As the only outside researcher on the project, I often have to base decisions about the implementation of the work on my own values and guess as to what should be done. Women in the community act as participants and supporters of the work, but it is my responsibility to maintain the momentum of the work, organize the material and arrange for further meetings and translations.

Part of the difficulty of our work, I believe, is the project's informality. This approach seems to be necessary, however, because of the heavy demands on women's time and my own hesitancy to take control. My situation is not unlike that of the co-ordinator for the Fairbanks project who feared assuming too much control over the work. She said, "I've been torn between being committed to a process and also ensuring a good product" (St. Denis, 1989, p. 85).

Because I am committed to a participatory model and trying desperately to avoid the mistakes I made the first summer in Sheshatshit, I am taking direction from the women in the community. I suggest meetings to record material and follow up on them when they are ready. Fortunately, we have no funders to appease with a finished product by a particular deadline. In this way the work truly is community-based.

Our plans are to eventually have the stories published for distribution across Canada and for use in Innu classrooms. Any profits from the work will go to the Sheshatshit Women's Group. For myself, I will benefit from not only the experience but also the credit as editor of the book.

We have not yet determined the extent to which the work will be edited. Ideally, I will work with several community members to do this work. Initially I had tended to leave the translated material as close to the original as possible. However, after reviewing the life histories of several Aboriginal peoples, I concluded that to make the work accessible to readers outside the community, extensive editing would be necessary (Kurelek, in press). Final decisions, however, rest with the women involved.

Our life history project will not be the first of its kind. Several scholars (Silman, 1987; Cruikshank, 1990; Ahenakew and Wolfart, 1992) have worked with Aboriginal women to document their stories for public consumption and for their grandchildren to read. This will, however, be the first English production⁹ of Innu women's life histories, something to which both I and the women involved look forward to.

Our more recent work in Sheshatshit, despite some difficulties, has met with more success than our work the first summer. Certainly the process is more participatory.

Maguire found her work of limited success. Our work could be described in a more positive light — particularly our life history work. The extent, then, to which participatory research succeeds or fails does not always depend on whether or not the work originates within the community. Nor does the amount of involvement outsiders have in the project determine its success.

In the case of Big Trout Lake (Jackson and McKay, 1982), a participatory research project initiated and managed by community members, the residents failed to achieve their goal of an acceptable sewage system. They did, however, feel empowered by the process. The project's failure stemmed from a failure on the part of the Canadian government to meet their demands.

Brown and Tandon (1983) mention that one difficulty with doing participatory research is that it is done in opposition to power-holding elites. This situation can lead to a lack of support for projects that require funding. Schensul (1987) reports a similar finding. With collaborative research done by applied anthropologists who work with and for a target community there is a lack of financial backing and institutional support. Even Ryan (Ryan and Robinson, 1990), an outside "expert" affiliated with the Arctic Institute of North America, reported difficulties with funding

agencies for her participatory research in Fort McPherson, North-West Territories.

In my own work with the Innu people, only one of my five visits to date was fully funded by outside sources. For my first summer in Sheshatshit (1990), except for a few hundred dollars from Carleton University and the Canadian Research Institute for the Advancement of Women, I used money saved from my personal earnings. My second summer with the Innu, I received very generous funding from the Northern Science Training Program (NSTP). The third summer I received partial funding from the NSTP. This lack of support has benefits in that we owe allegiance to no one but ourselves. It does, however, limit some of our endeavours such as the translation of our life history material.

The lack of financial support for our work also means that my visits to the community are governed by my ability to save enough to cover my transportation and living expenses, while in Sheshatshit, by doing wage work at home beforehand. Although it may be possible for me to acquire employment in Labrador in order to facilitate the continuation of our life history project, I also have commitments in Ottawa.

This last point illustrates the importance of participatory research that is community-run. I have no doubts that the women of Sheshatshit would like to see our book published; however, they have neither the time nor the energy to carry out such a project on their own. They are happy to have me organize the process and arrange for interviews and translation while they provide the stories and the enthusiasm.

Is our life history work participatory research? Yes, I believe so, assuming there are two main components to participatory research: first, consciousness raising and second, control and input over the process. We need, however, to expand our understanding of consciousness-raising work.¹⁰

It is clear that the Innu people are oppressed in many ways. On a daily basis, and particularly when they are not in the country, they experience racism directly from non-Innu people in the area, and indirectly (in the form of colonial policies) from those governing their lives in Newfoundland and Ottawa. Classism in the form of poverty is extreme, most vividly illustrated by the overcrowding and the inadequate water and sewage system in the community. They realize their oppression and fight against it, as evidenced by their protests and their approach to land claims (Ashini, 1989).

