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"When Are You Leaving?"

**Search for an Appropriate Research Methodology
for Work with Aboriginal Peoples.**

by

Cathy Kurelek, B.Sc.

**A thesis submitted to
the Faculty of Graduate Studies and Research
in partial fulfilment of
the requirements for the degree of**

Master of Arts

Department of Sociology and Anthropology

Carleton University

Ottawa, Ontario

August, 1992

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and Research acceptance of the thesis

"WHEN ARE YOU LEAVING?"

SEARCH FOR AN APPROPRIATE RESEARCH METHODOLOGY
FOR WORK WITH ABORIGINAL PEOPLES.

submitted by Cathy Kurelek
in partial fulfilment of the requirements for
the degree of Master of Arts

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September 23, 1992

ABSTRACT

This thesis addresses the issue of methodology in work with an aboriginal people. The criteria necessary for responsible work **with** people are suggested by critiquing anthropological and alternative approaches to research.

The work focuses on traditional anthropological research, participatory research and feminist methodologies both from the perspective of what they have to offer native communities and what difficulties they promote.

Each chapter ends with an account of the author's experiences with the Innu people in Labrador. Efforts were made to apply each of the methodologies discussed in order to empower those involved in the research process.

The motivation behind her work was to work **with** the Innu rather than **on** them. Both the difficulties encountered and the successes met when applying this approach are described to guide future researchers toward more appropriate methodologies.

ACKNOWLEDGEMENTS

This work is for myself and the work I have done with myself.

However, I offer my thanks to several people for their support during the long process of 'working on my thesis': Janet Gregoire for sharing her room and her experiences with me, my roommates and friends in Ottawa for assuming I could do the work, and to my friends in Toronto and Sheshatshit who told me about their pain and showed me their strength as I expressed mine.

I dedicate this thesis to four people. First to Rose Gregoire whose strength, hospitality, and patience allowed me to enter her community. Second to Jack Penashue for his invaluable friendship and laughter. To my father, from whom I inherited a desire to travel and a belief that the world needs to change and I have a role to play in that change. And for my mother, who gave me the greatest gift of all - an interest in people.

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Chapter 1

INTRODUCTION: Methodology and the Innu

The Research Questions

This thesis studies the methodologies which guide researchers working with Aboriginal peoples. In an era in which the 'why' and the 'how' of the research process are being questioned, it is important to understand the methodologies we employ. Increasingly, due to budget cuts and native self-determination, there is a move away from the production of knowledge for knowledge sake towards a more responsive approach to research. But for whom will the research be done? And, how will it be done? These questions, and others, will be considered in my thesis, which means to explore possible alternative methodologies in social science research. Can our work become more responsible to those we seek to understand?

The following is a study of the assumptions behind our choice of theory and method when studying 'others'. I will be examining participant observation, participatory research and feminist methodologies in terms of their effect on those studied.

In this thesis, I will define methodology as Sandra Harding (1987) does, "a theory and analysis of how research

should proceed" (2). Research had traditionally been defined as the "endeavour to discover new or collate old facts by scientific study of a subject" (Personal copy of Concise Oxford Dictionary). According to a student's guide to research, appropriate research follows a cycle (Northey and Tepperman, 1986). One begins with a theory to deduce an hypothesis which is operationalized in the form of observations. Generalizations are then extracted from the observations to formulate laws and the cycle is repeated.

The above definitions do not apply to research as I perceive it. This positivistic approach to research, I will argue, should be avoided as it fails to address the rights and the needs of those the research focuses on.

For me research involves the collecting of information and its collation into an accessible form. For instance, a life history may be collected and published without an accompanying analysis. Any interpretation is left up to the speaker and the reader.

Research, if it is community based, can also include analysis by those studied. I will argue they are in a better position to understand their lives than an outside 'expert'. The steps of positivistic research only serve to alienate the research process from the people we work with.

What follows is a study of how methods used to study others affect the researcher and the researched. Who benefits from the research process? Who controls the

process? Do the people who are being studied have any say in what gets researched and how the work gets done or are they mere specimens under a microscope?

This paper assumes that mistakes have been made in our studies of people in other cultures. I want to make clear that research that harms, exploits, or uses people only to advance academic agendas is inappropriate. What appropriate research involves and how it can be done will be explored by looking at the history of anthropology and more recent research alternatives.

To address these questions we also need to consider why we do research and who we do it for. We must understand why appropriate research is important and what motivates inappropriate research. I believe that appropriate research seeks to be feminist and non-racist. Furthermore, those who are the target of a study must have direct control over the research process and its results. This brings us to the question, 'who should be doing the research?'. Should it be done by the people themselves; thereby putting social scientists out of a job? Whoever does the research, they must see that the work done is relevant to the community studied. **What** gets researched is all important and it is only when community members are involved in this decision that appropriate research is possible.

I will consider each of these questions, first by reviewing the literature on traditional and alternative

research methodologies and second by discussing my work with the Innu people of Nitassinan.

The Audience and the Aim

My work is written with a particular audience in mind. Since my purpose is to explore methodologies to arrive at a better way of doing research, I have directed my work at new scholars. In particular, I speak to young female academics who are embarking on a journey similar to the one I have taken for the past three years. I started out as a positivist with feminist tendencies and humanitarian concerns and ended up an advocate of participatory research. I would like my work to be read to save others time, misunderstandings, and confusion--particularly students doing research for the first time. My hope is that new students, as they learn these more participatory approaches to research, will change the entire way in which research is done.

I want women coming after me to be able to avoid the mistakes that I made; I assumed it was enough to do research on women, without taking their needs into account. It is also important to avoid the pitfalls I encountered when applying participatory and feminist methodologies. I see it as the responsibility of white scholars to educate each other on racism. I believe that my research can contribute to this sort of education.

It is now accepted by some that research agendas should come from within communities; at minimum there should be community support for the projects proposed. Needs assessment projects are an example of a more practical research agenda. The objectives and the methods employed in needs assessment projects, however, are often still determined by non-native outsiders. Because the values and agendas of the researchers differ from those within the community, it is not uncommon for the resulting work to be inappropriate.

My thesis explains why it is important to be conscious of those we work with and how to work **with** people rather than **on** them. The reason I address my work to female students is my assumption that many male scholars are uninterested in doing work with women. However, I would be pleased if men read my work, especially those willing to deviate from typical male research patterns and willing to grow in their understanding of what research should involve.

Finally, I have produced this work for those who are the objects of anthropological studies. In particular, I have written my thesis for the Innu people so that they can understand what exactly it is that anthropologists do when they come to their community for months at a time, take notes and then leave to write about them. Basically I want the Innu to see 'what the hell we're doing in the

university'.

I explain the research process and alternatives to it so that the Innu can demand changes from future researchers. I also discuss feminism for them to understand what it has to offer their community and its limitations when applied to their lives.

Aside from the academic agendas of my thesis there was also personal motivations for my work. My thesis and my work with the Innu have been part of a quest to understand social science research and in particular anthropology. This thesis is a self-reflective journey to discover whether a career in anthropology is what I want. To some degree, the thesis has arisen from my privilege as a white middle-class graduate student who has the leisure to dally in philosophical questions about research methodology. I explore my disillusionment with traditional anthropology, my hopes and reservations about participatory research, and my feminist principles in flux. My hope is, however, that this work will enable me, and perhaps others as well, to do more responsible research in the future.

Chapter Outline

Following this chapter is chapter two which includes a description of the way in which traditional anthropology used people to further 'the study of man'. Included will be a critique of anthropological theory and methods in terms of

how they effect those studied. I will show that most often the motivation for this work has been academic interest and colonial pursuits. I will condemn the discipline's efforts to understand 'mankind' and to appear 'scientific'. How anthropologists seek to produce 'accurate' accounts of other societies will be considered with a particular focus of participant observation as it effects both the researched and the researcher. This chapter will examine the ineffectual way in which anthropologists responded to postivistic studies of 'the other' by doing reflective work. Personalized accounts of field relations failed to take into account the needs of those studied.

This chapter will end with a discussion of my plans for doing postivistic research, which followed from my anthropological training. I describe my history as a white academic and hence the possibility of my participating in the status quo. Also hinted at in this chapter, however, is my inability to accept standard claims to 'truth' because of my gender and because of critiques within the discipline. This discussion shows the reader why I chose to do the research I did.

Chapter Three begins with an examination of native critiques of anthropological research. How can an outsider understand another culture and why does our work continually fail to address the needs of the communities studied? The chapter continues with a discussion of power; how

researchers can make efforts to share power with the researched and try to produce 'useful' work. Participatory research is offered as a solution to native critiques of anthropology; a process that involves those studied in the research design, implementation, analysis and data use. This approach claims to empower those studied yet it is based on the ethnocentric principles of 'education' and depending on how 'community based' the work is, can be irrelevant.

This chapter, as well, ends with an account of my work with the Innu; in particular my struggle to do participatory research with the Innu. The reader will note an absence of goals and focus in my work with the Innu as I attempted to win their approval. The work shows how we, as researchers, do not realize how much power and privilege we hold over those we study.

The last main chapter, four, is an exploration of feminism and research. Traditional research is critiqued from a feminist perspective. I begin by illustrating how early anthropological accounts ignored women and later studies added women onto models for men. As feminism gained more hold in the social sciences, critiques of positivistic methods appeared and with them research alternatives. Following these feminist critiques there came critiques of feminism by women of colour. These are important contributions for our understanding of what needs to be

included and avoided if research is to be appropriate. In particular, research must not separate the issues of sexism and racism. The work of the scholars selected show us how an anti-men approach to research is inappropriate for research on women from oppressed groups.

My attempt to do feminist research with the Innu is also explored. I use the word attempt as my plans to work only with women were squashed my first summer with them. Later I was able to return to this agenda but in the meantime I learnt that Innu women's issues are community issues. What the reader may also note is the power and privilege I enjoyed as an academic allowing me to study only women, and in a way I found interesting. Innu women are more concerned with the survival of their community and families, for instance getting adequate living conditions, than with being involved in 'feminist' research.

The concluding chapter, number five, will answer the questions asked in this chapter and discussed in chapters two, three, and four. I will describe what I consider appropriate research and how it can be done, why it is needed. I will illustrate the benefit to this type of research by relating these issues to my work with the Innu.

Why did I focus on the work I did, not on more 'serious' social ills: why not avoid altogether work with native people? This will be explained. Quite simply I will show that change can occur; we should not 'give up' and

accept the world as it is. We can make life more acceptable for ourselves and those we interact with. I have chosen to do this through a thesis that shows how to avoid the mistakes I made and points to methods for less exploitative research. I also feel I am doing my part to move toward a better future for myself, the Innu and the rest of society by working with people in Sheshatshit.

I will briefly discuss how my focus on qualitative research does not preclude the use of other forms of work providing that the research principles outlined above are not overlooked. The most important item to consider is the amount of control those in the community have over the work.

The last section of the conclusion will discuss my plans for work with the Innu and my ideas for further studies. It is particularly important that other scholars not only work cooperatively with aboriginal people but also share their work with their colleagues so that more appropriate methodologies can be arrived at.

I will end my thesis with concluding remarks on my title: "When Are You Leaving?" During my first summer with the Innu I would meet people in the community and after asking me where I was from and where I was staying they invariably asked me, "when are you leaving?". Each time they asked me I was hurt and offended; I felt that they were inferring I did not belong and they wanted me to leave. Within time I came to realize that it may have been a

question of semantics that prevented them from realizing it would have been more polite (but perhaps not more appropriate) to have asked me 'how long are you staying?'

As it was stated their question emphasized my status as an outsider. I was a white academic coming to their community for data; whether data on their culture or on appropriate methodologies. No matter how 'politically correct' I tried to be, no matter how polite or sensitive, I was from a culture that historically had dominated, denigrated, exploited and oppressed their culture for centuries. I was also part of an academic elite that made a living out of recording their suffering. I doubted that I had a right to be there and so tried harder to 'do the right thing' by not imposing on people, and by helping out.

The Innu

Who are the Innu people? The Innu are **not** Inuit. They are an aboriginal First Nation. They are Indians, not Eskimos. Even my brother believed for months that I was living with the Inuit. I was not.

Innu, which means human being, is the word that the people I work and live with give to themselves. To scholars the Innu were known as the Naskapi-Montagnais. This term, however, is no longer used because 1) it creates a false division (Naskapi & Montagnais) within one people, 2) Naskapi has pejorative connotations, and 3) Naskapi-

Montagnais is a colonizers' term (Armitage, 1990).

There are approximately 10,000 Innu people, living on the Labrador-Quebec peninsula. They share a common language, Innu-eimun (a dialect of Cree), and are related by kinship. Most of the Innu live on the Quebec northshore. Their second language is French. The Innu in Labrador communicate with their friends and relatives in Quebec in their native tongue as their second language is English.

The Canadian state has divided the Innu nation politically and divided families economically. The Innu in Quebec are status Indians and they live on reserves. The Innu in Labrador are not. Nevertheless, people continue to move back and forth between the villages in Quebec and in Labrador. Congregations for social events and intermarriage between people in the two provinces is prevalent.

I worked in the Innu community of Sheshatshit, Labrador. A village of approximately 1,000 people, Sheshatshit sits on the south side of the North West River. The North West River is west of Hamilton Inlet, which is inland from the Atlantic Ocean. At Sheshatshit the water turns from salt to fresh. Several years ago the Innu camped on the north side of the river which now houses the Settler community of North West River. Northwest River started out as a trading post when trappers came from Britain to work in

the fur trade¹. The Innu, because they were following the caribou herds, were considered an unreliable source of furs so seasonal trading and then settlement was encouraged from overseas (Plaice, 1990).

For years before the Settlers came the Innu came to the region to meet with the missionaries, trade furs for supplies, and fish on the banks of the North West River. Innu grave stones from the turn of the century can be found in the village of North West River. However, I was told by several people in Sheshatshit that, eventually the Settlers forced the Innu to the south side of the river. But, it was not until 1957 that the first house was built in Sheshatshit, and not until 1965-1968 that significant settlement took place. Until then the Innu lived in tents moving over the region in pursuit of their livelihood (Armitage, 1990).

The Innu's mainstay is caribou; it is extremely important religiously, nutritionally, socially. Even today caribou meat is highly valued while the skin is used for moccasins and snowshoe lacing. When caribou is shot a feast (makushan) follows to thank the animal spirits for giving themselves to the hunters and their families.

The Innu continue to spend three months in the fall and

¹ Some settled and married Inuit women from the north thereby passing their 'Inuit status' onto their descendants. Hence people in North West River now are part of the Labrador Inuit Association (LIA) which is negotiating with the government for land rights that compete with those of the Innu people.

three months in the spring in outlying camps. It is also not uncommon to set up a tent and spent days away from the village in the summer and winter months.

Conditions in the village are not unlike reserves in Canada. Housing is provided by the Band Council and living conditions are rough as a result of 1) limited state support, 2) high unemployment rates and, 3) the legacy of colonialism and the racism that has accompanied it. However, unlike reservations funding for Innu health care, education, and administration is shared between the province and the federal government. Once land claim negotiations are completed (perhaps ten years from now) funding will be entirely federal.

The Innu are the original inhabitants of the area. Their name for the land they live and hunt on is Nitassinan, which means our land. The Innu do not consider themselves Canadians²; they have a fierce sense of their own identity and culture as Innu people. Although they pay taxes to the Canadian government and receive transfer payments from the province they are the Innu of Nitassinan. Recently they have been forced into land claim negotiations with the Canadian state as development schemes threaten them and their land from all directions. They will, however, leave the negotiating table if the government demand that they 'extinguish' their land rights to the crown (Innu News,

² Personal communication with several community members.

1991).

Development schemes threatening their land are not new to the Innu. As earlier as the 1920's Settlers and trappers in the regions were disrupting Innu hunting to such an extent that starvation was occurring. In the 1940's, when a military base was build to the southwest (now Goose Bay) more white people came to the area. Around this time that the Mealy Mountain caribou herd (to the south of Sheshatshit) was overhunted to the point of extinction by Goose Bay residents.

Overhunting in the region combined with actions by the Roman Catholic church to force the Innu to settle in the late 1950's. The priest at the time, Joseph Pirson, forced children to stay out of the country camps and attend school by threatening their parents with the discontinuation of their transfer payments. If they were to avoid starvation, the Innu had to rely on state money to supplement their bush food because of the overhunting of their land (Armitage, 1990).

Today, across from the Innu village of Sheshatshit is the Settler community of North West River; connected by a bridge that was built in 1979. Fifty kilometres to the southwest, separated by a paved road, is the town of Happy Valley-Goose Bay which is composed of white people from Newfoundland and elsewhere who came to the region in the 1940's, and some more recently, to work.

In the early 1980's plans began to revive the area by expanding the air-force base (used for refuelling during World War II) to allow NATO allies to do low-level flight testing. The Americans, the Germans, the Dutch, the British, and the Belgians all moved in and began flying. Although it meant more work and infrastructure for the residents of Happy Valley-Goose Bay, the Innu were upset.

During their flight testing the jets were not flying over uninhabited land, as the base claimed. They were screeching, faster than the speed of sound and as low as 100 feet, over Innu camps (Fouillard, 1989). People were terrified as jets came out of nowhere and caused sonic booms.³ Wild-life and vegetation was damaged and health problems, typical of stress reactions, appeared among the Innu and their land. Bombing ranges were set up in Innu hunting territories, flights all the while military personnel denying there was a problem (Armitage, 1989).

Then in 1987 the Innu decided they would no longer sit quietly while their land was destroyed and they began their now famous protests. The Innu went onto the runways to stop the jets from taking off and they went onto the bombing ranges to halt the destruction of their culture. In the

³ It was the sonic booms that forced huge public outcry in Britain and Germany that eventually had the flying banned overseas. Hence the allies need the 'uninhabited' land of Nitassinan to flight test in preparation for flying in the Soviet Union. In the recent war on Iraq the jets and pilots used were trained in Nitassinan.

villages there were high rates of poverty, violence and alcoholism that disappeared when they went to their camps in the country.

The Innu were arrested and fined for their protests but the media world-wide had picked up on their cause. Over night they became celebrities. They were asked to speak in the centres of Canada, Europe and even as far away as Japan. For people listening, the Innu came to symbolize a fight for the environment, for peace, and against imperialism.

Outsiders came to the community to lend their support and gather information. The Innu had rallied so much support for their cause that their resistance was putting pressure on the Canadian government to halt the building of a NATO base at Goose Bay.⁴ So CSIS (the Canadian Security Intelligence Service) was called in to investigate the Innu and their activities. CSIS failed to understand that the Innu were fighting for their lives and instead wanted to consider how 'foreign agents' were stirring up unrest (among the natives) and instigating anti-NATO activities (Lee, 1990; Secret Document, 1989).

It was around this time that I learnt about the Innu people. I made contact with them just at the height of their protests and was given permission to come out just before the courts ruled against a NATO base being built at

⁴ The base would have meant an increase in development to the region, more low-level flying, more bombing ranges and a host of other horrors.

Goose Bay. The Innu were able to stop the base from being built. However, the low-level flying continues.

It was these famous people that I eventually decided to work with; a classic case for an aboriginal people who still lived off the land. What more could I ask for? As I will explain at the end of the next chapter, my initial agenda was to exploit the Innu for data.

Chapter 2

STUDYING 'THE OTHER': Methodology and Anthropology

Introduction

Historically, anthropologists have studied 'exotic' people to expand our understanding of them. The ways in which these people have been studied ('methods') and the values and goals ('theory') that inform this work have changed somewhat over time. Nevertheless, the basic premise of this work remains, that anthropologists should study others to better understand them.

But does such a premise benefit those studied? In the first half of this chapter I will address this question. I will explore the methods, values and goals of the first anthropologists in light of the effect of their work on their subjects. This will be followed by a look at the rise of participant observation and the values that accompanied it. As well, I will critique the empirical approach to 'the study of man', practised in the 1960's. Lastly, I will show how the entry of reflexivity into the discipline, although it appeared revolutionary at the time, continued to ignore the interests of those who were studied.

In the second half of the chapter I will outline my initial interest in doing traditional anthropology and how

it affected my work with the Innu. I will end with a discussion of my venture into reflexive anthropology.

Early Anthropologists

Many of the first anthropologists studied other cultures - 'primitives' - as though this were a hobby rather than as a career (Langness, 198 :28). They were well-to-do gentlemen with a fascination for the 'exctic'. Rather than experiencing the culture they studied on a first hand basis, these scholars relied on men who were visiting 'the dark continent' to provide them with data. Having never met the people whose culture they were studying, these armchair anthropologists⁵ used second-hand information to formulate theories about the evolution of man.

Evolutionary theories have had a devastating effect on those not at the top of their evolutionary scales, an effect that survives even today.⁶ Societies of the world were ranked according to their similarity to western 'civilization'. The hunter and gatherers of North America and Africa were at the bottom of the scale as they appeared to lack sophisticated political, economic, and social

⁵ So called because of their ability to do their work from the comfort of their homes, without ever needing to go to the field.

⁶ Many people in the western world continue to believe that Africans and Aboriginal peoples are not as advanced as other peoples. Racism, the assumption that some races are inferior to others, informs much development policy and the Indian Act by assuming that its 'wards need assistance to develop'.

systems. In actuality it was their technology, values, and physical features dissimilar to the Europeans that placed them in this position. Evolutionary theorists also stated that these peoples would eventually evolve to the level of humanity occupied by industrial Europe.

In addition to dallying in a hobby that explored exotic peoples, anthropologists were interested in understanding themselves. It was believed that the study of 'primitives' would give insight into the origins of western man. This was the notion of our 'primitive contemporaries' still found in some anthropology textbooks.

By illustrating how matrilineality was inferior to partilineality, how capitalism arose from barter, and how animism was more 'primitive' than monotheism, the work of early anthropologists supported the 'manifest destiny' of the time. Clearly if these other societies were inferior they needed the guidance of those more advanced. This paradigm helped justify the enslavement of people of colour and the pillage of their lands.

The next generation of anthropologists had similar motives, upholding the status quo, but their methods differed. Bronislaw Malinowski developed the technique of participant observation when he was stranded in the Trobriand Islands during World War I. After his experience collecting cultural data, on a first-hand basis, he propagated the idea that this was the only way to truly

understand a culture. Data received second-hand by non-professionals, in his view was simply insufficient.

There were two reasons for participating in the activities of those one was studying. Firstly, the anthropologist gained a better understanding of the culture under study if he did more than observe. Second, by participating the anthropologist supposedly minimized his effect on those he was studying (Miller & Wilson, 1983).

Despite a movement from the armchair to the field, a distance continued to exist between the anthropologist and his⁷ subjects. There was always a psychological distance. Those early anthropologists preferred 'more civilized' company and considered those they were studying 'primitives' and 'savages'. In Malinowski's diary (1967), for example we see his preference for European accommodation and his use of terms like 'brutes' and 'niggers' to describe those he was studying.

Their motivation for study was neither benevolent nor revolutionary. Some anthropologists funded their research by providing information to colonial administrators to 'aid in the effective administration of the colonists' (Kuper, 1973). Most perceived their goal to be the understanding of 'primitive' culture.

This work, like earlier work, was meant to further our

⁷ I use the pronoun 'his' as the vast majority of anthropologists, until the last decade, were men.

understanding of all humanity. For instance, Claude Levi-Strauss was interested in the symbolic systems of mankind and used his work in South America to explore this topic. Other cultural theories were developed to understand the institutions, such as kinship, religion and political systems of 'exotic' peoples. Some anthropologists studied other cultures to better understand their own. Margaret Mead used her work with the Samoans to address issues of conflict among adolescent Americans (Bourguignon, 1988). Nevertheless, this was not just a matter of knowledge for its own sake. Both academia and governments might profit by the extraction of data from anthropologists' subjects. In many cases the information gathered remained within the walls of academia, benefitting no-one but the anthropologists whose careers were advanced by this work. In other instances the data collected by anthropologists were used against those studied. Colonial governments hired anthropologists to gather information in the colonies in order to more effectively control their 'subjects'.

Positivism in Anthropology

The 1950's and 1960's saw an increase in scientific rigour, a desire to arrive at 'the truth' about the cultures that anthropologists studied. As faith in the omnipotence of empirical methods grew in the hard sciences and the social sciences, anthropologists strove to align their

discipline with the social sciences, not the humanities. This led to their adopting methods and values from other social sciences. One of these values was objectivity. Unlike the survey approach to research in the other social sciences, with only brief contact between the researcher and the researched, the anthropologist had close and extended contact with those he studied. So in order to maintain objectivity in the field, anthropologists believed that a psychological distance between themselves and their subjects was essential. Also, to ensure the reliability of their results, anthropologists needed to check their work by applying various methods and by rechecking their data⁸.

Hence the repertoire of the anthropologist in the field expanded from participant observation to include formal interviews, surveys, questionnaires, and psychological tests. The discipline adopted a new language which included such telling phrases as: the need 'to maintain objectivity', 'control for observer bias', as well as 'carry out systematic research', and 'operationalize the project'. Researchers were encouraged to enter the field in order to disprove the null hypothesis (Edgerton & Langness, 1974). Pelto and Pelto (1978) state:

anthropologists need to develop research methods that protect the researchers from their own subjective assumptions and value judgements ... (and) methodological training can provide ways of

⁸ See Pelto and Pelto (1978) for an example of this philosophy.

minimalizing the researcher's personal biases by means of systematic, objectifiable research tools (36).

In this era it was an embarrassment for anthropology that different anthropologists were getting different results when studying the same society--there was no reliability, one of the requirements of positivistic research. Robert Redfield, for example, arrived at different conclusions than did Oscar Lewis who was studying the same people. Redfield was prophetic enough to realize that different researchers would have different findings but his contemporaries would not accept this until years later. Because the data anthropologists collected were questionable the theories based on this information were faulty.

Despite the call for scientific methods, the anthropologists of this era were not willing to sacrifice the technique which made their discipline unique--their close contact on a long-term basis with those studied. Text books of this era argued for an amalgamation of art and science in 'the study of man'. By living in the field and doing qualitative research the anthropologist was able to collect more information through observation and as a result of the rapport he would establish than a researcher making only a brief visit. However, being immersed in the field he would need to have to use multiple methods to get his findings and ensure he had accurate results.

The values of this era called for a focus on scientific rigour in one's methods and results to obtain an accurate

picture of other cultures. Text books made only passing reference to applied anthropology and viewed it as a way to help those studied. Instead, the authors argued for the right to study whatever they wanted with the possibility that it might produce positive results. Freedom in scientific study had produced some unexpected yet beneficial results in the past (Pelto & Pelto, 1978). As well they asked, if work was to be done to assist people, which people would it assist and who would dictate what was researched?

These arguments were put forth partially in response to criticisms from both within and outside of the discipline when anthropologists involved themselves with the CIA against the people of India, Peru, and Thailand (Edgerton & Langness, 1974:117). This show of disapproval was a radical change for a discipline that had a history of working to provide colonial regimes with information about their subjects.

However, for the most part anthropologists continued to do research that was of interest to them and a current topic within the discipline taking into account neither the interests of the state nor those they studied. Theory building and knowledge accumulation continued to be the primary motivation of anthropological work.

Reflexive Anthropology

The 1970's saw the rise of reflexive work in which the

anthropologist's 'objectivity' was neither necessary nor desirable. In the past the researcher had been invisible in the ethnography; he was only a machine that took in data, analyzed them and then presented them. But in the new ethnography the anthropologist became part of the subject matter.

Scholars began considering the effects that their presence in the field had on those they were studying and the effect that those they worked with had on the research. It became common for the anthropologist to look at himself looking at those he was studying and those who were studying him. The image of mirrors was used as a metaphor to describe this phenomenon in such works as A Crack in the Mirror (Ruby, 1982), Shattered Images (Cove, 1987) and "The Double Frame of Life History in the Work of Barbara Myerhoff" (Prell, 1989).

When anthropologists began writing about their experiences and feelings, their reflexive work took on a story-like character. Descriptions of the culture being studied appeared as a by-product, as background to the puzzle that the anthropologist set out to solve. Like earlier work, there continued to be a focus in arriving at the truth although by this time anthropologists were admitting that it was not a simple task. Reflexive scholars were interested in discovering 'the truth', not only about the culture they were studying but also, about themselves

and their own culture. What was mankind really like. In some cases a study of the self seemed to take precedent over a study of 'the other'⁹. Simon Ottenberg in "Thirty Years of Fieldnotes: Changing Relationship to the Text" (1990) described reflexivity as an anthropology of ourselves to avoid such unhappy issues as poverty (158). By focusing on ourselves and our relations with those we studied we did not have to address the larger and more significant power relations that oppressed people suffered under.

Like their predecessors, reflexive anthropologists continued to do their work for themselves and their discipline, not for those they were studying. The only reason that community members were consulted during the research process was to provide information to the anthropologist. Neither the needs nor concerns of these people were taken into account. Jean Briggs in Never in Anger (1970) mentions the trouble that she exposed her host family to yet she continued her work in the Arctic. Perhaps the epitome of this ego-centric approach to research is seen in Colin Turnbull's book The Mountain People (1972). In it Turnbull describes his problems with people who were starving to death!

In all of these approaches it is the anthropologist who

⁹ See for example Cesara's Reflections of a Woman Anthropologist, (1982).

is the expert. She¹⁰ chooses where she will study, assumes she will be allowed into the community and then proceeds with her research. It is the researcher who decides what will be studied, how it will be explored, how the data will be analyzed, and what will be done with the results. Only the anthropologist benefits from the work. As a result of her visit she publishes material that benefits her financially and advances her career (Patai, 1991).

A few people in the community studied may accrue short-term benefits if they are paid as informants or are able to sell food or accommodation to the anthropologist. It is also possible to argue that the anthropologist's work benefits the community studied as it publicizes the community's existence and possibly the difficulties it has to contend with. The difficulty is that the anthropologist does not consult with the people before the material is published. Hence, the material may be inaccurate, or worse, kept hidden from them. In Australia, for example, Aboriginal people were angered when the wife of a noted anthropologist released for public viewing scared objects, connected with men's ceremonies. These objects were dangerous for women to see. However, those involved had to resort to legal action to stop them from being shown.¹¹

¹⁰ I use 'she' as more women were entering the discipline, however, I could also have used 'he' as men still predominated.

¹¹ This story was relayed to me by Prof. David Turner at the University of Toronto.

The interests of the native people involved were not considered.

I adopted a similar approach to research during the early stages of my plans for research. I was more interested in making a significant contribution to the academy than in considering the needs of those I would work with.

My Traditional and Reflexive Anthropology

In 1989, at the beginning of my Master's degree program, I began looking for a thesis topic that would satisfy me and the university. Being a feminist, I wanted to focus on women and being an academic I wanted to do research on a subject that was novel and interesting. The topic I selected was prostitution. Although I could find little in the way of feminist material on the subject, I realized I needed to focus my work. After some preliminary research, I decided to study the link between prostitution and militarism in Canada.

I was interested in studying women who were 'different' - something unusual by the standards of most Canadians - and I wanted to link their oppression to the epitome of patriarchy: militarism. My intention was to avoid painting a picture of prostitutes as mere pawns of their pimps and customers. Instead, I intended to show how they exercised control over their lives but at the same time were

victimized by militarism.

I did preliminary work close to home to gain an understanding of the issues facing hookers by interviewing a youth worker and a police staff-sergeant, both of whom worked with prostitutes in Ottawa. I was hesitant to make direct contact with the hookers themselves. Frankly, I was intimidated.

In terms of my narrower focus on the link to militarism, I consulted with a scholar in my department who worked with the military and an engineering student who had been in the Canadian Armed Forces. Both of these men insisted that there was no link between prostitution and the military in Canada. I doubted their word but decided to shift my focus from Canada to Southeast Asia where there was a flourishing sex trade industry between American military personnel and Filipina women. Although prostitution and militarism was a 'hot feminist topic'¹², I could find little written on the subject. My academic advisor suggested doing fieldwork with prostitutes in the Philippines to overcome the lack of literature.

At about the same time I attended a presentation at St. Paul's University billed as an Innu family speaking about low-level flying. I had heard about the Innu people and their protests against the building of a NATO base in

¹² See "MATCH News--News about Women and Development" issue Jun\Jul\Aug 1989.

Labrador, from friends, so I decided to attend.

During this information session, presented by Innu men and a white Mennonite man, we were told not only about the low-level flying jets screaming over Innu camps but also about 'the prostitution' that was occurring between Innu 'girls' and military men in Goose Bay.

At this point I realized I did not have to look as far as the Philippines to examine the connection between prostitution and militarism. It was taking place in 'my own back yard'¹³. Once again my advisor suggested I follow up with fieldwork.

I do not think I would have pursued this suggestion had it not been for the encouragement I received from the instructor of a Native Studies course. It was one thing to consider doing research on people in Southeast Asia, who epitomized 'the other'. They were on the other side of the world, in an 'exotic' place, and only known to me as characterizations in written accounts of their lives. It was not difficult to objectify their lives and theorize about their struggles. I had never had contact with them so they appeared unreal.

This was not the situation with the Innu, I had had contact when I heard several of them speak at an information session. They were real people. But thoughts of my

¹³ Canada.

studying native¹⁴ people in Canada made me uneasy. As a white Canadian in a country that oppressed and exploited native people I felt a collective guilt and was super-sensitive of the need to do and say 'the right thing' when in contact with them.

So I was encouraged to think in terms of doing research on these people when I was requested to prepare a potential research proposal in my Native studies course. It is interesting to look back on my research intentions during the first few months of my program, when my goal was to:

study the relationship between prostitution and militarism by examining the conditions of prostitution between Innu women and military men. By exploring this relationship I hope to gain a better understanding of the status of Native women both in their own community and in regards to non-Natives. As well I will consider the theory that militarism helps to develop a community¹⁵, by examining the effect on Innu women. My study will test the hypothesis that conditions of poverty have combined with militarism to encourage Innu women into prostitution.

Clearly an admirable anthropological task; with data to collect, theories to analyze, and even an hypothesis to test.

In the proposal I outlined plans to do participant observation by accompanying the women to the military base.

¹⁴ I use the term 'Native people' here as this is the term I used then. Later I was told that, according to the Assembly of First Nations (AFN), the appropriate term to use is Aboriginal peoples or First Nations peoples.

¹⁵ The residents of Happy Valley-Goose Bay wanted the base to be built as it would bring employment and infrastructure to the area.

I also intended to "interview people, mainly women, for their views of , and experiences with, prostitution". Looking back on this agenda I am surprised at my lack of tact. I had an investigative mentality with no consideration for the thoughts, feelings, or needs of those I was proposing to study.

I had three research goals. 1) I wanted my research to be "stimulating and educational". 2) I planned to add to the data and theories on women's oppression. I stated: "While I realize that the final draft of this research will not please everyone, I hope to provide my readers with a greater awareness of the issues facing Innu women". 3) I wanted to produce a Master's thesis and a publication on prostitution and militarism.

At this stage of my work I had planned to apply for funding to such dubious sources as the Department of National Defense (DND) and the Department of Indian and Northern Affairs (DIAND)¹⁶. I also thought I would bestow my academic expertise on individuals in the community that wanted my help.

With this approach, when I stated that my research would not please everyone I did not realize how accurate I was. My research would have done nothing to benefit the Innu people. They did not need my research to inform them

¹⁶ DND was working with the NATO allies in Goose Bay and DIAND was infamous for its paternalism towards Aboriginal peoples.

about the connection between militarism and prostitution: they experienced it directly.

Similar to the anthropologists outlined earlier in this chapter, I had no plans to consult with the Innu about my work except to get their permission to study them and then to ensure that my data were correct. I would decide on my own what I would research and how it would be carried out. I alone would analyze the material and then use the results for my benefit. No control was given to the Innu, although I was 'graciously' willing to offer my academic skill to a few Innu people. Fortunately these research plans were halted.

Fortunately, before I made contact with the Innu a number of people told me that my topic was inappropriate. I was told gently, and not so gently, by native women, feminist scholars, and political activists: not only did my topic connect native women with prostitution but it was problematic for a white woman to do any research on Native women.

I was told that Native women were often stereotyped as 'whores', a label that had earned them a history of physical and psychological abuse. I also learnt that many Native people believed that any research to be done on them should be done by themselves. Had I accepted this I would have had to abandon my research agenda.

In despair I told my advisor of these problems with the topic of prostitution among the Innu. He encouraged me not

to abandon the topic but instead make it more palatable to those I would be studying. He suggested that I tell people I was studying the social effects of NATO on women, but for my thesis continue an unstated focus on prostitution. I was uncomfortable with this suggestion that I hide my research agenda from the Innu.

Fortunately, (for me) I found another way. Through my studies in feminism I was introduced to an 'alternative' approach to research. Readings on feminist methodology discussed working with women rather than on them. This literature mentioned work that benefited those studied, suggesting there was no need to maintain a distance between the researcher and the researched, in fact it was discouraged (Mies, 1987; Reinharz, 1987).

An exposure to this material prompted me to want to make my work acceptable to the Innu people, and to those critiquing my original topic. I decided to have the Innu determine what I would research: I would offer my services as a researcher to them.

Why did I choose to continue research on the Innu rather than turn my investigation of prostitution and militarism to another group? Reflecting back on the choices I made and the training and values that informed these choices provides a clue to my decision. Earlier I mentioned that when I was trying to study prostitution in Ottawa it was a sense of intimidation and fear of being invasive that

stopped me from approaching hookers for information. Yet I was willing to do research on women from an entirely different culture. Why? I believe that my choice of 'people' was affected by the long history of anthropologists studying small-scale, 'native' communities. Because of their 'otherness' I believed the Innu would be willing to accept my presence in their community. I had read many ethnographies in which the anthropologist moved into an 'exotic' community with no apparent resistance from those s/he was studying.

My racism and regionalism came into play as I viewed the Innu as objects of study. I felt that I was doing 'non-traditional' research by focusing on women and critiquing NATO. Yet my work was typical in that I planned to use the Innu for information, publications and a degree.

The scholars that I presented my research agenda to supported my plans to do 'alternative research' but encouraged me to select a focus for my work. One feminist scholar mentioned that I would need to select a topic as non-academics were unfamiliar with the research process and would therefore be unable to guide my work, or they would present me with an unmanageable topic. She suggested that I work with a women's group to get a direction and focus for my work. My reading of the literature on Innu protests had informed me of the existence of an Innu women's group. So I decided I would research how these women organized their

protests against NATO. I believed that this would be a valuable contribution to feminist literature on women organizing for change. I was interested in providing a unique perspective, an Innu perspective.

I then consulted with another feminist scholar who pointed out that Innu women already knew how they organized and hence the work would benefit no one but the Canadian Security Intelligence Service (CSIS).¹⁷ I decided to return to my plan to record the effects of NATO on Innu women. Each of these plans were made without having even received permission to enter an Innu community.

I had a topic and a methodology, all I needed was to make contact with the Innu. The question was how? I attempted to network with people who had been involved in protests supporting the Innu efforts against NATO. These people were unhelpful; was it because they lacked contacts with the Innu, or because they feared imposing a white researcher on them? I do not know. When I went to the Native Women's Association of Canada I failed to receive confirmation that they would liaison with the Innu for me. They did, however, emphasize the importance of having the Innu control the research process.

I was ready to abandon my plan to work with the Innu and was preparing an alternative thesis topic when I was

¹⁷ Because of their protests against NATO, CSIS had been investigating the Innu for 'foreign' influences (Lee, 1991).

referred to a scholar in the Carleton University School of Social Work who had worked with the Innu. I went to see her and she referred me to an anthropologist at Memorial University who worked with the Innu. I contacted him, he asked me a few questions about my research approach and then gave me the number of an Innu woman in the community of Sheshatshit.

Nervously, I phoned this woman, Rose Gregoire, and offered my services as a researcher. When I suggested that I record the effects of NATO on women she informed me that such a report had already been done. She suggested instead that I go out to a camp in the bush (which the Innu call 'the country') with her sister's family. I agreed but told her I would need time to prepare. She encouraged me to hurry out as they would soon be returning to the community.

I was humbled to have made contact and been given permission to visit them. But I had misgivings. How would I find enough time to do research on the Innu before I went out to Labrador? How would I study Innu women organizing against NATO if I was out in a camp in the country?

Similar to the early anthropologists who studied 'primitives' to understand the origins of 'man', I was interested in studying Innu women to understand the oppression of women. My research was also motivated by the belief that it was important for white middle-class feminists to have an understanding of the lives of women

different from themselves but sharing in a common oppression.

Despite the change in my agenda, suggested by Rose Gregoire, I was excited about my impending trip to Labrador. Looking back I feel it was bold of me to ask strangers if I could live and study in their community. They did not know who I was. I later discovered that they had assumed I was a friend of the anthropologist who referred me to them. They had not realized that we had only spoken briefly over the phone.

I arrived on the evening of May 22, 1990 and the following morning took a helicopter¹⁸ out to a camp south of the Mealy Mountains to arrive in 'the field'.¹⁹

What did the Innu think about this strange white women coming to stay with them? I am unsure but I do know that they graciously welcomed me into their homes and showed me their way of life. I was taught how to pluck ducks, pick boughs, clean fish and cut wood. I did mainly women's work, or perhaps I should say girl's work due to my low level of understanding in the country, but I also went on a hunting trip.

Our camp was composed of four tents with approximately 25 adults and children. The family I stayed with had eight

¹⁸ The trip, paid for by the band council, brought supplies and me to the camp.

¹⁹ I use '' here to emphasize my discomfort with the objectification of the Innu nation to extract data.

people in their tent. The language spoken was Innu-eimun. The younger people spoke English to me but not the elders. The camp had a cross-section of children, youth, middle-aged people and elders, all of who had particular tasks they performed. I was fortunate enough to witness two caribou being shot and cut up.

Our diet consisted mainly of bread and wild meat supplemented by processed food and fruit. While in the camp I spent my time fishing, cooking, chatting and writing.

I had decided, a few months before entering the field, that I would not engage in 'traditional research'. I was not even going to take fieldnotes (like a 'typical' anthropologist) and make the Innu into objects of study. I wanted to do what they wanted me to and I wanted to fit in as much as possible.

Before I left for Labrador, however, several anthropologists had advised me to at least keep a journal. So, although I took no fieldnotes in the camp, I faithfully wrote in my journal. I did so to provide myself with an identity and reason for being there.

The family I stayed with appeared uncomfortable when I wrote in my journal. They did not know what I was writing or what I would do with the material. I tried to reassure them that anything I wrote would be sent back to them before release but they had no guarantee of this and found it strange for a guest to be 'taking notes'. Although I

maintained I would not, looking back I likely was taking a form of fieldnotes.

The unfortunate aspect of my visit to the country was the romanticism I attached to the people living there. On one hunting trip I got separated from the men I was with but assumed that they would find me as they were 'Indian' and therefore totally in tune with the countryside. I realize now this was foolish but a typical response for a white urbanite coming into contact with Aboriginal people who spend time in the bush. Also problematic was my desire to get a Master's thesis out of the experience.

The positive effect of my visit to the country for me was the exposure it gave me to the Innu way of life. Whether I wanted to label it as such or not, I was doing participant observation by participating in the daily activities of the Innu and noting what I saw in my journal. I was privileged to experience this way of life and learn the things that I did. It was an invaluable experience that gave me some insight into the Innu people: their culture, their struggles, their problems and their spirit.

Upon my return to the village of Sheshatshit it was suggested by Rose Gregoire that I interview people about the differences between the country and the community. I worked with two community members to collect this information which

we agreed would be housed in the community resource centre.²⁰ I convinced myself that I was doing the work for the community, not to collect data for my research.

Despite the knowledge I gained living with the Innu people I was determined not to produce 'yet another' ethnography. I had changed from my original intentions of using the Innu for data to wanting to work for them - but I still needed to produce a thesis.