In addition to the exploitation of their land and the lack of self-determination granted them by the Canadian government, there is also a form of oppression that Witham (1982) calls "cultural domination." He

speaks about Native people, in general, being forced to view themselves and their culture as inferior to the dominant society. Certainly the Innu people are proud of their life in the country, but in the community life is difficult.

Among the Innu there is a history of Euro-Canadian formal education in which Innu history and culture was not included or valued (Naskapi-Montagnais-Innu Association, 1986). Also, the Roman Catholic church devalued the Innu culture by condemning life in the bush and the spirituality that accompanied it (Armitage, 1990). Apart from institutional oppression, Euro-Canadian culture and values still permeate community life via television, via the town of Happy Valley – Goose Bay (thirty kilometres from Sheshatshit), and even via the presence of White teachers in the schools.

The life history book that we are creating will help to counteract these influences on young Innu people today. Our work will complement the changes that the Innu are implementing in their school system. For instance, in the last two years teachers' aides, who are Innu, have been taking students out to the country for days at a time. The Innu have also talked about restructuring the school year to accommodate families who take their children out of school each spring and fall in order to return to the country. As mentioned, one of the motivations for our work was to produce a book for Innu youth to read in the schools.

This decision to produce work for Native youth is not uncommon. In my review of several life histories published in the last five years (Kurelek, in press), I discovered that many Native women were telling their stories for their grandchildren "who sometimes seem to live in another world entirely" (Ahenakew and Wolfart, 1992, p. i; see also Cruikshank, 1990; Meili, 1991). One of the storytellers in Cruikshank's (1990, p. 16) collection explained the importance of getting her story in writing for her great-granddaughter because "Pretty soon paper's going to talk to her!" Oral traditions are no longer enough if their history is to be passed on.

Although our work will not bring about "fundamental structural transformation," as Hall (1981, p. 7) suggests participatory research should, I suggest it will help "improve the lives of those involved." Not only will the book benefit people in the community, but the women enjoy doing the work.

Although the idea for the life history work did not originate in the community, as Hall suggests it should (Tandon, 1981, says otherwise), the women do have control over the process. It is a process that helps strengthen in people an awareness "of their own abilities and resources" (Hall 1981, p. 8). The women control the process; the work proceeds as

they want it to with a facilitator, myself, who is committed to their concerns. The work is also participatory in that they take part in each stage of the process (Tandon, 1981). Even the educational component of participatory research is fulfilled as we learn about each other's lives and how to produce a life history text. In the future, we will also learn about the game of getting funding for our work and the process of acquiring a publisher.

Finally, as Ryan and Robinson (1990) suggest, our work is participatory research because in the end I will be done out of a job. I will, however, never relinquish my relations with people in the community — these bonds will only grow stronger as our work proceeds.

Conclusion

I would like to conclude this paper with a consideration of what the future of participatory research holds, for myself and other researchers.

There is no denying that today we have responsibilities to those we study and, as mentioned earlier, many of the people we study are insisting on a say in how our work is done and how the results are used. Levin (1991) notes that the days are gone when those we studied did not read our work. Hence, he says, today we need to acknowledge that our informants have rights and, often, expectations different than our own. The ideals of scholarship must be balanced with the needs of the people.

In trying to do participatory research, several times I encountered the difficulties in balancing the two. Trying to do participatory research while at the same time obtaining material with which to produce several papers and a thesis is a challenge. My pending dilemma stems from my need to structure the work I do with the Innu into a workable Ph.D. dissertation topic. To date, I have suggested various ideas to community members, each of which has been negated in one way or another. Rose Gregoire has no difficulty with my using our life history work as the basis for a dissertation. However, I need an angle of analysis that would be acceptable to both the Innu and academia. Rather than changing my area of study, I feel it best to combine my work with the Innu and my Ph.D. work firstly because of time constraints — I now have years of experience with the Innu that are important building blocks for participatory research. Secondly, I enjoy my work with the Innu and have a commitment to our life history project.

This does lead me back, however, to my advisor's claim that I am exploiting the Innu by using my experiences with them to advance in the world of academia. It is more accurate to view our work as a mutual exchange of services. I do work for and with the community in exchange for material to discuss in published papers. I also believe that the type of

work I do with the Innu will continue to grow in popularity as Native victories in their struggle for self-determination are realized.

I now realize that, despite the admirable principles of participatory research, it is unrealistic to expect researchers to do their work without some form of remuneration. Small (1988) doubts that participatory research will ever be popular with academics. She sees it as incongruous with academic objectives. Intellectuals are interested in "more patently scholarly work" (1988, p. 93), and lack the time and energy needed to do such work.