After spending two weeks in the country and two months in the village I felt close to the community, and this had nothing to do with academic interests. Still, I had spent ten weeks in 'the field' and I was expected to write a thesis. Despite my advisors' interest and enthusiasm for the ethnographic data they thought I had collected, I decided to use my experience in a different way.

While in Labrador I made efforts to involve the Innu in the work I was doing. To some degree this succeeded but in other ways my efforts were limited. I had also intended to apply a feminist methodology in my work. I entered the field with no intention of 'pretending' I would be doing objective or value-neutral work, like feminist scholars such as Mies (1987) and Reinharz (1987) suggested. My plan was to work exclusively with women. In the end however, although the lives of women

²⁰ This process will be elaborated on in the next chapter on participatory research.

were not the sole focus of it.

These changes in research plans led me to decide to write my thesis on methodology. I would analyze and critique my efforts to avoid traditional anthropology and use participatory research and feminist methodologies. I believed by addressing this topic I would avoid exploiting the Innu for data. This thesis is a result of my decision to proceed in this way: however, I now have qualms about this choice. The focus of my thesis is reflexive. The question I now ask myself is: to what use can my reflexive work be put? The motivation for my work is to help researchers do 'more appropriate' research. By this I mean research that is responsible to, and of value to, the people under consideration. Too often anthropologists, and researchers in general, have entered native communities solely to extract data leaving behind no benefits (Richer, 1985). I hope that by producing this work other researchers will question their research assumptions, critique their methodologies, and adopt more responsible approaches to research. I also intend this work to be of assistance to those who are already converted to 'alternative' research approaches for people and are looking to apply them and/or are experiencing difficulties similar to those I did. I offer criticisms of participatory and feminist approaches to improve them, not to discard them.

But my question is, who will read this work? How will

the Innu benefit from my thesis? Is it more than an academic exercise that my supervisors will read? Am I in fact navel-gazing as some anthropologists have accused reflexive anthropologists of doing? Even worse, am I not exploiting the Innu for data by producing a thesis that discusses my work in Sheshatshit?²¹

I never received feedback on the material I wrote and sent back to community members for their comments and approval. Was that because the work is irrelevant to the community? Perhaps having to meet the daily demands of poverty, racism and raising a family, made the issue of what constitutes an appropriate methodology less pertinent. I also wonder if it was people's perception that I was exploiting them for data, which prompted them to suggest I abandon my studies and return to the community without the status of an academic.

During my interviews in the first summer of my work with the Innu I spoke with several elders²². At the end of the interviews I asked them if they had any questions for me or any comments to make. Several of them requested that I: 'tell the government that we are here and we continue to return to the country and live off the land'. I agreed to do so, however, my thesis is a study of methodology, not a report on Innu life. An alternative would have been to

²¹ Thanks to Jared Keil for this observation.

²² Through an interpreter.

produce a critique of the government's treatment of the Innu people. But because I felt I was not an expert of the Innu (and did not feel right discussing them as subjects) I choose to do reflexive work. This work does not inform those with power about the Innu people and their struggle.

The following summer I had intended to return to Sheshatshit and question people about the success or failure of my research approach the summer before. However, several people expressed discomfort with my plans to continue work as a researcher. As a result I abandoned this idea opting instead to offer to collect the life histories of two women in the community²³. I felt that the life history method would be a way to do work that would benefit the Innu people, getting their story out to the Canadian public. They received this suggestion enthusiastically but suggested that more women be involved. It was in this way that my work with the Innu people came to meet local goals. And it is in this way that I will be able to fulfil the elders' request that I tell people about their existence. In fact, the Innu will be telling Canadians themselves. They have been educating the public about their struggle against NATO through their protest actions and tours world-wide. With the publication of the life histories I hope to encourage the public to see the people behind the newspaper and film images. Also, I wanted others to see behind the

²³ To be elaborated on in chapter five.

stereotypes, informed by evolutionary anthropology, that painted images of native peoples being stuck in the past.

Chapter 3

RESPONSIBLE AND PARTICIPATORY RESEARCH: Towards Appropriate Research

Introduction

This chapter responds to the methodologies outlined in the previous chapter. As I explained in chapter two, there are several difficulties with adopting a positivistic approach to research with Aboriginal peoples. Reflexive work may also fail to consider the needs of those studied. This chapter will explore critiques made against anthropological studies from a native perspective, and then suggest guidelines for doing more appropriate research. I will also describe an 'alternative' research approach - participatory research - and its consequences, both positive and negative, for work with all people. Finally, to illustrate the critiques of anthropology and the effects of doing participatory research, I will conclude the chapter with a discussion of my participatory research with the Innu people and their response to it.

Critiques with a Native Perspective

Aboriginal and Third World peoples have condemned the work of anthropologists for many reasons. A basic criticism lodged against the discipline is its irrelevance to their

lives. Anthropologists that enter communities to study topics of importance to themselves, and fail to consult with those they study, produce material that is largely useless to community members. Though the issues explored may be exciting for the anthropologist and his colleagues, and the field experience may serve as a source of publications, the material produced is largely consumed by other anthropologists who are usually white, middle-class men with tenure. Cross-cultural comparisons are made, theoretical debates rage, and more and more data are collected; all to further 'the study of man'. It is the advancement of knowledge rather than the improvement of Aboriginal lives that motivates most anthropological work. It is in fact to the advantage of anthropologists to avoid assisting Aboriginal communities in their quest for self-determination, because, once having achieved some portion of this, they typically begin asserting their rights in terms of the research done on them.

For instance, the Gitksan-Wet'suwet'en of British Columbia, who have been studied by anthropologists for years, have recently established regulations for researchers working in their communities (Richer, 1988):

a/ the work is to be done in a certain time frame²⁴;

²⁴ According to John Cove, a professor at Carleton University who provided with these guidelines, the research suggested is contract work. The anthropologist had a specific topic to research and a certain time to do it in.

b/ the research shall 'indemnify and save harmless' the Tribal Council (e.g., against claims/damages);

c/ all reports, notes and data are the property of the Tribal Council²⁵

d/ the Tribal Council can use any results without the consent of the researcher, as long as the researcher's name is not used (46).

Putting a time limit on the research that an anthropologist does directly contravenes one of the discipline's distinct characteristics, the prolonged and intimate relationship that the anthropologist attempts to establish with those under study. Due to the lack of distance in the field between the anthropologist and those she is studying, efforts to do positivistic research in anthropology have typically been premised on the need to collection of large amounts of data. It was also assumed necessary to test and verify theories through long-term study. By restricting the amount of time the anthropologist spends in the field, for instance, it is no longer her choice whether or not to do a year of fieldwork in a particular community before writing her dissertation. A time restriction limits the anthropologist's ability to collect data on all aspects of a society, material which ethnographies is usually based on. Research plans must be instead limited, specific and well-organized.

²⁵ Cove also says (personal communication) that original data stays in the community but the anthropologist can keep a copy for him/herself. The anthropologist must have any work produced from the data approved by the band council before it is released.

Items c/ and d/ strike at the heart of scholarly work as the people studied claim ownership over the knowledge produced with respect to themselves. Much effort is made in academia to earn a reputation for ourselves via the publications that result from the data we collect and analyze. However, according to these regulations, the results of these works, which we come to see as our creation, can be taken from us before it is moulded (as in the case of notes and data) or after it has been modeled (as in reports and other works).

The question however remains: Was this material ever really ours, or were we robbing others of what is rightfully theirs? Several scholars have in fact suggested that the discipline of anthropology has been based on the exploitation of peoples (Patai, 1991; Kayongo-Male, 1979; Trinh²⁶, 1989; Richer, 1988). We enter a community, at times even without permission, live with people, eat their food and make demands of their time and energy for our own benefit. We ask questions, collect artifacts, and study people in order to produce journal articles, publish books and present papers; all of which further our careers and thereby give us the status and material comforts that characterize the life of an academic.

²⁶ This author's name is listed on the cover of her book as Trinh Minh ha, however, in the book's bibliography she cites herself as Trinh, Mihn ha. Therefore this thesis will cite her as Trinh.

Usually the material produced by anthropologists is of no use to those studied, and, tragically, there are cases in which the material collected was used against the people. Anthropologists of the past worked with colonial administrators to assist in the 'effective rule of colonial subjects'. This relationship has been extended, as anthropologists are now often employed by non-governmental organizations, governments and multi-national corporations more so today. These agencies often apply the product of the anthropologist to implement 'development' schemes. The schemes are created and applied without consulting the people at whom its projects are directed. In some cases the only contact the organizations have with the affected people is through the anthropologist, who is considered an 'expert' on the culture studied. Unfortunately, because he is paid by the organization, his allegiance is most often with them, not the people. In an editorial in Current Anthropology, Kayongo-Male (1979) commented on the state of anthropology:

many of these "experts" often become representatives of official organizations and begin deciding the course of national development, perhaps even acting as supervisors of the implementation of projects, in the foreign country. Even efforts at collaborative research between insiders and outsiders seem to have tinges of academic paternalism, the outsiders managing to dominate the selection of problems and even of theoretical approaches, very often by virtue of their wider access to research funding (569).

These projects, and the research that accompanies them, neglect the concerns and opinions of the targeted group.

Agendas for 'development' are decided by elites in First World countries who use the work of anthropologists to justify their policies. Multi-nationals such as Xerox (Deutsch, 1991) use the work of anthropologists to assist them in their labour and market agendas: profit making and exploitation. Richer points out that: "colonial states can be extremely interested in using anthropologists and their work as instruments of social control" (412).

One of the difficulties with the use of anthropologists for work of this nature is that it sets up scholars as the 'experts' on a particular people. Gururani (1991) notes that the voice of native was "completely muffled under the domineering tone of the anthropologist" who sought to "interpret the native's point of view". For her, ethnography is "enmeshed in a world of enduring and changing power inequalities" where Western assumptions and categories are unavoidable (2-4). In fact, as she points out, the:

highly valued personalized relationship between the ethnographer and his 'subject', simply masks the actual difference of power and knowledge which flourishes underneath ... (and) the simple gesture of the ethnographer to go and prepare 'their' ethnographies is a power related decision, as the ethnographer presupposes a position of the 'knower' (8).

Even in the post-modern texts, which seek to give a voice to 'the native', the ethnographer controls what gets published. Gururani points out that in Marjorie Shostak's life history of a !Kung woman, Nisa speaks only because Shostak wants

Nisa to speak (14). It remains "the responsibility of the ethnographer to interpret the native's point of view ... and it is the anthropologist who still holds the reign of describing the other" (3).

Patai (1991) and Richer (1988) both describe the research process as exploitation. We extract raw material in terms of peoples' stories (Patai) and the cultural elements of a people (Richer) for data which we then turn into a commodity - published material - that benefits us but not those we study. Richer says:

For the anthropologist operating within a capitalist nexus, the ethnography becomes a commodity--it can be exchanged with universities, colleges, and publishers for a host of values, including advanced degrees, professional reputation, career mobility and book revenues, to name a few (412).

Anthropology is also said to be a collection of inaccurate pictures painted by cultural 'strangers'. Critiques of these 'strange' anthropological 'masters' have come from scholars within the discipline, and from those studied. One particularly vivid example of the damage caused by anthropological accounts of 'primitives' appeared in Ogan (1975), who cites (from Kasaipwalova) a Trobriand Islander:

I have many times felt very embarrassed and awkward when I meet new people and they ask me 'Where are you from?' and I say, 'Oh, I am from the Trobriand Islands,' and they reply excitedly, 'Ohhh, Malinowski, free love!!' What I am pointing out is that if we are going to depend on anthropological studies to define our history and our culture and our 'future', then we are lost (328).

This type of association could lead to dangerous consequences for Trobriand women just as the stereotype of Native women as 'whores' has led to sexual assault.²⁷ The further harmful effects of anthropological studies of the lives of women will be explored in the next chapter.

Trinh Minh-ha, in her book Woman Native Other (1989), discusses the detrimental effect of anthropology's tendency to objectify 'other' women and men. She says:

I have wondered time and again about my reading myself as I feel he reads me and my false encounter with the other in me whose non-being/being he claims to have captured, solidified, and pinned to a butterfly board (my emphasis) (48).

She believes the objectification of 'the other' stems from racism and scientific objectivity. She makes us squirm with embarrassment as she throws the titles of famous anthropological books at us such as How Natives Think, The "Soul" of the Primitive, The Savage Mind, The Sexual Life of Savages and Primitive Mentality.

Trinh critiques Malinowski's call for scientific work for its silencing and neutralizing of others:

"Complete," "systematic," "methodical," "scientifically trained," "really relevant," "aims," "real importance"--so much of the scientific mind condensed in such a repetitively short statement. Common sense turns out to be the kingdom of the mute, the unreal, and the "illiterate." (57)

She goes on to connect earlier anthropology to contemporary work:

²⁷ Personal communication with native woman at AFN.

The Great Master ²⁸ has made it clear: anthropology is a question of paying off old scores between white men. Science is Truth, and what anthropology seeks first and foremost through its noble defense of the native's cause (whose cause? you may ask) is its own elevation to the rank of Science. Sharing the Great Master's endeavour, men highly committed to their academic community continue thus to maintain that 'anthropology is not the bastard of colonialism but the legitimate offspring of the Enlightenment'²⁹ and suggest with complacency that 'despite their failings, social anthropologists have on the whole been at least as competent and perceptive as factory inspectors ... and perhaps have worked harder and suffered more' (Trinh's italics) (57).

She condemns the idea that Third World people need to be trained to do anthropology and cannot be expected to be more "racism-free or anticolonial ... than outsiders". ³⁰ She complains that the discipline's approach is that:

Natives must be taught in order to be anti-colonialist and de-westernized; ³¹ they are, indeed, in this world of inequity, the handicapped who cannot represent themselves and have to either be represented or learn how to represent themselves (59).

She identifies another biting truth of anthropology by equating it with 'gossip':

A conversation of 'us' with 'us' about 'them' is a conversation in which 'them' is silenced. 'Them' always stands on the other side of the hill, naked and speechless, barely present in its absence.

²⁸ Malinowski

²⁹ Raymond Firth's Proceedings of the British Academy cited in Trinh.

³⁰ Cited in Trinh as "several anthropologists' responses to Diane Lewis's essay (155)" "Anthropology and Colonialism" in Current Anthropology 14 (5), 1973.

³¹ Who better to teach 'them' than 'us' in the West.

Subject of discussion, 'them' is only admitted among 'us', the discussing subjects, when accompanied or introduced by an 'us,' member, hence the dependency of 'them' and its need to acquire good manners for the membership standing. The privilege to sit at table with 'us,' however, proves both uplifting and demeaning. It impels 'them' to partake in the reduction of itself and the appropriation of its otherness by a detaching 'us' discourse ... a false incorporation ... (that) allows itself to nibble at the bait of Lies (67).

Trinh's description of anthropology as a union of gossip and science reminds us of the games we play in our discipline:

the gossipers' need to act in solidarity, leaning on and referring to each other for more credibility. The confidence they ³² (re)gain through the ritual of citing all their fellows' (dead or living) names had allowed them to speak with the apathetic tone of the voice of knowledge ... Scientific gossip, therefore, often unveils itself as none other than a form on institutionalized Indiscretion (my italics) (68).

Kadegis in her "Sabbatical Song" illustrates the feeling of objectification that anthropologists create:

Hey, I'm a fieldwork object.
I'm the lost Property Office
for mislaid identities ...
as I watch them scribble.
Sometimes I'll undress
and give them a toss:
to gain my culture
the kudos of
promiscuity.
(It makes them feel good):
to think they have
fathomed me.
So I act sort of gawkily
and watch:
the theories grow (228).

The Cree writer Joy Asham Fedorick (1991) ponders why

³² we

others³³ want to study native peoples. She suggests this is because: "We're interesting to look at, analyze and speculate upon". She notes that even the Pope is interested in Native spirituality. Further motivation is provided by altruism, "the growing market for things-remotely-Native, and the need to exercise power" (4). Fedorick goes on to describe her dinner conversation with a Caucasian female sociologist who was practising 'Cocktail Anthropology' on her. Her footnote describes this phenomenon via a poem by Aboriginal poetess Skyros Bruce:

Cocktail Anthropology
Want to measure how long
My fingernails grow (8)

At the dinner the women was:

badgering me with endless questions about 'what is it really like to be a Native woman'. I was hungry and wanted to eat. Sometimes we evade these questions, you know, just wanting to be. But finally, figuring if I ever was going to eat in peace, I'd better answer. I told her what it's like in this skin, the good, the bad, the perceived ugly, and once more, picked up my fork. 'That's not what it's like at all,' she said."!!
(my emphasis)(4).

She suggests that those who do work with Native culture examine their reasons for wanting to do so, and work with people rather than in competition with them. She also suggests a 'self-censorship checklist:

The Ethical and Protocol Positives:

Am I doing this with permission, both before

³³ Interesting play on words here. By 'others' she means non-Native people.

undertaking and before releasing completed artwork or writing?

Do I have the consent of those affected?

Have I attempted to use 'as near as...that man's words'?

Am I being humble? honest? responsible? caring? open-minded and aware of my own filter screens?....

Am I courteous and fair?

The Disgruntling Negatives:

Am I caricaturizing instead of characterizing and thus increasing negative stereotypes?

Am I commercially-driven ³⁴

Am I reading my own cultural interpretation into what I perceive? ³⁵

Am I failing to credit sources? ³⁶

Are my intentions destructive?

Am I being arrogant? dishonest? untruthful? disrespectful? (6).

This checklist contrasts with a sociologist's suggestions for work with indigenous peoples. Richer's (1988) focus is on sharing the research kitty with those studied. He advises:

1/ A research tax on anthropologists to support indigenous university students and/or faculty;

2/ Co-authorship of monographs, as well as share in book revenues. One might even envision retroactive claims made on revenues of well-known and financially successful ethnographies of the past;

3/ The receipt of a portion of anthropological research grants;

4/ The regular garnishing of a portion of an

³⁴ Or academically-driven?

³⁵ Some anthropologists would argue this is impossible to avoid. The latter part of this chapter and chapter four will explore ways to avoid this problem.

³⁶ In this case Aboriginal sources, not academic sources. See Trinh's comment on the use of academic referencing above.

anthropologist's university or research institute salary (417).

The need, however, it is not only access to the material benefits of research. It is important that the whole enterprise of the research process and the motivation behind it be deconstructed. Only by removing the expertise and the irrelevance of anthropological research can the process have a future with Aboriginal peoples.

Gururani (1991) illuminates the power relations in anthropology by describing the motivation for our work:

The anthropological endeavour 'to know' and 'to represent' the 'other' (non-Western, colored and those without history) has grounded the discipline itself in one-sided, skewed discourse of dominance. This 'other' which constitutes the 'object' of study is clearly defined as the 'distanced, different and mute'. And it remains the singular (authoritarian) enterprise of the anthropologist (usually the White male) 'to recreate' and 'to represent' the social 'reality' of the 'other'. Anthropology hence assumed the increasing responsibility to 'know' and to describe the contemporaneous savage in order to salvage the savage from being pushed into oblivion by the global process of Westernization. This ethnographic exercise of 'othering' should be seen embedded in power related discourses of Western and non-Western, of white and colored, and of self and other, which have ever since been produced and reproduced (1).

And despite efforts to break with the authoritarian voice of early work, reflexive and post-modern works continue to view the social description of the other as the core activity (Rabinow cited in Gururani, 1991:3).

When 'the other' is given a voice it is silenced by post-modernism. Gururani cites Hartsock, who asks:

Why is it that just when subject or marginalized peoples like blacks, the colonized and women have begun to have and demand a voice, they are told by the white boys that there can be no authoritarian speaker or subject (8).

Trinh (1989) claims that the motivation for reflexive work continues to be one's career and is not done for the benefit of 'the other' (157). Warry (1990) sees the discipline as concerned with "sterile questions of representation and reflexivity" (62) and condemns "the ineffective musings of post-modernist writers whose attention to discourse leads to apolitical ethnography" (69). He also points to the disillusionment of Native peoples with anthropology:

Native people view anthropology as largely esoteric, irrelevant and as incapable of contributing to solutions to problems facing their communities. Native people say they have been 'researched to death'.... Research findings, cloaked in jargon, have been unintelligible to communities or have been largely irrelevant to community needs (63).

Warry sees the basis of the problem as the lack of control Aboriginal people have had over the research process:

Taken as a collective body of work, anthropological research has been scarred by an inability to consistently produce research of an applied or theoretical nature, which is truly meaningful in addressing the manifest problems faced by Native communities. The philosophical and structural foundations of the discipline encourage the possession, propagation and ownership of indigenous knowledge through esoteric control over research in order to construct and protect university-based careers. In sum, anthropology continues to engage in neo-colonial research which exploits indigenous knowledge (63).

As students we are not taught to do responsible research but

to produce work for other academics:

This ideology is no more evident than in our graduate training, where on-going collaboration in analysis is actively discouraged, and where Ph.D. requirements render dissertations unintelligible to local communities (66).

Fortunately there are alternatives to all these criticisms--of research that is irrelevant, inaccessible and non-directed. One possibility that is more readily accepted in Native communities and appears often in the Third World is participatory research (Warry, 1990; Small, 1988).

Participatory Research

Participatory research, as defined by Freire (1990), is a method of empowerment for everyone involved in the research project³⁷. There are no clear distinctions between the researcher and the researched. Instead, those in the targeted community define the problem that needs to be explored and select the topic to be researched. The people decide how the research will be carried out, and once they collect the data, they either do or participate in its analysis. Of prime importance is their control over the results; they decide what is to be done with the final product.

This approach to research, like a feminist approach, cuts across disciplines and does not need to be, in fact

³⁷ An approach that can be used in any type of community research, whatever the discipline.

rarely is, restricted to academic work. Those involved in the work seek to empower everyone involved in the process but also produce an end-product, either a report of some nature or a change in people's lives.

The participatory research model originated from work done in the area of adult education. In his literacy work with Brazilian peasants, Paulo Freire realized that the material the students were working with was irrelevant to their daily struggles, and that the traditional approach to teaching, in which it was supposed that the teacher knew more than the students, underestimated the intelligence of those he was working with.

Freire's alternative was to involve people directly in the production of a knowledge that differs from that controlled by the elite. Rajesh Tandon (1988), coordinator of the International Participatory Research Network explains:

The major element of this philosophy is based on the premise that adults are capable. They are capable of learning, of changing, of acting, and of transforming the world.(5)

This contrasted with:

the increasing organization of knowledge industry ... produce its own agents--the professionally trained researchers, who alone were seen as legitimate producers of knowledge... (which) created a widespread notion that ordinary people lacked the capacities and the tools for production of knowledge ... (instead) it took extraordinary training in elite academic institutions to be able to acquire the competencies to produce the knowledge (8).

The goal of this research approach was to free the oppressed. Freire believed that the oppressed 'needed to realize' the conditions of domination under which they were existing, and the exploitation they experienced. Having done this, they would over-throw their oppressors to claim freedom as their prize. He believed that participatory process would assist in the education of the oppressed, which would in turn lead them to take action and change their living conditions (Freire, 1979; Tandon, 1988).

Verna St.Denis reminds us that knowledge is power, and when the powerless generate their own knowledge, the balance of power shifts into the hands of the powerless (St. Denis, 1989:26). John Gaventa, research Co-ordinator at the Highland Research and Education Center, explains the way in which participatory research³⁸ demystifies the research process (1988):

The participatory process of confronting the knowledge-holders also provides an experience which can help to develop consciousness of how the power structure actually works. People may discover for themselves dominant knowledge or interpretations of reality which do not conform to their own experience, in which case they must ask why not. Or the process of popular investigation may reveal previously hidden information that does confirm through 'official' knowledge what the people have suspected from their own experience. When the former occurs, people may continue to question and to pursue the contradictions. When the latter is the case, the fusion of the official knowledge with that of popular experience gives validity to the people's claims and may unleash

³⁸ St. Denis calls the process research. An end product is obtained.

new movement ... (21).

This approach to research avoids all attempts at positivistic research. The principles of objectivity and value-neutrality are contrary to participatory research which discourages impartiality and distance between those involved in the project. As well, the goal is not to add to academia's search for truth, but to instead assist in the dismantling of oppressive structures. St. Denis claims that research which fails to do this (research that is not 'for the people') is immoral (31).

The role of the researcher, according to Tandon (1988), is then to avoid an elitist methodology and to instead "explode the myth of neutrality and objectivity ... emphasiz(ing) the principles of subjectivity, involvement, insertion and consensual validation ..." (7). Gaventa (1988) says there should be a break-down of "the distinction between the researcher and the researched, the subjects and objects of knowledge production ..." (19). Those experiencing problems are the best able to understand them, and should therefore conduct the research.

Warry (1990) sees collaborative work as important. He says that what Aboriginal people desire "is nothing short of full partnership in the research process" (62). He believes that participatory research is the logical extension of Native efforts at self-determination.

He states that it is not only important to involve

Native representatives in the design, implementation and interpretation of the project, but also train them to do the work on their own. The outside researcher then becomes a trainer or facilitator who encourages community fora and workshops (67-69). By involving Native people in the interpretation of research findings, indigenous science, "which is based on experiential and humanistic interpretation" (64), is taken into account. Warray places importance on sensitivity to the knowledge of those working in the community, in order to provide for culturally appropriate research (65).

"Guidelines for Community-Based Participatory Research"

Based on her study of a needs assessment project for the Fairbanks Native Association that utilized participatory research, St. Denis makes several recommendations (1989:98-99)³⁹. She suggests that participatory research takes time. It is important for people to get to know each other and have time to express their opinions. Trust is very important. Regularly scheduled, well-publicized meetings are necessary to encourage participation. Those involved should realize that the process of the research, in which the community is participating, is more important than the

³⁹ Granted it may have been preferable not to have had to do any research but since effective administration of government funding necessitated a needs assessment project, members of the community decided to make it participatory.

end-product. It is important to have the involvement of leaders in the community and to be sensitive to them. Barriers to participation must be considered. The professional researchers⁴⁰ must be experienced group facilitators. They must also give up assumed control and power and share in decision-making. As well, researchers must examine their assumptions about the community. Do they trust those involved and are they trusted? Use of jargon should be avoided. Because participants are more familiar with having research done on them than doing it themselves, they need to know what the community wants from them and how they can meet these expectations. There must finally be a sensitivity to grass-roots opinions and a variety of voices, as participatory research is "a process of facilitating communication and an understanding of the needs of the community" (99).

Difficulties and Problems with Participatory Research

Although a vast improvement over empirical research, participatory research is not a perfect research approach. Criticisms of participatory research have come from scholars and from those studied.

Worry warns that some scholars view participatory

⁴⁰ Presumably 'professional' were brought in to facilitate the process. That is to organize and focus the project and ensure that the issues were addressed that needed to be. Most community members had other demands on their time.

research as 'anti-theoretical' and disruptive of the research process. Because the work is not done solely by an expert the research process is contaminated. They claim that the use of participatory research in anthropology will lead to "the loss of objectivity, heightened relativism, censorship, or the politicization of the discipline" (70). In response, Warry admits that scholars involved in this process loose power over the analysis and the use of the data collected, and may need to abandon their research agendas directed at contributing to the discipline's theoretical debates (62). However, he says collaborative work does not involve:

the abandonment of explanatory power in any way, shape or form. There is nothing in participatory research which threatens the integrity of a science of culture.... Theory formulation is informed by the inclusion of indigenous interpretation. Our explanations are strengthened by the discourse that arises between Native and non-Native analysis (70).

Power must be relinquished if the discipline is to acknowledge Aboriginal quests for self-determination.

Richer (1988) has a more cynical view of academic efforts in participatory research. He views work of this nature as a response to Aboriginal demands for control over research done in their communities. In order to gain access to Native culture, the anthropologist must now agree to do work for the community. Richer also sees this choice of methodology as having monetary motivation. As cutbacks effect the funding and availability of positions at the

university, researchers must seek employment from Aboriginal peoples, and must therefore do research that is of interest to the community (417).

Delle Small takes this argument a step further. Given the lack of academic support she received for her plans to do participatory research, she doubts that academics will adopt this research approach:

even if they have the talent and inclination. (We know that grassroots people tend to be anti-intellectual, and that intellectuals in universities generally get their rewards from more patently scholarly work.) Participatory research needs an input of time and energy which academics can rarely give ... for university staff to make themselves available to community groups, they may need to reduce their commitment to teaching ... and some of the other demands being made on them ... (And) academics ... are not generally in touch with the community. They serve a conservative institution which as a result of economic and political measures is becoming increasingly elite ... for the most part, radical research has had little support (93-94).

However, she herself did eventually take the initiative and introduce participatory research to her students and began work with them and the women's community where she lived.

Patricia Maguire (1983) also embarked on the difficult journey of doing participatory research at the grass-roots level. Through her work at a shelter for battered women, she developed the idea of conducting follow-up sessions with women using a participatory research model.

In her work she faced several difficulties. For one, Maguire found the work emotionally exhausting. She was unable to:

hide behind the rhetoric, radical or otherwise. When deciding to be 'with the people' you are forced to continually examine what that looks like in everyday life. Intellectual theorizing and radical structural analyses are not enough. Finding a way to do research which attempts to close the gap between theory and practice, or thinking and doing, was difficult. Many times I had to choose between staying home to read or write and going over to the shelter or to women's homes to hang out and talk (155-156).

She also found the progress of the project, to empower women, slow and hard to measure. Sometimes she felt the work was not enough. "Big deal, big revolutionary deal. Trying to organize nine or ten women in a small, dusty southwest town. Surely the real revolution is elsewhere" (156).

Her relationship with the women who had been battered was also problematic. She discovered that the women expected her to guide the project, make decisions for the group and solve their problems (189). She worked hard to combat this (as she felt in principle there should be more equality), but at the same time maintained control by setting up the project, by trying to ensure that the women came to their meetings, and by acting as facilitator. She was disappointed when the women failed to make a structural analysis of their oppression.

St. Denis found that the Fairbanks Native Association's needs assessment project encountered similar difficulties. People were not sure what to expect from participatory research, and therefore were hesitant to get involved.

Those coordinating the Fairbanks project were also inexperienced, and waffled between taking control and letting go. One participant expressed the feeling of not wanting to bother anyone with the demands of the project (82). There seemed to be a lack of commitment to, or understanding of the project, due to the lack of organization and information before it started (84). One team member said "I had a conceptual understanding of participatory research, but in reality I didn't know how to put it into action" (87).

St. Denis reported that a lot of time was needed for the work (87), a fact that other participatory researchers also stressed (Maguire, 1987). The participatory nature of the work means that a lot of time is spent working collectively, which is more time-consuming than the hierarchical approach in which one person delegates responsibility. In many such situations people simply do not have the time to become involved. Maguire found this to be true for the women with whom she was working: as single mothers with endless domestic work and child-care responsibilities, they had no extra time on their hands. This situation is aggravated for women and men in a community lacking the time-saving devices that we have (i.e. washing machines, running water, central heating and processed foods).

Another limitation to participatory research is the

money needed to finance the project. People need a source of income in order to support themselves during the completion of the work. Maguire mentioned that she had to rely on the financial support of her husband during her project. Other scholars used their position at a university in order to finance their living expenses (Small, 1988). Who controls the project budget and who has access to it is important (St. Denis, 83). This will have repercussions for peoples' participation in the project. It will indicate to them how genuine the work is, and who will benefit ultimately from the project.

In the literature on participatory research there is a definite need for more discussion on funding. Most of the documents I read demonstrated a failure to address this issue. What the reader needs to know is: who funded the project?.

The issue of funders is linked to the issue of facilitation. We need to know who initiates the project and who directs it. Will an outside researcher or institution introduce the idea for participatory research to the community? Or will the decision come from within? It is also important to note who facilitates the work? Will it be an outsider? Will she be from a different culture, class or colour than those in the target group? These questions about power must be considered when determining how the work will proceed and succeed. Yet each project varies in the

degree to which outsiders are involved and in what capacity.

St. Denis noted that her being a Native woman did not automatically grant her entry to the community under study. She was from a different nation, a different region and, most importantly, she was an academic. How much more difficult would it then be for a white middle-class urban scholar to conduct participatory research with Aboriginal peoples who experience racism, poverty and colonialism?

Maria-Ines Arratia (1991) also calls for a consideration of the relationships underlying the research. Her work assesses participatory research projects conducted by her colleagues in Chile. She found that the objectives of the planners differed from those of the community members, and notes that these differences have not been explored: "Apparently, everyone performed their expected roles and nobody complained. On the surface, things were going just fine" (6). Her studies showed that "the 'receiving' population" was most impressed with research projects that helped increase their household incomes. More abstract objectives, such as fostering cultural revival, on the other hand, were seen as senseless. The communities were in general sceptical of the projects. Some even resented outsiders using their community to gain funding on their behalf⁴¹ (5).

⁴¹ What criterion Arratia used to determine if the work was participatory or not is unclear. Her evaluations focus on research done by her colleagues who entered the communities as outsiders.

Like St. Denis, Arratia found that:

'the recipients' could not understand this new approach, worst still, 'the deliverers' did not fully understand it either! In their idea of what participatory methods were, they only involved themselves in the process marginally. They knew these methods were an 'up and coming' trend for those of the 'correct' political persuasion (6-7).

Several of the recommendations resulting from Arratia's work were similar to those stated by St. Denis. She found that in order for participatory research to work, the aims of the project had to be clear, and it was required that the participation of the community be gained. In addition, Arratia discovered that it was important to acknowledge the power differences that existed between those who 'delivered' the research and those in the community. This difference was less prevalent in St. Denis's project, as those carrying out the work originated in the community⁴². There must, according to Arratia, be both a commitment and a plan to change the hierarchical relationship that usually exists between the researcher and the researched. This must be accompanied by a willingness to abandon oppressive paradigms:

As anthropologists, we are usually prepared to sacrifice our daily comforts when 'in the field.' But are we equally willing to sacrifice some of our categories of analysis, accept those we are given by our counterpart and even develop new ones that are more relevant to the specific situation? (8).

⁴² St. Denis's primary aim was to evaluate the participatory research, not do participatory research.

Arratia also noted the importance of recognizing communication problems that stem from language and cultural differences⁴³. Another question to be considered is: what happens once "we leave the field?" (8). How will the process and the results effect the community?

Her conclusion of participatory research is that it:

is not an easy route to follow, that this is not just another type of method that we can use or not use. This is the start of a new kind of interaction. Thus, it requires a great deal of planning, depth and length of involvement. And in the end, much will depend on the context of your own research (9).

One of the goals of participatory research is that of education. Ideally, everyone involved in a participatory project will be educated to an equal degree (Tandon, 1988). However, the nature of the research process, because it arises from institutions with power and prestige, is such that those initiating the project tend to perceive themselves as more knowledgeable than those they work with.

Participatory researchers claim that their methodology enables the teacher and student to learn from each other. Freire (1990) argues communication can be achieved only through a dialogue between the two, not through a monologue wherein the teacher/researcher pontificates to the students/community (81). A closer examination of this supposed dialogue, however, reveals that while both may

⁴³ In Chile, for example, we may assume that the academics spoke Spanish while the community members spoke their native languages.

speak, it remains assumed that 'the students' are to be
 'helped' by this exercise. Freire's use of the words
 'teacher' and 'leader' hint at the supposed assistance that
 'outsiders' are expected to provide the oppressed:

Teachers and students (leadership and people), co-
 intent on reality, are both Subjects, not only in the
 task of unveiling that reality, and thereby coming to
 know it critically, but in the task of re-creating that
 knowledge (56).

Freire believes that the oppressed do not realize they are
 oppressed, and are, due to its familiarity, often
 comfortable with their existence:

The oppressed, having internalized the image of
 the oppressor and adopted his guidelines, are
 fearful of freedom. Freedom would require them to
 reject this image and replace it with autonomy and
 responsibility (31).

He says that it is only when teachers and revolutionary
 leaders work with people that they are able to understand
 their oppression.

Maguire says that given the 'tools' and "opportunity to
 learn about and understand their own reality" (39) 'ordinary
 people' are "capable of critical reflection and analysis (my
 italics)" (38).

But at the same time participatory research is supposed
 to be a partnership: "We both know something; neither of us
 knows everything. Working together we will both know more,
 and we will both learn more about how to know (author's
 italics)" (37-38).

This perspective contrasts with the image painted by

Maguire's review of work done by Tandon (1981) and Ellis (1983) in which the oppressed are perceived as ignorant:

In a vicious cycle, people do often lack the information, skills, and experience to critically understand and analyze the social structures and relations which shape their powerlessness. Their lack of information and preoccupation with daily survival interferes with their understanding of how power structures work, and affect their lives. Therefore, the oppressed often share their oppressors' viewpoint, blaming themselves for their own poverty and powerlessness (37).

She cites Tandon as reporting that 'the poor farmer' believes his poverty is a result of "... his own stupidity, ignorance, (and) incompetence" (37).

In her own work, Maguire felt pressure to 'educate' the women she was working with (and researching) about the 'underlying causes' of their problems. She was disappointed that the women did not come to realize that they were oppressed by 'capitalism, patriarchy, or racism' (188).

Yet many people are aware that the conditions they live under are not of their own doing, but are rather a result of 'the rich', 'the government' 'management', 'men', 'whites' etc.. ⁴⁴ When the 'worldview' presented by those in power strike people as 'questionable' and 'uncomfortable', they are less likely to accept it as truth ⁴⁵ (195). 'Ordinary

⁴⁴ See Emily Martin's chapter on class and resistance in *Woman in the Body* for a summary and analysis of consciousness and resistance among the oppressed.

⁴⁵ Martin states that women are less likely to accept aspects of science, created by men, that contradict their bodily experience (195).

people' realize they are oppressed and resist in various ways,⁴⁶ but also realize that efforts to 'overthrow' oppressors can lead to more (or at least a continuation of) hardship and suffering.⁴⁷ It seems presumptuous then to assume that most people need to be conscientized.⁴⁸

My Efforts in Participatory Research

The Innu people are an example of a group that have no need of conscientization. With little assistance from outsiders, they successfully stopped the building of a NATO base in Goose Bay, Labrador. The Innu continue to fight for their rights with the use of educational tours, protests, civil disobedience and blockades. These efforts bring public attention to their causes and show outsiders that they are unwilling to be exploited or oppressed. They are interested in gaining control over their lives and their land in order to provide a better life for themselves and their children.

It was my hope before entering the field that any work I did with the Innu people would assist in the endeavour.

⁴⁶ Martin for example describes women's efforts to resist that include non-action and sabotage (183-188).

⁴⁷ Such was the case in Nicaragua, Cuba and the Soviet Union where people suffered economic hardships and even military threats for their efforts to break-free of Western domination.

⁴⁸ A term used by Friere (1990) to mean "learning to perceive social, political, and economic contradictions, and to take action against the oppressive elements of reality" (19).

Whether it did or not is unclear.

As mentioned in the previous chapter, I had realized that the thesis topic of prostitution and militarism would be unacceptable for my work with the Innu before entering the field. As an alternative, I became interested in the work of people who were doing participatory research (Mies, 1983; IDRC, 1988).

My limited reading of this approach to research, before I went to Labrador, led me to understand that I should give full control of the process to the Innu: what would be researched, how the work would be done, and what would be done with the results. I decided that with this approach and my lack of a topic would make me acceptable to the community. How could they refuse an offer to have research of their choice done at no cost?

At that stage I believed that I could enter the community without upsetting peoples' lives and without being a burden, to offer myself as a free spirit who would examine whatever they wanted me to. I considered myself a blank slate in terms of research agenda.

This picture contradicted the discussions I had with my advisors before I left the university about possible research agendas. For one, I came to realize that because I was a feminist I was interested in working with women on women's issues. Also I was considering research on the Sheshatshit Women's Group to see how they organized their

protests or research on the effects of NATO.

My choice of possible topics was in all likelihood affected not only by the feminist literature I had read, but also by the literature I had not read. Before going to Labrador, and after my return, the classic anthropological works on the Innu were not on my reading list. I was unfamiliar with the work of the big-name anthropologists who worked with the Innu (then known as the Naskapi-Montagnais) such as the late Eleanor Leacock, Frank Speck, and Georg Henrikson.

Though my opinion may change, I viewed my continued ignorance of the research interests and perspectives as an asset. By avoiding this literature I was more able to deal with the Innu as a contemporary people with contemporary issues, rather than as relics of the past. The early studies of the Innu that I had read struck me as: 1) racist - it described the people as primitive; 2) exploitative - their culture was studied to prove or disprove academic theories on for example kinship; and 3) irrelevant - some anthropologists ignored the damaging effects that colonization was having on the Innu people.

In contrast, and with I might add a self-righteous attitude, I **decided** that I would let the Innu select the topic to be researched (yet I was uncomfortable at this point with my lack of a topic). I had not contacted the Innu, and, even for several months after I had contacted

them, did not know what they wanted me to research. An M.A. student without a research topic is inconceivable!

As mentioned earlier, when I finally made contact with the community, there was an absence of interest in the topics I suggested but no others were offered in their place. Instead, I was sent to an Innu camp.

Once back in the community I was determined to begin my research. No one had told me what to look for or analyze while in the country and I failed to see its connection to my work. Just as my ignorance of earlier studies of the Innu worked to my advantage, I believe that my lack of a research agenda in the country also benefitted me. In the camp I did not need to take copious notes or ask endless questions. Instead, I tried to help and to fit in. This experience also provided the people in the camp with an opportunity to get to know who I was by asking me questions and watching what I did.

It had been arranged that when I returned to the community I would stay in the Roman Catholic sister's house. This positioning had an effect on my research that first summer in that I was separated physically and socially from the Innu.

After the country I found it very difficult being in the community particularly the first few days, although for several reasons this feeling persisted throughout the summer. Firstly, I missed the people in the camp. Both

physically and emotionally we were much closer living in tents and working together to support ourselves. In the community I felt happiest when I was visiting the family with which I lived in the country, but even then the feeling of closeness was not matched to that of the country as people were either coming, going or entirely absent from the house.

Secondly, I was uncomfortable being in a community of strangers where my presence was made conspicuous by my whiteness and the small size of the community. I felt extremely shy and self-conscious. I did not like to take walks through the village for fear that everyone was staring at me.

Part of my discomfort stemmed from my lack of justification for being in the community. Why was I there? What was my purpose? Not only did I not know what I was researching, but I was also beginning to doubt the validity of my role as a researcher. It did not seem to fit with the lifestyle or concerns of those I had met.

As a result of these factors I hid in the nun's house for a few days hoping that Rose would come over and tell me what I should be doing. When she failed to appear I phoned her. She told me she would come over but did not for another few days. By this time I had decided to do interviews and had met with two members of the community interested in working with me as interpreters. All I needed

was a topic.

When Rose eventually came over, she suggested that I (we) speak to the elders about the problems in the community. She was interested in having me explore the differences between life in the country, which was peaceful, productive and healthy, and life in the community, which was riddled with social, economic and health problems.

She approved my decision to work with Anne Riche and Jack Penashue as interviewers/translators/interpreters and my decision to formulate questions to be administered and recorded on tape. However, I did not give her much of a choice as to how the work would proceed. I had already made the decision to work in this way, rather than ask her how she would like me to do the research. Perhaps she did not really want me to do any research. I was desperate for a topic as I had only one more month in the community. I needed a direction and purpose if I was to prevent myself from sinking into a depression.

So like a good boss, I requested a meeting with the two interviewers so we could set an agenda and design our research. I felt I should not include Rose in this work as it was clear to me that she was a very busy woman. In addition to being a mother of four and with household responsibilities, she was employed by Social Services, and was very active in community concerns.