For myself, I have a sense that as I advance in life I may find it more difficult to adhere to truly participatory principles, particularly when doing work that focuses on consciousness raising to overcome one's oppressors — one of the primary tenets of this approach. The simple fact that this type of work receives little financial support will force researchers trying to support a family to seek alternative and/or additional projects. The idea of working *with* rather than *on* people is something I can continue throughout my career, thanks to my work with the Innu people and my study of participatory research.

Small (1988, pp. 93-94) also mentions that the demands participatory research makes on our time will conflict with the requirements of academia. As well, we must contend with a colonial mentality that interferes with us working closely with community members, a position that requires us to forgo the positions of privilege bestowed on us as members of a conservative institution. Radical work, she reminds us, has received little support in the past.¹¹

Small (1988) tells us that participatory research among the Maori is successful because of their long history of making do with minimal outside support and their tendency to share resources. However, the cornerstone to this methodological approach, and the one that makes their work successful, is their ability to identify their problems.

Clearly, the Innu people have identified their problems. What I would like to posit here is that the way in which the Innu are using participatory research is not so much to raise their own consciousness as to raise that of non-Innu people. Considering the grand scheme of things, it is possible to see everything I have done with the Innu as part of a larger community agenda to empower themselves by gaining additional public support for their life in the country.

By them sending me to the country to witness the benefits of this lifestyle, the Innu know I can act as an ambassador to the outside world via my oral and written presentations on their experiences. Our exploration of the problems in the community as opposed to in the country, during my

first summer with the Innu, also helped to clarify the difficulties they experience under a colonial regime. Lastly, our life history work will not only assist Innu youth in affirming a pride and belief in their culture, but the work will also serve to educate people across Canada about the Innu way of life. In this way, then, the research did originate in the community, since it was the people of Sheshatshit who decided to use the power of knowledge to educate the world and to rally support for their cause — my work with them is one of their tools.¹²

One final issue I would like to address in regards to anthropological participatory research, and in particular my work with the Innu, is that of whom we choose to work with. Traditionally, anthropologists have worked with Indigenous peoples. By doing participatory research with the Innu I work against the Settlers of Northwest River, whose land-claim competes with that of the Innu (Plaice, 1990), and against the residents of Happy Valley-Goose Bay, who supported the building of a NATO base in their town. Suffering the effects of Canadian regionalism, these people also have the right to fight for a better life. Yet by working with the Innu I choose not to support their causes.

My initial decision to work only with the Innu stemmed from a realization that militarism was affecting Innu women “in my own backyard.” As I got to know them, however, I acquired a respect for Innu culture and their struggle to survive against great odds. The enthusiasm with which the Innu support their culture not only inspires me to work with them, but also facilitates fruitful participatory research work.

Notes

- 1 While Chambers lists action anthropology as a subtype of applied anthropology, Tax (cited in Ryan and Robinson, 1990) perceives the two as different. For Tax, action anthropology benefits, specifically, those in the community involved.
- 2 I wish to acknowledge the research of applied anthropologists working to assist Native people with their land claims. Government agencies requiring this work insist on research that appears positivistic. Several anthropologists throughout Canada are meeting this demand for rigorous work to verify Indigenous use of the land.
- 3 Several Innu men and Bob Bartell (Sheshatshit's former Mennonite worker) spoke at St. Paul's University, Ottawa, in 1990.
- 4 The Canadian Security Intelligence Service (CSIS) had investigated the Innu because of their protests against NATO (Lee, 1990; Anon., 1989).
- 5 My thesis advisor pointed out to me that even my attempt to avoid exploiting the Innu for data by writing a thesis on methodology was unsuccessful, since I used my work with the Innu as the basis for this text.
- 6 See Brown and Tandon (1983) for an excellent review of the differences between participatory research and action research.

- 7 I did, however, pay Penashue and Riche to translate the material from Innu-eimun into English so that I might summarize the findings in a written report.
- 8 Elders in the community do not speak English.
- 9 José Mailhot is currently working on Innu life histories in French.
- 10 The following interpretation of Innu consciousness and participatory research, while it is my understanding, has been confirmed by Jack Penashue.
- 11 Jacobs (1974) found her action-advocate approach to work in a Black community in the United States required her to forgo her position in the university for a few years.
- 12 Discussing this point with Jack Penashue, he reminded me of the response his grandfather gave when I asked him during our interviews if there was anything he wanted me to do. His reply was that I should tell Canadians and the government "that we're here" and about the Innu way of life.

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