I felt that the work I was doing was unimportant:

designed by Rose only to keep me occupied and happy. It was not a project that she herself or other community members would have done. Looking back on the work I believe it was the design that was problematic, not the topic. Why was a strange young woman (most people thought I was a teenager), from a different culture (ignorant of Innu ways) a different country (not Nitassinan and certainly not rural), and different class (never having to get social assistance) collecting information about Innu lives?⁴⁹

I tried to lessen my discomfort with the project and justify my work by telling everyone involved that the material we collected would be housed in the community resource centre and not used by me as data for my thesis.⁵⁰

I also made an effort to make my work with Jack and Anne collective, while at the same time I hoped to execute one of the tenets of participatory research: education. I felt that I would contribute to the community by teaching Jack and Anne how to conduct community research. Then they would have no further need of outside researchers and could instead do the work themselves. (An obviously naive objective.)

The difficulty with this belief was that to do work

⁴⁹ There are many reasons: one being the need for a thesis. The other sequence of events that led to my work in Sheshatshit were explained in the previous chapter.

⁵⁰ The reader should note that I do not discuss the findings of our interviews in this thesis.

acceptable to funding agencies requiring research certain techniques had to be learnt from the academy. These skills could not be learnt in a month. They would also face difficulties doing the work themselves because of the belief that they would be contaminating the data with their biases. There could also be resistance to self-produced community research as a result of racist assumptions about their inability to do 'scientific' work.

There was a problem of my deciding to 'teach' them. I outlined my plan to work with the interpreters to design a schedule of questions. By this time I had also decided that in order to address the issue that Rose suggested that we should we would need to ask people specific questions.

However, Anne informed me that she would be unable to participate in the 'participatory' preparatory aspect of our work. She had too many other commitments being a mother and working at a translating job. She was, however, interested in doing the interviews. Jack agreed to work with me to design the questionnaire but stated that he would interview people without me. Due to their responses, I felt I was losing control, my reason for being in the community was slipping away. I wanted desperately to interact with community members, but if Jack and Anne were going to do the interviews on their own, and I continued to live with the nun, the only opportunity I had to meet people was at church. I am unfortunately an atheist (a fact I did not

publicize).

Another problem with the work was the issue of salary. I had only \$600.00 to pay them for their work. I provided these funds myself, as my university funding covered only the cost of my plane ticket. The interviewers fixed their rate per hour. And then to determine how many people they would interview we divided \$600.00 by the time each interview would take to record and translate. Our meeting then adjourned until Jack and I would complete the construction of the questionnaire.

The three of us never did meet again, and I failed to work with Jack to formulate the questions for the questionnaire. I could not figure out how to work with him to get the type of questions I wanted. My training in positivism set in as I went about formulating a questionnaire that would get at 'the truth'. I wanted to ensure that people addressed the issue we were interested in, but at the start I also saw the work as an invaluable opportunity to collect little histories from the Innu elders. I was not aware that other scholars had been collecting the experiences of the Innu and hence feared that when the elders died, their history would die with them.

These goals were too ambitious. I had hoped to combine the two agendas by asking people about their year-long existence in the country as young people to compare with contemporary life in the community. After writing questions

aimed at acquiring this information, I showed the questionnaire to a few individuals in the community. They told me that the questions were too vague and too all-encompassing, more of a focus was needed.

Anne tried the schedule of questions I wrote on her husband and Jack on his wife. Then I interviewed Jack. A few points became clear. We had to delete some of the questions and be more specific in our wording of the others. The result was a strict regimen of questions.

I also found we needed two questionnaires, one for young and middle-aged people and one for older community members. Although Rose had suggested that we interview elders, I had expanded this to include younger people in response to discussions with two community leaders who thought it was also important to hear what younger people had to say as well. I decided that we should get a cross-section of ages, and directed the interpreters to interview representatives from four different age groups. I also told them Jack should not interview women, only Anne should, with the hope that women in the community would be more comfortable speaking to a woman and therefore open up more during the interview.

Jack changed his mind about my not accompanying him to the interviews. I went with him perhaps because a friendship had developed between us or to ensure that the questions were being administered 'as they should be'. In

the beginning Jack would check with me to see if he was doing the work correctly. He also asked me at the end of the interviews⁵¹ with the elders if I had any other questions for them. I grabbed at this opportunity to 'collect data'. I was worried that I would be returning to the university shortly with nothing to show for my visit, since I had told people I would not be using the interviews for my thesis, but would instead house them in the resource centre⁵².

Only as I write this do I realize that I was not being honest. What I meant when I told them I would not use the interviews was that I would not use direct quotes from the interviews to produce an ethnography on their culture. I did have Jack and Anne translate the tapes into English only realizing in retrospect that had I really intended the interviews to be solely for the use of the community there was no real reason for them to be translated into English (their first language is Innu-eimun).

I justified the translations (to myself) by stating that I would compile the information into a written report for the community. But who would read it? It would have to be translated back into Innu for the older people to read it. This plan gave me a sense of purpose and a feeling of

⁵¹ All done in Innu-eimun.

⁵² Yes, there is a contradiction here. I put it down to the pressure (or the brainwashing) of academia.

control over the project. It was my baby - I alone would produce the final document⁵³. The translations also allowed me to understand how people were responding to the questions, and although I claimed I would not use the information in my thesis, it is stored in my memory (and my closet - I made copies of the tapes) for future use. Their interviews were to help me carry out more informed work on the Innu at a later date.

Because I feared returning to the university without 'data', and when Jack gave me the chance to talk to the elders, I asked them about garbage. I had decided that I should 'at least get an article' out of my experience with the Innu, as plans for a book did not seem possible⁵⁴. Searching for an appropriate topic to write on, I choose garbage. I planned to link the way the Innu treated garbage contemporaneously to the way they did in the past, as a means of demonstrating the continuity of their culture, their respect for the environment and an aspect of their spirituality.

When I first arrived in the community, the R.C. nun, who drove me in, pointed to the garbage on scaffolds and the roofs of peoples houses, and told me it was placed in this way in line with traditional religious beliefs that what one

⁵³ Not very participatory I acknowledge.

⁵⁴ At this stage I did not know what my thesis topic would be on.

2

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animal spirits gave to the Innu should be returned. When in the country hunting with Jack and his brother Bart, I watched them stick their paper garbage on bush branches rather than dropping them on the ground. I was sure I was onto something spiritually significant, and hoped my article would be of interest to anthropologists of 'traditional' culture, and environmentalists who played on the image of Native peoples as 'natural' environmentalists.

My efforts to get this information were unsuccessful. I assumed it was either not something that could be answered in a few minutes at the end of an interview, or it was something the elders chose not to reveal to a white woman. I have since learned that the elders are protective of their traditional religion, but I suspect it may also have been that they did not know what I was talking about. Months later Jack laughed and told me that people put their garbage up high to keep the dogs away from it.

I also wanted to determine the nature of relations between men and women, but was no more successful. I was interested in comparing the status of Innu women in former times with that of 1990 in order to get published in a feminist journal. At the end of the questionnaire for the elders, I tried to get them to address this issue by hinting at what I was looking for. I asked them to tell us about how things had changed from when they were young in terms of, for example, the role of women. But they missed my point

and failed to give me information for a feminist analysis of the status of women as I had hoped they would. The men failed to show their sexism by saying that women no longer knew their 'proper place.' Nor did the women complain about how oppressed they now were by men. Their responses showed instead concern with community problems which affected women, men and children. In fact, I found out after the interviews were done that the women that Anne interviewed complained to her that the questions were too structured and focused too much on the individual, not allowing them to talk about their families.

Perhaps one of the problems with our project was its size. Because I felt I could only afford to pay two interviewers, and I believed that others in the community were either too busy to participate in the project or not interested, I had a lot of control over the work. Had more people been involved, and particularly people who were not 'working' for me, our approach would have been different.

Perhaps more people would have liked to be involved, but I failed to ask. This was partly because I was new to the community and did not know who to ask or who would be interested. However, it also stemmed from my racist assumption that the Innu would not have the ability to be useful participants. As mentioned earlier, I also decided that people lacked the time and the money to participate in a project of this nature. However, the response we received

in terms of people being interviewed, and the following summer (to be explained next chapter) of women taking control of the work, showed otherwise.

However, due to the smallness of the project and the extensive control I exercised over the questionnaire and its administration, it cannot be seen as participatory research. But there were participatory aspects to the work. For example, I was better equipped to address Rose's topic after my trip to the country, which originated at Rose's suggestion.⁵⁵ I believe people felt good about the fact that I went to the country. It was like I went through an initiation process, and it was important that I experienced a life in which they were in control, not the government, and in which they had a history and a sense of pride. Some aspects of the project were participatory. Rose chose the topic, making it more relevant than one chosen by an academic; the interviewers chose to work with me and then decided for themselves, with whom, when and where they would do the interviews (although Jack did ask for my suggestions about who we should see); people were told the research was being done for the community and was not intended for me. The result was that many people⁵⁶ participated in the

⁵⁵ Although it was only the following summer that I experienced first-hand what the Innu meant by 'problems in the community' The first summer by being sheltered in the nun's house I avoided these problems.

⁵⁶ Approximately sixteen.

project willingly and without remuneration.

I assume that it was not just pressure to be polite that motivated their participation, but rather the steps that I took to give control of the research to the Innu.

Unfortunately our understanding of what participatory research entailed was limited. I had neither read enough about the approach nor consulted with people who had done it before leaving for the field. As far as I knew no one at my university had conducted participatory research. It is only as a result of presenting papers on my efforts with participatory research that I have met people who conduct research of this sort.

My work also encountered difficulties due to a lack of clear guidelines on how to do participatory research. Even 'the experts' are not clear on how it is done. The situations and circumstances in which the work is done vary, as do the goals and objectives of those involved. The result is that all of the reports that I have read on participatory research have very different stories to tell - different approaches and methods.

Another problem with participatory research is its educational aspect. How could I pretend to think that I had something to teach Innu people? Though at one point I felt I could teach Jack and Anne to do research (create appropriate questions, administer them and then analyze them), I unfortunately failed to trust in their ability to

understand the research process and their community. As a result I failed to use open-ended questions, or to suggest that the interviewers either formulate the questions or 'wing it'. I should have taken more guidance from them and other community members, and asked for more direction and support, or, better yet, stepped back more and let them take control. The following summer I was able to apply these lessons.

In the country I had abandoned the role of researcher except when I wrote in my diary. In the community I returned to it. Due to my academic training, I perceived myself as 'the expert', and combined with my racism and classism, I assumed the Innu would not do the work 'right'. This is ironic considering it was supposed to be their work.

My resistance to change in the traditional power dynamics between researcher and researched was evidenced when I balked as Jack and Anne told me they would be doing the interviews without me. To my credit, or perhaps due to pride, I did not resist their decisions. I had after all continually told people that I wanted them to control the project.

How useful was the project? A year ago I questioned its validity when I was told that half of the tapes we made were misplaced for six months, and yet no one seemed to miss them. As well, I did not get translations for all the interviews and failed to write a report from the information

I did get. Once I returned to school, my priorities changed from producing a document for the community to producing material for publication.

In retrospect I think Jack and I should have announced our work in the community newsletter, explaining what we were doing, and why and where the data could be obtained. It is likely that most people in Sheshatshit do not know these tapes exist. We could also have used the radio station to inform people about our work.

Despite the limitations of the project, our work provided an opportunity for those who were interviewed to voice their perceptions about the problems in the community. As well as providing a voice, our work may have been educational for those who were present during the interviews: the children (both young and old) of those interviewed, the interviewers, and myself. During the course of our work, we heard common concerns and solutions expressed by those interviewed.

In a further effort to give control of the research to the people, I realized I should provide those interviewed with a copy, on tape, of their interview. As well, the material that I wrote as a result of my experience that summer was sent back to several people in the community (primarily to Rose) before I released it to my professors or for publication.

And, if our work did not conscientize the Innu, it

certainly conscientized me. I learned that the Innu are organized, articulate and strong. They have in the past few years made incredible efforts to regain control over their lives. They are continuing these efforts, and have met with many successes. The battle is however by no means over. There exists in fact an ongoing struggle due to the restrictions, impositions and violations imposed upon them from outside: problems which have affected relations within the community are now making matters worse for everyone.

I learned through my research with the Innu how little I know. Despite the status and prestige I enjoy as a Master's student in my world, my scholarly pursuits did not impress the Innu. In fact, several people expressed to me that they considered them useless. My university training did not help me pluck a duck, chop wood, or drive a ski-doo. In Academia I did not learn to listen without giving advice, or how to work on a project collectively. Instead, the university instills in us an attitude of competitiveness and an artificial belief in our own expertise. The ivory tower fails to value the education that the Innu receive in the country.

So my first summer with the Innu showed me there was a lot I did not know. However, because they did not overwhelm me with new activities, and I did many things that my friends in Toronto had never done, I was overconfident about what I did know with respect to the Innu way of life. Only

during my later visits did I come to understand how little I knew. My skill level in the country, and even the community, was that of a child's (sometimes even below that of a child's). My language ability, for instance, compares to that of a two year old.

In terms of participatory research, I came to realize the pitfalls of my work once I returned to the university. I began to analyze my research efforts by writing papers on my methodology, and by reading more on what specifically participatory research involves. I knew then that the work I had done had not been 'participatory' enough. My decision to write a rigid schedule of questions on my own was one problem. My administering the salaries of the interviewers was also inappropriate, as it gave me too much authority. My awareness of these problems and my additional reading on the topic was insufficient, as is illustrated by the mistakes I made the summer of 1991, in Sheshatshit.

A few months before returning to the community for a second term of 'research', I made a two week visit to Sheshatshit for social reasons. During this time I visited people, talked to them, had fun with them, and spent a couple of days in the bush with no academic agenda. I was visiting friends rather than conducting research. I had no tape recorder, no notes to take and no questions to ask. I found I was more comfortable with this role, and I believe the Innu people were as well. This feeling contrasted with

the sense of uneasiness I felt (and still do) as I wrote about the Innu people. Problems stemmed from the fact that I was presenting them as a homogeneous group, and as objects in my study of methodology, rather than as different individuals and/or families. I have yet to reconcile this sense of discomfort, which is a result of my relationship with the Innu people, and my desire to continue in academia to seek 'knowledge', status, and security.

It was this desire (as well as that to have my second summer in Labrador paid for) that led me to apply for the Northern Science Training Grant (NSTG). Although I had had trouble in the past with accepting money from DIAND, I was assured that the NSTG came indirectly enough from DIAND to make it 'safe'. Hence I would not be selling my soul (and kissing the support of the Innu good-bye)⁵⁷ to the devil. So I applied.

The challenge then for me was to make my work acceptable to what I perceived as the conservative body of scholars that would access my application. I felt there would be little support for an 'alternative' research approach that sought to give control of the project to those who were traditionally researched. Even worse, I once again did not know what the Innu wanted me to research. I had asked several people what they wanted researched next (not if they wanted anything), and had suggested several topics,

⁵⁷ They always asked were I got my money.

but had received no response. How could I write a research proposal if I had no topic? I also figured the NSTG committee would be interested in work that added to the body of knowledge on Aboriginal peoples (i.e. that which studied them in an empirical manner). Hence, in addition to exploring appropriate methodologies, I wrote that I was going to continue my work: "**documenting** their lives in the country and the difficulties they face in the community.... (to) further my **investigation** into Innu lifestyles (emphasis added)." I also told these potential funders that I would be using participant observation (along with participatory and feminist research) to accomplish this task. This was so despite my aversion to participant observation which I perceived to be a tool for objectifying people as people were observed to gather data. Finally, I claimed that my advisor, rather than the Innu, would guide my work.

This approach was obviously successful. I got \$4000.00 (\$1000.00 more than I had asked for) to continue my work with the Innu. I then began thinking about my research topic for the following summer. I decided my project would be to inquire about peoples' views of my research the year before. In other words, I was interested in getting their analysis of my attempts to do participatory research. By doing this I would be able to add 'authentic' Aboriginal discourse to my thesis and papers on methodology with Aboriginal peoples.

This research topic was chosen as I was encouraged to objectify the Innu. With a return to academia and the accompanying immersion in academic interests, as well as a successful publication rate on my work with the Innu, I saw the Innu as subject matter. A shift in my thinking and my values occurred such that I was no longer asking the Innu what they wanted me to do.

As touched upon last chapter, when I told people in the community about my successful funding application, and my plans to continue my work from the year before, they were disappointed. They had hoped that I had finished 'my work' and wanted to return without a research agenda.

This response served to remind me of my original ethical aim: to work for the Innu not on them. Their reaction led me to abandon these plans, and I was left with funding but no topic. A little later, when Rose Gregoire and her sister Elizabeth Penashue (with whom I stayed in the country) were in Toronto, I suggested an idea I had been considering for some time. I had not brought it up earlier, as I felt the project would take the expertise, time and funding of a Ph.D. student. However, because I knew Rose and Elizabeth well, and had a lot of respect for them, I wanted to collect their life histories and have them published. I found their lives fascinating, and was sure there would be a market for their stories due their famous protests against NATO, their being Aboriginal women, and

their concern with the environment. As well, I thought Rose and Elizabeth would be interested in having their story told in order to increase public awareness of their situation and rally support for their cause.

They responded with interest, and I was satisfied that I had a project on which I could work during the coming summer, something that would be useful to the community, but would also address: my academic interest in feminism and an Aboriginal culture, and hope of being published.

A More Participatory Approach

When I returned to the community for a second summer, I stayed this time with Rose and her family rather than in the nun's house. After a few days, I brought up the topic of life histories. Rose was still interested in the idea, but felt that it was important the book contain the stories of several women in the community rather than just two.

I had intended to have the two stories complement each other, since Rose had spent most of her life in the community, while Elizabeth grew up in the country and continues to spend half the year there. However, having learned my lesson the previous summer, I was open and interested in following their suggestions.

Once again I emphasized that it would be their book not mine. The book was to contain their words, I would merely be the editor. I did not want to produce a book that gave

background and analysis to the Innu. I had in mind instead to produce something similar to Enough is Enough. I also intended that the women themselves do the editing, and decided who would be in the book. Any royalties that came from the work were not to go to me, but rather to the women.

This time I was honest about what I would get from the project. The main benefits for me were to be: continued contact with the community and a sense that I was doing something worth-while. I would in addition receive publication credit, as I was to be listed as the editor.

In many ways this project has been a very successful attempt at participatory research. Because of what I had learned from the summer before (to stand back and allow the women to make choices and take responsibility) and because I spent a lot of time doing housework for the family I was staying with, the women in the community took on the project with enthusiasm. Also of importance was the topic. Not only would the project get their stories out to the Canadian public, but it was also to be used by Innu students.

The community school had no texts of this sort. By publishing the book, we would provide the young people with Innu literature, history and social studies. This would help the students take pride in their culture through a school system that immersed them in white history and culture.

As the word spread of our project, different women

approached me about participating. Rose and Elizabeth also spoke to women about the work, and arranged for me to meet with them. I would then explain the project and set up the tape recorder so they could tell their story on tape. These tapes were then to be translated from Innu-eimun into English. Although some of the women spoke English, I felt it was important to allow for freer expression for them to be able to speak in their own language when telling their story. This too because I had learned that there were some words (particularly those used in the country) that could not easily be translated.

I met with several women in various locations at times that were convenient for both of us. Sometimes the women met in groups to talk, while other times they were by themselves. I would show them how the machine worked, and then ask them if they wanted me to stay or leave the room. In the beginning, I felt it was important that my presence be felt to some degree, so that they would keep in mind what I at first perceived to be their intended audience: primarily white feminists.⁵⁸

Sometimes they wanted me to stay with them while they spoke (though I do not understand Innu-eimun), and other times they did not.

I knew the project was a success when Rose offered to go and record her sister speaking. As well, I was

⁵⁸ I was hoping to have Women's Press publish the work.

encouraged by a couple of the women to interview other women when they saw that I had been neglecting the work for a few days. My project had clearly become also their project.

In order to prevent the difficulties with money that we had the year before, I arranged with Rose to share in the administration of money to the translators. However, this approach was also problematic, as the money was still coming from my bank account. In the future I will solve this by having a portion of the funding I receive deposited into an account for the Sheshatshit Women's Group to administer to translators.

With the influence and support of the women in the community, our work got off to a good start. Without Rose and Elizabeth, I would not have been able to make the contacts with the women that we did. As with the previous summer, those who spoke were not paid, and the women working with me to organize the work were not paid either. People offered their homes and their vehicles for transportation to and from the recording sessions. The women made arrangements for child-care and we got permission to work in the school and the Innu Nation office where there were fewer distractions. This co-operative effort made our work a community project.

I continue to feel some guilt about writing this thesis and other papers on the Innu. Nevertheless I know that the book we are doing is a worth-while endeavour. Even during

my last visit (March, 1992), one of the women was interested in discussing the project with me. Unfortunately my stay was too short to collect any more stories, although in some ways it is important that I did not work on the project. I feel it is important for me to not restrict my role to that of life history collector. If I become too preoccupied with getting the work done, I will begin harassing people to participate. I am learning to see that when people do not return my calls, or do not keep appointments, they are likely not interested in participating (at least not at that time).

At one point I went to see a women whom Rose suggested should participate in the project. However, we had never met and my timing was inopportune. From this I learned that more liaising was necessary for me to do the work. It might help me to give up more control by having the women collect each others' stories. I should use the funding I receive in the future to purchase more tape recorders and give them to the women to use. They could collect the stories and see to it that the work gets translated. If this happens I will loose a lot of control and my role as organizer would be changed to merely facilitating the publication process.

Just how extensive my role will be in soliciting women to speak and getting a translator is unclear. They might not have the time that I will to do this work. Also in question is my role in the editing process. Much has been

written on the power held by the person who collects⁵⁹ the life history, and how they can distort what is said to meet their interests and values. In the beginning I told the women that I wanted them to decide what would be left in the final work and what would be taken out. However, this may not be appropriate. Again there may be time limitations that would prevent this involvement. They may leave it up to me, or perhaps I will work with a few people who do have time to edit the work. We shall see. Nothing is definite here. Our work does not follow a rigid schedule or format since our project involves many people and is not of primary importance to them. For someone doing a Ph.D, the work would be more important. Although it may one day be this way for me, for instance when doing a year of fieldwork in order to write a dissertation, the women in the community have many more pressing concerns.

Staying on Track

In order to conduct participatory research, a continual effort must be made to work with people, not on them. The first summer I spent with the Innu, I told myself the project should be theirs, but to a large degree I did otherwise. The next summer I kept reminding myself to let

⁵⁹ The power of the translators will not be addressed here except to note that because our project involves many people different family members will likely be reviewing the translated material.

go of control over the work. I made an effort not to worry about doing things 'right' and 'on time'. My efforts to let go were perhaps too successful. We recorded only eight hours of speaking, and translated only two of these. While part of this was due to the busy schedules of the women, some may have been due to a resistance to the work. Our lack of productivity was also due to the other work that I was doing. Because of the responsibilities I had taken on with the family I stayed with, I found I had very little time for other things, and often felt more like visiting or 'going into town' than working on arranging for and attending the life history work.

I had, however, a particularly moving meeting with Kathleen Nuna which refuelled my interests in the project. Her enthusiasm for the work, so that the school could use the book, was contagious. Also one night (after she recorded her story for over an hour in Innu-eimun) she told me parts of her history in English. I was fascinated. It was a lesson for me of hardship and a proud culture. I felt very privileged to have been told it. And I knew then that we were doing the right thing by getting their stories published.

The work is presently at a stand-still. One reason is the difficulties we are having with translating the material as the women are busy and translation is expensive. I had exhausted my funding source of the previous summer before

more than a few hours were translated. It will be interesting to see how our work progresses this coming summer, as I have failed to obtain funding for translation.

On a daily basis I continue to struggle with participatory research, not so much in terms of the life history in which the women are active, but more in terms of my own research: my thesis, the papers I publish and present, and my proposals for future research.

I have found that I am much less conscientious about sending papers to Rose before they are released. There are several reasons for this. Firstly, I have never received comments or criticisms from the Innu people that have read my work. I had expected that when I gave them my work they would go over the information page by page to correct my mistakes and tell me what to delete. This did not happen. The closest I came to this was when I sent a report I did on Innu employment, which I did for one of my professors in exchange for funding, to Peter Armitage (the anthropologist who works for the Innu). Peter critiqued a number of points in my paper, and even sent me material that would help me correct the work. When I showed this same report to Jack Penashue, he found the work interesting and told me it made him think about things he had not before.⁶⁰

⁶⁰ My report critiqued government control over Innu land, structural violence inflicted on their culture in the country. The marginal position that the Innu hold in the economy, reflected in severe unemployment rates, was also examined.

Another problem with sending my work back to the community was that, due to communication difficulties with telephones not working and mail taking weeks, I could not get permission to release my work as fast as I wanted it. I sometimes got no response. During my last visit to Sheshatshit, Rose explained to me that just after she received my last chapter she learned of another incident wherein an outsider who had come to live in the community had abused an Innu person. As well, several books on the Innu by non-Innu people had just been released, leaving her feeling angry about outsiders speaking for the Innu and getting benefits from their endeavours. There were also problems in these books with the way in which people in the community were portrayed.

Another deterrent to my sending my material out to the Innu was the confidence I gained with my position in academia. I had to watch that my allegiance did not shift from the community of Sheshatshit to the university. Unfortunately, the university does not have a watch-dog mechanism to ensure or even encourage us to send our results back to the people we work with. I have found that some professors are unsupportive of my approach on the grounds that they must wait for the document to be approved before they can receive it.

I have not had criticism from the Innu on the things I have written, but find it takes a lot of time to get

permission to release my work. The last time I sent anything to Sheshatshit, I had to visit the community before I received permission for its release. These two points, as well as my immersion in academia, led me to begin considering myself an expert on what type of published material would be acceptable to the Innu. This was dangerous, given that I had no way of knowing. I am not from their culture and enjoy privileges that they do not due to the colour of my skin. My graduate education, my position in a secure socio-economic class, and the speaking and writing skills that a native English speaker has, all make my experience very different from that of those in Sheshatshit.

I also ran into difficulties keeping to a participatory research approach as I prepared a research schedule for the coming years. With plans to do a Ph.D., I prepared research proposals for potential funders and for application to Ph.D. programs in anthropology. Similar to my earlier experiences, I felt it was necessary to make my research agenda acceptable to the institutions to which I was applying. However, I continued with my commitment to the community by wanting to do work that benefited the Innu.

My plan was to continue with the life histories. However, I was also interested in doing the type of consulting work that several outsiders had been doing for the Innu by preparing reports that the community was

required to provide to government for their land claim.

I thought it might be useful for me to prepare a report on one or several of the issues concerning women in the community. At the opening ceremonies to land claim negotiations several women had expressed concern with housing, violence, education and culture. I wanted to work with the women in order to document their needs in these areas. When I suggested this to Rose, however, I received no response.

This lack of direction from the Innu with respect to Ph.D. research made me uneasy. Though I assumed our life history project would continue, I felt it was not enough for a dissertation topic. Yet I could not state I had no research topic nor a selected method as the Innu people had not given me any. They could not foresee what type of research they would want or need several years into the future when I would be doing my fieldwork for a dissertation. I also felt it was essential for my research proposals to have a theoretical framework not made available by participatory research. I therefore created my own research agenda.

I decided that I would draw on the literature I was most familiar with: feminism. Despite the numerous criticisms lodged against feminism by women of colour,⁶¹ I was interested in working in this area. My plan was to work

⁶¹ To be explored in chapter four.

with these criticisms and examine feminism based on my knowledge of and work with the Innu people. I knew that some people in the community were wary of feminism, but thought that a critique of this nature would be beneficial. I would contribute to the women's movement by illustrating how white feminists had made a mistake in focusing on the issues of white, middle-class women. I had in mind to assist in the process of women working together for change by educating white women about the triple oppression that Aboriginal women experience, as well as the strategies they used for change. I hoped also to explore what feminism has to contribute to our understanding of the lives of Aboriginal women.

My plan was to make suggestions for a more appropriate feminist model, one that included the issues of racism, classism and imperialism. I felt that a feminist analysis would help the Innu people. In my SSHRC application (1992) I stated: "For feminist theories to be of use to Innu women they must address this (the country) lifestyle and their struggle to maintain it." I was once again willing to set the Innu people up as objects of research to fulfil my degree requirements. I proposed that:

By examining Innu life in the community and the country, with their concerns and struggles, I will illuminate the failure of much feminist theory to address the issues of Aboriginal peoples still under colonial rule. These models do (however) remind us of the need for access to the means of production, an issue my dissertation will address.

I declared my aims as two-fold. Firstly, to make research more useful to Aboriginal people. It is, however, perhaps more accurate to say my aim was to make research more palatable to enable academics to continue their (our) study of 'other' cultures. Secondly, my stated aim was to make feminism more responsible to Aboriginal women, although a cynic might say my aim was to bring these women into the women's movement. I said:

Ultimately my aim is to make scholarship more responsible to Aboriginal people. Just as my Master's thesis advocates researchers sharing power with those they research, my Ph.D. dissertation will emphasize the need for scholars to work with oppressed people to arrive at appropriate models for change. While feminists have begun to listen to critiques by women of colour and are looking for ways to improve, it is not enough to add others onto models made for white women, nor merely add a chapter on their lives to our books. Instead, we must become aware of our mistakes and seek solutions which will only come from working with women of colour. By describing the lives of Innu women to Canadian women, via the life histories and by discussing feminisms with the Innu, my project will be part of a necessary dialogue between feminists and Aboriginal peoples.

Because of the need to be specific about my course of study, I gave a detailed plan of how I would spend the next five years doing research - this without first discussing these plans and getting approval for them from the Innu. I wrote in my proposal that I would spend the third year of my program "entirely with the Innu". I stated that I would spend the spring and the fall in a camp in the country and the rest of the year in Sheshatshit. Who I would have stayed with, and would they have wanted me around that long,

were questions I tried not to think about as I prepared my agenda. I also stated that I would be doing 24 interviews with Innu "women and men, from varying age groups and with different life experiences" to record issues of concern to them and possible solutions, and then to explore their perception of "feminism and its role, if any, in their community". This is clearly a project of questionable use to the Innu.

To justify my report and inform the Innu of this possible agenda, I sent an open letter to the community by way of Rose Gregoire and Elizabeth Penashue's son, Peter, who is the president of the Innu Nation,⁶² and to Francis Penashue, Elizabeth's husband and chief of Sheshatshit. In it I told the community that although I would continue with the life histories, and was interested in doing research for the community, I needed to address the issue of funding for the work. I told them that if my request for money from SSHRC was successful, we would be able to proceed with our plans to have the life histories translated and published. I went on to explain that there were no "SSHRC police", and if we found that my plan to do a feminist analysis was inappropriate, we could "change the topic later".

However, the question one might ask, and one which

⁶² Formerly called the Naskapi-Montagnais Innu Association.

Nahla Abdo⁶³ did, was: why study the Innu? In my SSHRC application I was honest when I stated that I wanted to have feminism work for the Innu people, I was as a feminist trying to resolve my relationships with the Innu and my work with them. However, I was also using my relationship with the Innu to further my academic career. My relationship with the Innu gave me what every anthropologist wanted - access to, and permission to work in an 'other' community. I felt that the discipline demanded a focus on an 'exotic' group. Margaret Rodman (director of the graduate programme at York University) informed me that working with a people traditionally studied by anthropologists would help my admission to York University. Work with people in urban centres was considered less acceptable by 'the old guard'.

However, my discomfort with studying the Innu resurfaced as I prepared my Ph.D. proposals for applications to York and McMaster Universities. I realized that I knew many women with interesting life histories that should have been recorded and published, but would likely not be, as their 'issues' at the time were not 'fashionable' to study or read about. Individuals I knew in Toronto had waged personal struggles against the poverty, racism and other hardships that accompanied women who immigrate to Canada, or who live with economic hardship as they work to obtain

⁶³ Abdo is a Palestinian feminist scholar at Carleton University.

better lives for themselves and their children. I was also interested (given my own experience working in a shelter) in speaking with women who had been involved in the shelter movement in Ottawa - certainly a struggle worth recording. As a result of this realization, and my discomfort with 'studying' the Innu, I decided to compare the lives of these women I knew to those of Innu women. In this way I hoped to move away from studying the Innu as 'the other' to studying all women.

A month or two later I meet with Peter Penashue when he was in Ottawa on business. I explained to him that I was embarrassed by the first letter I had sent to the community which stated that I would explore how feminist theory related to the Innu. I told him instead that I was going to compare the lives of Innu women to those of women in Toronto and Ottawa. He agreed that my original plan was useless to his community, but went further to say that my alternative plan was "silly". I was distraught.

As an alternative to his disapproval, I suggested the possibility of comparing the lives of other Aboriginal women in Canada to that of Innu women. This suggestion was mentioned to me by Wayne Warry at McMaster University. Peter also saw this plan as ridiculous. He suggested instead that it would be more useful to compare Innu women to women in Palestine or Nicaragua, or even South Africa. He pointed out that women in Toronto and Ottawa enjoyed so

many privileges that Innu women did not.

Initially I rejected Peter's suggestions for two reasons: 1) his dislike of feminism and 2) the difficulty of doing a cross-cultural comparison. I believe that in order to do a good comparison I would need to visit the country of the women I chose to compare to Innu women. A few weeks later I spoke to Jim McDonald at York University. I informed him of my plan to compare Innu women to women in Toronto and Ottawa. When he expressed his reservations with the agenda, I told him of my conversation with Peter. McDonald advised me to do what Peter suggested (a cross-cultural comparison), since Peter is an Innu leader and his suggested comparison implied an analysis of Aboriginal conditions that most Canadians would not expect native people to have.

Whose suggestion then was I to follow for Ph.D. research? I decided that I would ask the Innu women, and in particular Rose Gregoire, what they wanted me to focus on. So during my visit to Sheshatshit in March (1992), I explained my research dilemma to Rose, she listened but gave me no answer. I decided that I would not push her for a research agenda given that, assuming I got into a Ph.D program, I would not be doing the work for another few years anyway.

Then, to my disappointment, I was rejected for Ph.D. funding and program entry. I was shocked, and was not sure

if the rejections were a result of my academic standing, my feminist perspective, or my desire to do participatory research. What I knew for sure, however, was that I felt abandoned by the academic community, but not by the Innu, who were happy I was not going on with my studies. For over a year they had been trying to convince me to 'drop out of school'.

The effect of these rejections on my work is interesting. A part of me feels an even stronger need to make my work acceptable to the academic community in order to gain entry to a program next year. Hence I will continue to present papers and seek publication for my work with the Innu. However, because I was rejected by academia, but not by the Innu (who have interpreted my rejection to mean that I can spend more time with them) my allegiance is now more firmly placed with the Innu. In future proposals for a Ph.D. I will not seek to win the approval of the academics reviewing my application by pretending I will do empirical work but instead state that I will be doing participatory research with the Innu.

The denial of funding for my work has also affected my plans. I had requested funding in the form of a Northern Science Training Grants (NSTG) in order to return to Sheshatshit as part of my participatory research with the Innu. It was essential for me to go over my thesis with the community before it was released. Unfortunately for this

work, I was only granted a fraction of the funding I had requested, and hence will not be able to spend several months in Labrador this coming summer⁶⁴. I had hoped to use university money to fund my stay in Sheshatshit and to pay people to translate the life history tapes. Several of the reasons given for this lack of funding showed a lack of support for participatory research. Firstly, the committee stated that the money I had requested for translating seemed excessive. By denying me money for translating, my work would not be as accessible to those who did not speak English, and our collective efforts to get the life histories done were denied validity. Another reason for the lack of support for our work was the amount of time I had spent on my Master's degree. Ironically, the reason it had taken me extra time to complete the degree was due to the amount of time I had spent with the Innu people, a total of seven months - always seeking to better my relationship with them.

Another irony to the lack of support I received from the NSTG was the expectations it placed on the Innu. Because I was only granted money to fund my transportation I will be unable to pay the Innu for boarding and lodging. An expectation of this sort would put a financial strain on

⁶⁴ After completing this thesis, but before defending it, I returned to Sheshatshit for a month (September, 1992). At this time I brought with me several copies of my thesis for the Innu to review.

families that have little money to begin with. As a result, my return to the community was postponed until I could save enough from my earnings to purchase food, which is quite costly in the north (see previous footnote).

I believe there will be a lot for me to learn from these rejections. I am finding that my work is changing as my ties to academia loosen and I am curious to see how our life history project can proceed without funding. Though I am now committed to work with women in Sheshatshit, I want the project to benefit the entire community. Perhaps a year away from academia will allow me to move away from a focus on feminist research to a more holistic approach to work. It will also be interesting to see if the year away will provide me with the courage and direction needed to write research proposals that are acceptable to the Innu. Or will it accentuate my disillusionment with the university's objectification of those studied and its irrelevance to their lives to the point of my abandoning plans for a Ph.D. Whatever the result I believe my relationship with people in Sheshatshit will benefit from these rejections.

Chapter 4

THE STUDY OF WOMEN: Feminism and Methodologies

Introduction

As mentioned in the previous chapter, my initial contact with the Innu resulted from my doing a Master's in anthropology. As well, the entire time I spent with the Innu has been while enrolled as a student at the university. This has affected my relations with them. It meant that the work I did was informed by the theories and methods that I was studying and it also meant that our day-to-day relations were subject to objectification as I turned my experiences and conversations with them into a thesis.

One agenda that I carry with me where ever I go, and therefore came with me to Labrador, is that of feminism. My feminist tendencies were intensified by my studies at the university and hence permeated my work with the Innu.

I will begin this chapter with a discussion of feminism as I perceive it, and then explore the connection between feminism and anthropology. As well, a feminist critique of social science research in general will be discussed. Then, feminist research will be critiqued from the perspective of women of colour whose interests are overlooked. Possible research alternatives, and the values that accompany them,

will be posited in light of this material. The chapter will end with a description of my experience with feminist research approaches and a forecast of the way in which my work with Innu women will proceed.

By reviewing feminist accounts of how to do research, I will illustrate what I consider appropriate research to be. In the papers I selected for review the authors address the principles needed for 'good' research. Taken collectively their work directs us to work that is more acceptable to those studied. I will argue that an avoidance of objective and value-neutral research is important if the work is to be appropriate. In particular we must work with those we study and do work that benefits them.

For the purposes of this paper I will include my definition of feminism. Although each of the authors reviewed in this chapter have their own understanding of feminism, I will start with my own definition as my critique of their work will be based on how closely it adheres to the principles I advocate.

I arrived at my definition of feminism from my experiences as a woman in a patriarchal (ruled by men), androcentric (male-centred) world, from my experiences in the women's movement, and from my familiarity with feminist scholarship. In particular, my definition has been informed by the more recent critiques of feminism by women of colour and by Patricia Maguire's work in participatory research

(1987).

My definition of feminism describes an ideal, a goal that we should be working together to achieve. I propose my definition simultaneously as a system of beliefs and as an agenda for change.

To me feminism includes an understanding that women experience life in ways different from men and accompanying these differences are experiences of: exploitation (being used or taken advantage of), abuse, oppression (treated unjustly), and restrictions on freedom. Differences between women and men do **not** assume that all women are the same or have common concerns. And, while we as women experience difficulties based on our gender we have ways of coping, resisting, and working for change. Feminism assumes that women are strong. Finally, I will add that feminism is not a stance against men; because of the power dynamics that can exist between women, and the fact that most women live and work **with** men, feminism assumes that both women **and** men need to understand oppression and work for change. There needs to be structural changes throughout the world so that not only sexism but other oppressions, such as racism, imperialism, and classism are eliminated.

Feminist studies, or women's studies as it is sometimes called, assumes the oppression of women and seeks to not only better understand this oppression but also change it (Mascia-Lees et al., 1989). Ideally, feminist theory is

linked with feminist political action in order to bring about change in the world (Flax, 1987).

Feminism and Anthropology

In the beginning of 'the study of man' men were studied by men.⁶⁵ An androcentrism existed in the discipline because men were anthropologists and men were informants (Atkinson, 1982:255). Women were overlooked except as circles in kinship diagrams, or for their exchange value (e.g. Levi-Strauss's offensive theory of the exchange of women). Women were not included in studies of religion, politics, economics or social networks.

Upon the entry of a few female anthropologists to the discipline, such as Margaret Mead, the lives of women were no longer ignored. However, their studies only included women to better understand the 'primitive' systems the anthropologists they were studying. The work of these women failed to examine issues of particular concern to women.⁶⁶

When feminist anthropology came into existence in the 1970's women began studying women in their own right. However, the lives of women in **other** societies were the

⁶⁵ Actually 'primitives' were studied by 'men'. See The Invention of Primitive Society by Adam Kuper for an illustration of those studied as sub-human. Also see Malinowski's diary for a sense of the difference between men and 'men'.

⁶⁶ They also failed to examine conditions of oppression arising from imperialism.

focus of these anthropologists. At this time it became common practice to study women in non-industrial societies to understand Western constructs of women. For example, was the status of women equal to that of men before the entry of capital into primitive society or were women always oppressed? Authors such as Atkinson (1982) and Strathern (1987) discuss and critique this subject matter. Feminists of this era failed to see that they were making the women they studied into the new 'other' (Mascia-Lees et al., 1989:33).

More recently, feminists sought to construct new theoretical interpretations of women's lives. With women as the central category of analysis and feminist politics framing the research questions (which change as the movement does), the aim of feminist research in anthropology was to alter the power relations between women and men, and do away with women's oppression. The goal was to understand women's experience⁶⁷ in a hierarchical world (Mascia-Lees et al., 1989).

The decision to study women in other cultures combined with racism to ensure that the topics we chose and the methods we used to study 'other' women were like those of men's anthropology but had the added 'bonus' of perceiving women as victims (Atkinson, 1982:250). It is only as

⁶⁷ Note the singularity of experience here. A discussion of difference and power imbalances **between** women occurs later in this chapter.

feminist studies moves into the study of methodology that we understand how we, as feminist researchers, have perpetuated power imbalances ourselves.

Feminist anthropology, however, remains on the side lines, as a sub-discipline that failed to challenge male models (Strathern, 1987). Studies of women are marginalized while the work of men - who still dominate the discipline numerically (Davies et al., 1992) - continues to address issues that overlook the lives of women (Mascia-Lees et al., 1989:18-9). Ward and Grant (1985) show that male scholars fail to read the work of feminists and continue to generalize the experiences of men to everyone.

Researching Women

Renate Duelli Klein (1983) critiques traditional research methods for an androcentrism which positions men as 'the norm' and female as 'the other'. However, she points out, even when women are studying women the researcher is still considered the expert (90-1). The value placed on this work by institutions and non-natives to the culture is such that value of the knowledge is distorted. Often the researcher will believe that because she has academic training and has spent time in the field she knows those she studies better than they know themselves.

It is this attitude that we see illustrated in Mbilinyi's (1989) work. "I'd have been a Man" is a classic

illustration of the approach described as 'the study of woman' in which the power of the research can easily lead to abuse. In her paper Mbilinyi describes a struggle that took place between herself and Kalindile, the Tanzanian peasant woman whose life history she was collecting. Each had a different analysis of Kalindile's life.

Mbilinyi set out to illustrate the negative effects of colonialism on women. However, Kalindile challenged her assumption that Tanzanian women were better off with a black administration. According to her independence brought the threat that state sanctions would be removed against husbands who beat their wives (219).

Initially Mbilinyi had also decided that Kalindile was furthering women's enslavement through her work with the church. She said Kalindile was teaching the women to be subservient to men. Upon hearing of this, Kalindile was angry with her misinterpretation of a situation that required 'guerilla warfare'. Women needed to combine a strategy of retreat and advance as they lacked the power to engage in direct battles with men. Throughout the project Mbilinyi continued to challenge Kalindile on issues that arose from her life story.

Patai (1991) condemns Mbilinyi for this approach. Any attempts by researchers to manipulate through dialogue, Patai calls "savage social therapy". When we insist that others interpret their lives in the 'correct' way - as we do

- we are acting as through we know their lives better than they do, and we can 'help' them (148).

Because Kalindile argued back and because Mbilinyi was willing to listen to get the story 'right', her initial errors in interpretation were overcome. But was it right for Mbilinyi to challenge Kalindile or interpret her story? Certainly Kalindile was upset by hearing that Mbilinyi wrote that she was advocating 'complacent submission to male rule', a mistake very nearly overlooked. I argue that more respect towards our subjects is needed. Yet Mbilinyi appears to congratulate herself in her paper for working with her subject by arguing with her.

She also implies that in order to arrive at 'the truth' we need to return to the field to verify our work with those we study. This I argue is insufficient. Her methods are such that ultimately she, Mbilinyi, has the power to decide how Kalindile's story is presented. Her position as a white Western university professor gave her the power to argue with a woman from another culture about how her life should be interpreted. This is a power which needs to be diminished.

Oakley (1981), like Mbilinyi, attempted to bridge the gap between the researcher and the researched by developing an intimacy with the women she interviewed. Based on her work with expectant and new mothers, Oakley found the need to abandon the positivistic principles of objectivity and

value-neutrality. She was unable to maintain a 'professional' distance between herself and the women she was interviewing due to their shared life experience and the personal nature of her topic--the transition to motherhood. Oakley mentions that, despite text-book instructions to keep a distance between herself and those she was interviewing, she was unable to respond to the women's question with 'neutral' responses. When asked:

'Which hole does the baby come out of?' 'Does an epidural ever paralyse women?' and 'Why is it dangerous to leave a small baby alone in the house?' (48).

She could not answer with the recommended responses like:

'I guess I haven't thought enough about it to give a good answer right now,' or a head-shaking gesture which suggests 'that's a hard one' (or by) laughing off the request with the remark that 'my job at the moment is to get opinions, not to have them' ... (48).

Responding in this way would have been 'morally indefensible' and would have contravened the feminist agenda of empowering women (41).

Instead Oakley willingly answered the women's questions about birthing and about herself. She became so involved in the process that she developed friendships with several of the women (46).

Oakley describes how Elenore Smith Bowen (a pseudonym) in Return to Laughter described a similar awareness of the contradictions in the research process when women interview women. After her key informant, who had become a friend, became deathly ill and died, Bowen was struck by the

impossibility of remaining detached in order to write about the situation (57). Despite being from another class and culture, Bowen became close enough to the women that she worked with to have her quest for 'scientific' data interfered with.

Oakley, who was studying women much like herself, did not even try to be 'scientific' in her methods. Like Mbilinyi's work, however, the process and the end-product were the same--the extraction of data for scholarly work. Klein (1983) condemns feminist work that is done merely for the sake of knowledge. She says that if our work is to be feminist it must work towards social change (91), not I might add merely a change in our approach to research. If the research we do is to be of benefit to those we study we must do more than answer questions and 'be friends' with those we study or assume that challenging dialogue is fruitful.

Reinharz (1983) advocates the use of non-hierarchical, non-authoritarian, non-manipulative work with one's 'subjects'. Research done should be of concern to both the researched and the researcher and should have some social value. When working with others, she says, it is important to be a 'student' rather than an 'expert'. This can be accomplished by combining participatory research, participant observation and dialogue (178, 181-2). Just how the research topic is arrived at and how those involved work

together is unclear from her article. How much power does the researcher have?

Maria Mies (1983), perhaps the most cited 'alternative' feminist methodologist, describes a radical approach to research. In "Towards a Methodology for Feminist Research," she provides the interested feminist researcher with the specifics of how work should proceed if it is to empower everyone involved in the process. Her work is based on the assumption that research should combine an understanding of class and gender exploitation; an awareness that androcentric research fails to address (118, 123).

Her work calls into question the ability of quantitative researchers to remain value-neutral. She also questions their separation of theory from practice. For Mies the alternative is a new methodology which integrates our common oppression as women with a sharing of research power. Her guidelines include conscious partiality⁶⁸, and a need for research 'from below' so that research is for the oppressed. Theories should be based on their utility in the everyday world of women. Research should conscientize those involved and should include an understanding of women's history and women's lives (121-7).

To assist the reader in the application of these principles Mies ends her paper with a description of how she

⁶⁸ I have difficulties with this concept. It implies an expertise on the part of the outsider, an expertise I critique in chapter three.

applied these guidelines in a participatory research project to start a shelter for battered women in Cologne.

Sandra Harding (1987) in her introduction to Feminism and Methodology, states that because there is no one 'woman's' experience there is no one 'correct' method for research. But at the same time she reminds us that oppressed people want to be liberated, learning 'the truth' about their lives is only relevant to outsiders. Therefore, she says, we should be studying 'up' not 'down' to people⁶⁹ and we should be critical of power sources. It is important, she says, for us to admit our biases and how they effect our work. If, for example, men are to do work on women we need to be wary of what they produce but we need not discard it (7-12).

But what if research by feminists is not done 'up' and it fails to be critical of power sources - is it then 'incorrect'?

In the conclusion to her edited book Harding discusses the inconsistencies between some feminist theories and the claims of black feminists such as Bell Hooks. For instance, feminist empiricism is condemned for its claim to have the truth about women's lives, while failing to acknowledge difference among women. Feminist standpoint epistemology is

⁶⁹ When studying 'down' the researcher assumes an expertise over those he is studying and controls the research process. Studying 'up' involves work at the grassroots level with people.

also problematic for its tendency to homogenize women's experiences. Black women experience conditions very different from those of white women and hence have different issues and different ways of viewing the world. Harding says theories need to acknowledge these differences if they are to be useful (182-9). But how do we apply this understanding in our work? Harding, who is a philosopher not a practitioner, does not tell us. Other writers have suggestions for dealing with difference and racism in scholarship. Buts before solutions are posed we will take a closer look at what the problems are.

Feminism and Racism

In this section, I will show how racism prevents us from doing appropriate research and I will offer solutions for combating this difficulty. It is ironic that the tool some of us use in our fight to improve the lives of others, research, can also be used to perpetuate racist relations. If we are to overcome this paradox we need to acknowledge that advocating feminism does not absolve us from racist relations.

What follows is an illustration of the ways in which feminism, and in particular feminist research, reinforces racism and how we can attempt to address this problem.

Before feminism will address the concerns of all women, it is important to acknowledge the prevalence of racism in

our society since racism and sexism combine to oppress women of colour. Moraga and Anzaldua (1983) say "it is only white women who can 'afford' to remain oblivious to these effects (of racism). The rest of us have had it breathing or bleeding down our necks" (62). I believe, however, that as white women we cannot afford to ignore racism either.

Violence against people of colour, for example, is a serious matter. Structural violence is such that 80% of women of colour⁷⁰ with breast cancer will die from it while most white women are diagnosed early enough to be treated (Lorde, 1983b:97). We also need to acknowledge the historical relations we have had with people of colour. Cameron (1983) reminds us that white people kill, maim, beat-up, insult, rape and cheat Indians. Unlike the experiences of most Euro-Canadians, she mentions the endless numbers of funerals she had been to as death touches her people repeatedly as a result of the structural violence and physical violence inflicted on them (47).⁷¹

Wolverton (1990) says we learn from an early age to perpetuate a status quo that gives economic, legal, political, educational and social power to whites, while at the same time robbing people of colour of their natural

⁷⁰ Lorde does not specify in which country.

⁷¹ Numerous studies have been done on the effects of the absence of services in Native communities and the denial of self-determination. Suicides, alcohol related deaths, family violence and poor sanitation and health care all work to not only demoralize but even kill Native people.

resources, culture and the right to self-determination (187-8).

Unfortunately we have also been taught not to talk about racism and especially not to protest against it. We deny it, "I'm not a racist", we hide it, or we feel guilty and helpless, "I can't do anything". She also, however, says it is no longer acceptable for us to 'just be' for as we are we condone and participate in racist institutions (McKenna, 1991:188).

Furthermore we need to explore our part in the maintenance of this racist society. It is important for us, as feminist scholars, to acknowledge our power and our errors because, despite the fact that feminist writing is marginalized, it is part of the global hegemony of Western scholarship and hence does have political clout. The distortions that Western white feminists are creating hinder political practices and limit coalition building between white women and women of colour, and between poor women and those better off. Nor is it enough to work on racism within the women's movement as feminists make up only a small percentage of society. Bigger change throughout society needs to take place (51-53).

Moraga and Anzaldua (1983) agree that we have the power and economic privilege to implant racist ideologies. Our position of privilege within the university allows us access to the mass media, research money, publishing houses, and

public forums (Hooks, 1984; Goetz, 1991). We have enormous power that we can use to challenge the status quo or condone it. If used incorrectly we reinforce existing perceptions of 'others'. When used effectively the tools of academia can foster societal change. Therefore, whether consciously or not, in our day to day relations with people, both in the field and at home, we choose to either fight against racism or accept it.

The university has several effective techniques for silencing others around issues of resistance. In this institution, as in others, we learn to communicate within the terms of the dominant. We learn to regulate communication and suppress the challenges of women and others. Deliberation gets used as a vehicle to regulate conflict. Often, says McKenna (1991), the job of the academic is to act as a go-between between real people and 'the authorized objectified discourse' - to pass on the rules (117, 122).

Furthermore, the discursive (philosophical reasoning) is not a conversation between equals but more like one between the colonizer and the colonized. Our use of elitist language in the university is a tool to dominate and a way of silencing 'other' women. 'Normal' discourse in academia creates silences and gaps which mean that non-white women disappear and racism becomes our 'normal' way of seeing. When complaints are lodged against institutional racism its

continuation is justified by the principle of academic freedom (114-116, 127).

Hooks (1988) states that for feminist scholarship to acknowledge its ethical issues it must address the presence of power imbalances which reproduce oppressor\oppressed, exploiter\exploited relationships between the subject and objects of research. We get to choose how our research will be done and to what end. And as researchers we get to define reality for those we study, as we saw Mbilinyi did. We can describe people as abnormal, crazy, or primitive if we choose and we have the power to write their history. Or we can choose to ignore them entirely. Patai (1991) cites E.B. White who says:

I rise in the morning, torn between a desire to improve (or save) the world and a desire to enjoy (or savor) the world. This makes it hard to plan the day (139).

By ignoring our power and privilege we participate in the oppression of others in several ways. One way is through the methods we employ and the philosophy behind them. Our methodology determines the purpose of our work and the way in which we interact with those we study. Two of the tenets, objectivity and value-neutrality are particularly objectionable as they fail to address the concerns of those studied. Allowing positivistic principles to determine appropriate research prevents change as "The Master's Tools Will Never Dismantle the Master's House" (Lorde, 1983a).

Ladner (1987) explains how her experience as a Black woman clashed with her training as an academic when she researched the lives of poor Black 'girls'⁷². Academia taught her to keep a distance from those she was studying, however, she rejected the need for objectivity in research stating that it was immoral to maintain this approach when faced with the conditions of poverty and racism that the girls had to live in. Instead, she had to decide 'whose side she was on.' Her choice was between unconventional anti-racist research or traditional research (the universities form of institutionalized racism). She sought to avoid the domination and oppression that researchers engage in when they use traditional research theories and tools (75-79).

Applying positivistic principles to our research produces work that is irrelevant or in some cases harmful. However, when we bridge the gap between ourselves and those we research similar results can occur.

Patai (1991) says that feminist efforts to decrease the distance between ourselves and our subjects are ineffectual. Despite our common womanhood, there is no avoiding the fact that when we in the First World study women from the Third we are exploiting them for data (137-9).

In fact, Patai claims that the intimacy that we hope for as feminists in the field (an intimacy that blurs the

⁷² Ladner's word.

distinction between research and personal relations) sets up false expectations for those we work with. Our subjects hope that through contact with us they will gain access to various resources; most of which fail to materialize. What is accomplished through this fake intimacy is that we get more information for the same ends as positivistic research-to publish. The work of Anne Oakley (referred to earlier in this chapter) is an example of this. According to Patai, Oakley's attempt to trade subject/object distance for 'sisterhood' was done to arrive at a better research process and to get better results. In the long run, we retreat to our separate lives, leaving the women we study no better off than before our arrival (143-4, 149).

Patai uses her work with impoverished women in Brazil to illustrate how, despite the best of intentions to uncover racism, sexism and poverty, her research led her to objectify and show pity for one of the women she studied. Patai asks herself what the women whom she describes in such sad conditions would say if she read Patai's account of her life (141). Whether the impossible distance of positivistic research or the 'mystifying chumminess' of feminism we are engaging in either exploitation or patronage.

Lorde (1983b) says we use the quotes and experiences of black women to add 'colour' to our work. Hooks (1984) agrees that women of colour are wanted for information and used as objects of a privileged discourse on race. McKenna

(1991) condemns scholarly practices in general for their tendency to treat the words and experiences of others as resources. Often, we fit this material into our preexisting frameworks with no concern for those involved; the result being oppression (114).

Further errors occur in our perception of 'other' women when we compare them to ourselves. Several women of colour have critiqued white feminist scholarship for using white women as the norm to which other women are compared to unfavourably. Goetz (1991) says what is taking place is "norm setting activity by a counter elite" (144). If white women are normal other women are abnormal: either deviant, exotic or primitive. The contradiction is that we critique male scholars for comparing the behaviours of women unfavourably with those of men.

Ladner (1987) and Dill (1987) both describe the negative repercussions for our perception of black women when their lives are compared to those of white women. Black women are described as oppressing black men with their dominant characteristics. Women from the Third and Fourth Worlds are described as 'exotic' or 'primitive' when compared to white women. Bhavnani and Coulson (1986) also critique white as norm for it overlooks the oppressions women of colour experience. The women are described as different and their problems, if discussed, are blamed on their differentness rather than on the conditions they are

living under.

Another approach to white women as 'the norm' is to homogenize the experiences of all women from those of white women. Hooks (1984) states that it is inappropriate to consider the 'oppression' that bored rich white women experience equivalent to that of single mothers on welfare. Nor can we expect women of colour to work with us if we consider our oppressions the same.

Goetz (1991) says assuming a commonality between all women, and celebrating our intimacy with others, is not the solution to the oppression of women. Relying on a feminist solidarity to sooth our consciences, or as a "feel good" measure, is fraudulent. Instead, we need to acknowledge that the world is divided along lines such as race and class (145-147). Otherwise we overlook those most oppressed and unorganized (Hooks, 1984).

What is considered liberation for women of colour differs from white liberation. Feminism if it does not include a freedom from oppression for women of colour is not appropriate. Hooks says for some white women feminism means they are allowed to become men but for most it is (Ehrlich cited in Hooks, 1984):

not about dressing for success, or becoming a corporate executive, or gaining elective office; it is not being able to share a two career marriage and take skiing vacations and spend huge amounts of time with your husband and two lovely children because you have a domestic worker who makes all this possible for you, but who hasn't time or money to do it herself ... (7-8).

This phenomenon of white women as 'the norm' also means that women of colour are added onto models for white women. Hence women of colour are on 'the margins'. Once again their oppression addressed inadequately, if at all. We also critique male scholars for adding women on to models for men as this approach fails to address our issues. Trinh (1989) says:

'Wo-' appended to 'man' in sexist contexts is not unlike 'Third World,' 'Third,' 'minority,' or 'color' affixed to woman in pseudo-feminist contexts. Yearning for universality, the generic 'woman,' like its counterpart, the generic 'man,' tends to efface difference within itself. Not every female is 'a real woman,' one knows this through hearsay ... Just as 'man' provides an example of how the part played by women has been ignored, undervalued, distorted, or omitted through the use of terminology presumed to be generic, 'woman' more often than not reflects the subtle power of linguistic exclusion, for its set of referents rarely includes those relevant to Third World 'female person.' 'All the Women Are White, All the Blacks are Men, But Some of Us Are Brave'.... 'Third World,' therefore, belongs to a category apart, a 'special' one that is meant to be both complimentary and complementary, for First and Second went out of fashion, leaving a serious Lack behind to be filled (97).

Sojourner Truth (cited in Trinh), at the turn of the century, sought to expand white peoples understanding of womanhood:

(having) 'never been helped into carriages, lifted over ditches, nor given the best places everywhere,' (but instead having) 'plowed, and planted, and gathered into barns,....(yet) ar'nt (sic) I a woman! (100).

As well as overlooking difference among women, there is a tendency in feminism to homogenize the lives of women of colour (reemphasized by the term itself). The women are

considered similiar and usually described as victims of male oppression.

Mohanty (1988) critiques feminist scholars for describing Third World women⁷³ in blanket terms as powerless victims of male violence and colonial processes. In reality Third World women are diverse. Women's problems and their needs vary as, for example, do those of the middle-class housewife and her maid. African women should not be considered as one, nor should generalizations be made about Arab women who inhabit twenty different countries. Stereotypes show Asian women as passive while Afro-Caribbean women are strong yet at the same time exploited by men (59-61, 77).

The homogenization of Third World women, says Mohanty, when combined with the victim model, and racism, fosters a view of these women as ignorant, domestic and victimized. This view is contrasted with the norm which is either men or white women. So that Third World women are "ourselves undressed" (56).

Mohanty says defining men as the exploiter and women as the exploited is too simplistic. In fact, labelling men as the oppressor is merely substituting biological determinism for social determinism in an effort to unify women. Western

⁷³ Mohanty and I both use this term with reservations for its homogenization of many women in the world. It is an oversimplification but, for lack of a better word, will be used.

feminist scholarship on Third World women uses different methodologies to universalize male dominance and female exploitation. Inductive leaps are made, for example, about veiling, which is perceived by feminists as a means of control without considering the history and power of women in particular contexts. Neither can the sexual division of labour be understood so simply as to be considered universally oppressive (59, 64-68).

As well, the feminist quest for equality between the sexes is irrelevant for many Black women who are already 'equal' with Black men in terms of work outside the home--with neither enjoying the benefits of 'success' (Hooks, 1984).

Goetz (1991) points out that, like differences **between** women, productive **relations** between women and men are ignored. These oversights often lead to the failure of development projects in communities where women and men work together. She points out that some communities are suspicious of projects that seek to divide them along gender lines. In many non-western communities the man/woman relationship does not have the centrality that it does in our culture, therefore too much emphasis on opposition between the two, or on male dominance, is misplaced⁷⁴. To describe women as a group being dependent on men as a group

⁷⁴ This applies to the Inn, as will be illustrated later in the chapter.

is to ignore history and the other oppressions people experience. It also ignores women's economic activities and the incidence of women and men working together for survival (140-2).

Another distortion found in research on 'other' women is a focus on the study of race, not racism or the oppression that people of colour experience. Others study racism only because it has become a 'popular' topic. They fail to make the necessary analysis to facilitate an understanding of oppression. Patai (1991) explains our willingness as feminists to address the topic of difference yet not carry the analysis far enough as a matter of keeping our place within the university. She says "most of us do not want to bite the hand that feeds us; but neither do we want to caress it too lovingly" (139). It is the conundrum that we as 'politically correct' feminists in academia find ourselves in.

As white women, McKenna (1991) says we have access to the abstracted mode of presentation. We learn to express ourselves in an indirect and non-aggressive manner so that we are tolerated. To avoid dealing with racism we rationalize. The result is that feminist scholarship 'legitimizes the illegitimate' by excluding all but the initiated. Any critical thinking we do is 'legitimized and sanitized' so that it gets separated from political action and from popular view (122-6).

According to Hooks (1984) the work of white women who write on racism is patronizing if it is aimed solely at a white audience with the intention of changing attitudes. The work is also problematic when it fails to address racism in a historical and political context. The methodologies that accompany these works are bad and assume only white women are authorities on race issues, or we must act as mediators between racist white women and angry black women (12, 15).

It is not enough, says Hooks, for feminists to include material by women of colour on their course lists or hire a token person of colour (51). These actions do not surrender our hegemonic control.

Another popular strategy for addressing racism but not dealing with it is the reflexive approach. Some scholars assume it is enough to situate oneself without working for change or giving control of the research process to those we study. Patai (1991) says it is not enough to qualify our work:

'As a white working-class heterosexual ...,' or 'As a black feminist activist ...' Sometimes these tropes sound like apologies, more often they are deployed as badges. Either way, they give off their own aroma of fraud, for the underlying assumption seems to be that by such identification one has paid one's respects to 'difference'--owned up to bias, acknowledged privilege, or taken possession of oppression--and is now home free ... such rhetoric once again deflects attention from the systematic nature of inequality (149-150).

The self-reflexive process, she says, merely replaces "what is to be done" with "who am I" (150). The result is that

those studied benefit in no way.

Even if the authority figure is progressive and caring, when others are silenced domination is maintained. In reflexive and confessional literature the researcher still hold the reins of control. Quite simply, when we write on a group to which we do not belong we are reinforcing our power over those we study.

Some feminist scholars have sought to deal with racism in research by avoiding generalizations and value-judgements when studying women from other cultures. Rather than portraying other women as abnormal or deviant they are described instead as different. The goal is to understand other cultures but not condemn them. Each way of life and each culture is accepted and valued in its own right; not measured according to white standards.

Goetz (1991) discusses how this acceptance of diverse experiences and differences among women has thrown feminist politics into crisis. She critiques the responses of two feminisms to the issue of difference. Both cultural feminists (who respect the diversity of all women's experiences) and the French feminists (who claim there is no one category of women) support a fragmented view of women which hinders political unity and hence, action (134,144).

By acknowledging the issue of difference **among** women, Goetz says we have moved our preoccupation with difference between women and men to women and women. Both approaches

disallow for unified change:

In effect, the political has come to be reduced to the personal, instead of being distilled from the personal towards a critical understanding of how structures of power construct difference, ... understanding how oppression is constituted as a system, ... and facing our own implication in certain systems of oppression ... (145).

Goetz views it as irresponsible to postpone our investigations of difference and appear to be in acceptance of the status quo because of post-modern claims. By denying the existence of truth or definitives we are postponing communication and learning which would improve conditions for people. We need to realize there is "a fine line between diversity and fragmentation" (146). We must also realize the inappropriateness of treating each perception as valid when we are confronted with conflicting accounts from, for example, a woman and her abuser (148-9).

We have become so afraid of being racist or ethnocentric by judging people unlike ourselves we end up being racist for doing nothing to improve things. We fear applying our feminist standards to others and we fear working in coalition with women of colour for change.

It is not only white women who have difficulty working with women of colour. The reverse is also true. Hooks (1988) comments on having been silenced when in a room of white feminists. Her issues are not addressed and her voice is not heard or she is told her perceptions are incorrect.

McKenna (1991) (who is white) relates just such a

situation when she brought her Ojibway friend to a women's conference on peace. When her friend told the audience that her community was more concerned with mercury poisoning than nuclear war she was told she did not care about the future of her grandchildren. McKenna concluded that, by attending the conference, her friend was exposed to 'violence' by those trying to force their own agenda on her (112-4).

Lorde (1983a) speaks about the irony of having feminist conferences while women of colour are in our homes cleaning and looking after our children for us (100). Trinh (1989) describes the sense of feeling like "the maids upstairs in the guest quarters" when they do attend conferences (83).

In what capacity do we expect others to attend 'our' conferences. Are we looking for token representation or for objects to examine? According to Trinh our 'inviting' women of colour illustrates our power to allow the silenced to speak. As well we may be tickling our fancy for 'endangered species' and engaging in a 'feel-good' measure:

Now, i (sic) am not only given the permission to open up and talk, i am also encouraged to express my difference. My audience expects and demands it ... We came to listen to that voice of difference likely to bring us what we can't have to divert us from the monotony of sameness (88-89).

As white women we get to act as saviours of 'endangered' cultures. Unfortunately, Trinh says, this fight to preserve 'culture' will keep people occupied with the Saviour's concerns, not those of the oppressed (89).

The 'saviour' also decides who and what is authentic

and therefore needs to be saved. Is there "a whitening (or faking) of non-Western values (?) ... No uprooted person is invited to participate in this 'special' wo/man's issue unless s/he 'makes up' her/his mind and paints her/himself thick with authenticity" (88-89). We determine who represents non-white women at conferences and in our research - who is authentic.

Patai (1991) believes there is a disparity in feminist research between our awareness of the issues and our willingness to do more than talk or write about them. Patai says our "desire to transform the world is often weaker than the wish to enjoy it as it is" (145). Even our efforts to share power with the researched are often only attempts to be affirmed and validated. Hence there is a fissure between theory and practice.

To avoid these problems, we need to consider why we want to work with 'other' women. If they are from cultures considered by some to be 'endangered', is our motivation to 'help preserve their culture'? Or do we enjoy the work because of the power we get to exercise? The answer to these questions can be determined by how well we listen to women of colour. Whether we follow our agenda or their agenda determines who the work is really for.

Unfortunately, Lorde (1983b) says 'destructive guilt and defensiveness' can interfere with our listening and our changing. Rather than calling upon black women to teach us

about themselves and the racism they experience we need to educate ourselves. This is important, Lorde says, as white women are more receptive to other white women speaking about racism. Anything else is like men insisting that we educate them about sexism and only serves to keep the oppressed occupied with the Master's concerns. It is a diversionary tactic when white women insist on being told by blacks how not to be racist (97).

We should not avoid the subject, claiming 'we didn't know who to ask' (Trinh, 1989:80-6). Trinh mimics us:

'What's the difference?' as if I cared? Or yes, I mean it, help me see? Shall I quench my thirst gazing at the plums while waiting for my helper to come by and pluck them off the branches for me? Do I really ask for difference or am I just saying it's not worth trying to find out? (84).

The way to learn is to listen to women of colour who do speak. Black women are rewriting history and addressing the different contexts of their lives. This work needs to be acknowledged and integrated into mainstream discourse. According to Lorde (1983a), it is not enough for journals to have an issue on Third World women (100). Nor is it sufficient to have the work of a woman of colour on our reading list or a token faculty member in our department (Hooks, 1984:51).

Women of colour worry that when they do speak (or write) we only listen if they can avoid making us feel uncomfortable or if we can use their material in our existing frameworks (Lorde, 1983a). Bell Hooks (1984 &

1988) complains about the marginal position of writings by Black women in an already marginal scholarship--women's studies.

Hattie Gossett (1983), in the introduction for her book expresses her fear as a black woman, of speaking to an unreceptive audience by entitling the work "who told you anybody wants to hear from you? you ain't nothing but a black woman!" Cameron (1983), in "Gee, You Don't Seem Like An Indian From the Reservation," says whites are surprised to discover⁷⁵ an articulate native woman. Despite the resistance in mainstream scholarship to work by women of colour Hooks (1988) tells us that people of colour can and do speak for themselves and want to be heard. We need to stop valuing the work of white people writing about others more than we do the works of the people themselves (45).

Resistance from black feminists to the mainstream women's movement arises from the assumption in it that black women are ignorant (Hooks, 1984). In actuality black women do not need white feminists, or their theories, to inform them of their oppression--they live it everyday (10). The solution is for black women to take a central role in the making of feminist theory, and not just black feminist theory (12,15). We must also acknowledge the role that some black women have had in the women's movement and their feminist work in the black movement (6-7).

⁷⁵ no pun intended in the use of this word

Many of the criticisms of the women's movement outlined above are now being addressed by white scholars. However, I have included this material as many scholars are still ignorant of these issues, either consciously or not, and much still needs to be done to change research **on** other to research **with** others.

We have seen that according to some women of colour (Ladner, 1987; Dill, (1987)), a racist approach to research is characteristic of work done by white researchers. Because of the differing history and power that whites and blacks have, 'cross-race research' is problematic. In fact, we cannot avoid the position of power and privilege we hold and the fact that we are exploiting those we study for data.

However, Mohanty (1988) acknowledges that Third World scholars can make the same errors as Western feminists because of their class privilege. Middle class scholars within the Third World may also homogenize those under them and write of them strictly as 'the other' (52).

Most of the authors reviewed above, although critical of the research process, do not call for its discontinuation. They believe that research on and work with others is necessary if our world is to improve. They suggest several guidelines for making the work we do more appropriate.

Solutions

According to Patai (1991) we must acknowledge that "Neither purity nor safety resides in calling one's research 'feminist'" (150). Then we need to realize that we would all benefit from the eradication of racism. We should then fight racism in the academy with the vigour that we fight sexism. Only by putting a halt to our denial of positionality and a fear that we will 'ruin dinner by discussing reality' (McKenna, 1991:111) we can break the silences in academia.

We must challenge our 'ways of knowing' and realize that theory and practice need to be combined. When McKenna went to live in an Ojibway community she discovered that intellectualizing racism was only a starting point. We need to understand that theorizing about racism is not enough (111-112).

Wolverton (1990) says that if we are to eliminate racism we need to expose the inequalities, unlearn the lies, and dismantle the institutions that uphold the status quo (187-8). If we are to improve we must make radical changes in our outlooks and stop fearing we will lose "either the master's favor or the master's position itself" (Trinh, 1989:113).

A denial of our racism, Wolverton (1990) says, is ingrained in us and in the women's movement. To prepare ourselves for criticism we need to acknowledge that

sometimes we do and say racist things without meaning to. It is a fear of not doing the right thing and a fear of difference that have kept us isolated from women of colour. Unfortunately, this separatism perpetuates racism. If we, as white women, are to educate ourselves about racism we need to work together and not wait for the day we are blameless before we act. It is important to avoid confusing our attempts to appear not racist with working to eliminate it (190-1).

By deciding to do something about racism, Wolverton tells us we run the risk of exposing our own racism. We must, however, take this risk since racism is a white problem that requires a white response. A necessary part of this process is to look at why we are doing anti-racist work and what we will get by examining our racism (192).

The next step is to reconstruct the research process. Patai (1991) says that the only way to avoid the exploitative nature of the research is for the community involved to be in control--for members to be the researchers and the researched. Implementing participatory type research that attempts to do this, however, can be problematic. Difficulties arise such as; who in the community do we send the work to for approval, and how long do we keep this 'courtesy' up? As well, merely allowing people to tell their story and giving them editorial control over the work, does not prevent them from being our subjects

(147-9).

Hooks (1988) proposes that rather than an 'authority' doing the work, it should be the oppressed who describe themselves, and define their own lives and history (42-3). She does not, however, state that particular cultures or genders should never write about issues or lives other than their own. Men can write about women but their work should not be considered 'definitive'. It is important for us to learn about the oppressions that others have experienced. Hooks uses the example of a black person writing about the experiences of Japanese American during World War II. The author of this work, however, must be clear as to why she is writing about the topic and she needs to watch for racial bias. This type of work is necessary if it is not the primary responsibility of blacks to educate others about their oppression (45-47).

Trinh Minh-ha (1989) sees the understanding of difference as a shared responsibility. We need to understand that "speaking nearby or together with certainly differs from speaking for or about." As whites we must avoid 'complacent ignorance' and a reluctance to involve ourselves in the issue of difference. Even though it's easier to dismiss difference we need to raise consciousness about racism and we need to see how we oppress others (101).

According to Goetz (1991), it is important to realize that sexism can unite women at the same time that women from

different nations are working with men in various liberation struggles--there is no contradiction here (151).

As women we must agree on some principles and certain agendas. For instance, it is inappropriate to separate gender from race when studying women of colour as they are never one without the other (Trinh, 1989:104). Rather than prioritize oppressions, we need to address the day to day life with race, class and gender variables (Amos & Parmar, 1984:17-18). Amos and Parmar state that true feminism understands imperialism and challenges racism. As well, the lives of those studied need to be put in a historical context and the economic difficulties the people experience need to be addressed (Dill, 1987:104).

Mohanty (1988) says that the local context women live in must be considered, as should their particular history, the material conditions they live in and the ideological power structures impinging on them. She sees Mies' study of the lace makers of Narsapur as more appropriate than most feminist research for it shows the patriarchal organization of a particular culture and women's history in it. No generalizations are made about **all** Third World or **all** Indian women, nor are they described as passive (63-64).

Hooks (1988) says there is a need to radicalize feminist theory by uniting it with strategies of resistance for freedom. We cannot be complicit. We must think differently and challenge structures of domination. We need

to abandon academic elitism, with 'the intent to mystify rather than liberate', if we are to address concrete lives (37).

In feminist movement, Hooks explains, there is a tug of war between the academics and those who see academics as privileged bourgeois theorists. This leads to little congruity between theory and practice, yet the women's movement is forever seeking to unite the two.

In our anti-racism struggle in the university there are several issues to consider. For one, we need to expand our understanding of people of colour to include more than just an identification of them with racism. Language can create problems as it can worsen silences (Wolverton, 1990). Terms such as: women of colour, Third World women, and non-white, force categories and facilitate stereotypes (193).

Gossett (1983) and Hooks (1984) both discuss the importance of reaching an audience by making academic work accessible to 'the masses'. This is done by avoiding the use of 'jargony' words and phrases and theory that are irrelevant or uninteresting to most people. Gossett notes that books such as hers are not sold in ghetto bookshops or even in airports and therefore she questions their usefulness (47-51).

There are difficulties with focusing on written material as a way of improving the world for women, particularly when the work is inaccessible (Hooks, 1984).

Many theorists do not plan for their ideas to reach the masses. Yet Women Studies continues to grapple with the relevance of their work to real women.

There are problems, Hooks says, with spending thousands of dollars on conferences that only reach a select few. One reason we continue to produce inaccessible work is our fear that our material will not be accepted by the academy if it is 'too accessible', yet this decision means that it is only available to a small group of people. The institutionalization of feminist education needs to be deconstructed. Hooks calls on us to take feminism out of the university and into the streets and homes. Why not teach women's studies at the YWCA where more women can be reached rather than in an elitist institution like the university? (108-111).

In seeking to combat racism in society, Wolverton (1990) and her anti-racist group discovered several things. For one it is important to not shy away from the racism we see in other whites. Often, when we are in the presence of someone who makes a racist statement we are either silent or accusatory. No dialogue takes place. This is unfortunate as it prevents learning (197).

When confronting racism in other people, the group found it most effective to avoid accusatory or argumentative responses. This approach avoided polarizing people. It is best, instead, to face these people with honesty and

compassion. Clearly guilt can immobilize people and prevent them from taking responsibility for their actions and attitudes (194-5).

Wolverton also suggests that as white people we can be anti-racist by writing letters and articles of protest, holding workshops, working with other whites on our racism and correcting others (199). Only once we take the initiative to work on ourselves and our society will real change take place.

She also suggests that if we are to fight racism we need to understand our own culture. Wolverton says that many people in the West lack an understanding of their own culture hence they feel it necessary to mine other's. Our buying Latin American clothing or African masks is an example of this. These behaviours accompany a romanticization and objectification of other peoples (199).

Feminism and Participatory Research

As feminists our research agenda is to improve the lives of women. We are also learning to acknowledge the power and privilege we have as academics, particularly if we are white and middle-class. Once we acknowledge our racism we need to find different ways of doing work with women from other cultures. Several scholars have proposed solutions based on their research projects.

Patricia Maguire (1987) attempted to combine

participatory research and feminism in her work with battered women from three cultures. She does not address racism as a topic in the participatory research and encountered several difficulties with the project, however, her analysis and experiences are extremely insightful and outlined in book form. She begins Doing Participatory Research: A Feminist Approach with a review of the participatory research literature. What she discovered was that, like positivistic social science research, participatory research is androcentric. In her words it is "one more male monopoly" (48).

The researchers use of terms such as 'the people', 'the community' and 'the oppressed' hide the fact that the work focused on men, relegating women to the periphery. She wondered about these participatory research projects:

exactly which people are empowered and which social structures are challenged? When participatory research claims to empower a community or group, are the women in the community equally as empowered as the men? When participatory research declares its intention to attack oppressive social structures, is patriarchy one of them? (50).

Surely, she says of one researcher, "he can hardly conclude that the community is learning to work democratically when half the population was excluded"(57).

Her study shows various ways in which women are marginalized by participatory research projects. She discovered that unequal access to project participation effected the agenda and the results of the work, as did

unequal access to the project benefits. Unless special measures are taken, women's participation is limited by lack of financial resources and lack of time because of the double work day. Patriarchal attitudes and practices can also inhibit and even prevent the involvement of women in such projects (51-2).

Maguire cites Morgan who claims that without women's participation in these projects "the alleged worldwide redistribution and equalizing of power and wealth may in fact be taking place only between men (72)."⁷⁶

However, as a result of a resurgence in the women's movement in the 1960's, women's issues⁷⁷ were once again⁷⁸ being discussed. The movement's effect on academia has been such that it is no longer appropriate for "a circle of men to define the world" (83).⁷⁹ Feminists denied that there was only one view of reality and only one way to investigate it.⁸⁰ Maguire's point, however, is that women have different ways of knowing and different things to know;

⁷⁶ This echoes Kalindile's complaint to Mbilinyi that for Tanzanian women rule by black men was not different than rule by white men.

⁷⁷ For the most part, the issues of white middle-class women as mentioned earlier in this chapter.

⁷⁸ We tend to forget the women's movement at the turn of the century which got us (white women) the vote.

⁷⁹ It expanded to include definitions by white women. See above.

⁸⁰ The alternatives they came up with were described earlier in this chapter.

their everyday experiences are different from those of men (88).

The social science research process hid this fact. Research was⁸¹ used to increase social control of the many by a few; particularly masculine control. Feminist research sought to change this by filling the gaps with accounts of women's lives and later by critiquing positivism. The feminist agenda was not knowledge for the sake of knowledge but for women. Rather than the goal being documentation it was a vision of what should be (86-88, 92, 99).

Participatory research and feminist research validate people's perceptions of their reality. Both approaches also benefit ordinary people by facilitating the realization that their personal experiences are part of larger struggles. However, traditionally participatory research, not feminist research, has involved people directly in the work. "Unfortunately, the **how** of feminist research is not as well developed as the **why** and the **what**" (101).

Maguire's study of feminist research provided her with the principles and the goals but not the method. "I was left disappointed, wondering now what" (102). How should her research be carried out for it to empower women? She says there were only 'vague and sketchy' descriptions based on principles of dialogue, information exchanges, the development of trust and personal relationships, and the

⁸¹ And still is to quite a degree.

reduction of power relations.

However for Maguire it was important that women be able to share with each other, not just with the researcher. As well the research process needed to be one of empowerment like the consciousness raising groups of the 1960's. She said, "Liberation will come when ordinary women share power rather than when powerful women share vulnerability" (104).

Maguire found that the framework for participatory research provided her with the 'how-to' of research that she needed⁸² and, when combined with feminist principles, the approach's androcentrism was avoided. By focusing on problems of women in a targeted community one could do participatory research (103-4).

Maguire choose to do work for her Ph.D dissertation on pariticipatory research with battered women in Gallup, New Mexico. She landed in Gallup when her husband got a job there and, looking around for a feminist group to do participatory research with, she discovered the Battered Family Services. On her own inititive⁸³ she volunteered to do follow-up work with the former residents of a battered

⁸² My experience with participatory research showed otherwise. I found little guidance or direction from the literature in terms of the how-to of participatory research. I had to rely on the principles to guide our work and then 'wing it', successfully in some ways and not so successfully in other ways as described in chapter three.

⁸³ She explores the difficulty with "putting yourself in a position to be 'requested'" (112) for participatory research.

women's shelter by facilitating a support group.

The women that eventually formed the group she was to facilitate were from different cultural backgrounds: nine Navajo, two 'Anglo', and two Hispanic. She realized the difficulty of doing cross-cultural work, herself being white, and that Native issues were not feminist issues, but decided to proceed and not be "paralysed by perfection" (128).

During the entire project she encountered much frustration and loneliness as a result of little support for the work. Most people did not understand what she was trying to do.⁸⁴ The women in the group expected her to take a leadership role which she continually tried to shrug off. She also became very emotionally involved in the project and the lives of the women she was working with. She was constantly evaluating her actions and her relations with the women as she struggled to give up the power she had (155-178).

Maquire had to learn what her boundaries were with the women and what she would have to leave undone if she was not willing to do it herself. She was disappointed, for instance, when the women failed to make a structural analysis of their oppression and discontinued the meetings once she stopped taking part (178, 204).

⁸⁴ Likely she continued with the work because of her commitment to the women, the shelter and to her dissertation.

Despite this, the women reported that the meetings had helped break their isolation and self-blame. She herself fought:

The temptation to dismiss or underestimate our efforts because they do not appear long term, transformational, radical, or important enough. The challenge is to celebrate our collective accomplishments, however small, and nurture ourselves as we move, however slowly and imperceptively, in the direction of change for social justice (199).

Part of the difficulty with her work, she acknowledges, may have been her whiteness. Maguire ends by wondering if any project can juggle gender, race, class and cultural issues (210). Earlier in the book, however, she cites Tandon who says "Participatory principles are not purist, you can't sit and wait for the ideal situation. **Waiting to do it right is paralyzing** (my emphasis)" (127). This theme we hear echoed again and again--it is better to do something than nothing.

Delle Small (1988) was also interested in doing something to improve conditions for women. Her paper reports on her efforts to do participatory research in New Zealand with both white and Maori women.⁸⁵

Small begins her article with a review of Maria Mies' work to illustrate the point that feminist research should bring theory and method closer together. Participatory research, she says, has this agenda but must include

⁸⁵ Her description of the Maori people as 'they' implies that she herself is non-Maori.

empowerment for women. (89-90).

The role of feminist scholars, according to Small, should be to connect their disciplines to women's struggles at the grassroots level. To do this we need to acknowledge that women understand oppression and are doing development in their own way. However, the women need more unity and less poverty in their lives (91).

Our task then, as research facilitators is to "link women in a common task, ... vitalize the struggle and inform the study" (91). A project such as this would include the organization of women, the production of information, an understanding of the problems and strategies for change--not just a refinement of methods and theory⁸⁶ (91).

In her own work with women, Small adopted this type of participatory approach to research. Although her colleagues did not support her efforts, she persisted in her plan to work **with** women, rather than **on** them. With the assistance of her students, who had contact with low-income women, Small embarked on participatory research with local women in Christchurch. Her agenda was to empower and conscientize the women she worked with (86-9).

Small's work combined participatory research with a feminist agenda of empowering women. Her 'development' work brought women together to identify their experiences and see

⁸⁶ Problems with the assumptions this type of research are based on are discussed in chapter three. Her approach assumes women need the assistance of outsiders to organize.

patterns in their lives. They also identified myths that legitimized their oppression by men and by other women. They were empowered by the process and then brought their knowledge to Small's students at the university (88).

Small describes her initial research projects with low-income women in Christchurch from the perspective of an expert. She and her students facilitated the women's conscientization. However, her work with Maori women was described differently. She mentioned that the women knew what their goals were without her assistance. Although they had employment concerns, their agenda was to strengthen their culture for all Maori people. Her role with them was not made clear by the article (92).

Small described, however, that the Maori had a lot of experience doing participatory type research. Their work naturally involved a bottom up approach as they were at the bottom and need to work together. Their tribal institutions facilitated a community approach to research which included action and development (92).

Out of necessity, due to the lack of outside support their initiatives have received in the past, the Maori identified their problems, addressed conflict, reached consensus and got their needs met with minimal outside assistance. Their aims were clear. They were interested in protecting their culture, their language and their land and fishing rights. This work, Small said, was aided by the

networking possible in a communal culture with a rich oral history (92-3).

In "Women's Participatory Research in the Kayahna Tribal Area: Collective Analysis of Employment Needs", Grace Hudson⁸⁷ (1982) describes another research project done under the initiative, not of academics, but of several Native communities in Northern Ontario. The people and their goals are similar to those of the Maori--marginalized people interested in community and economic development.

Those involved in the project knew that solutions for their problems needed to come from within the community. It was from within the communities that needs were defined, resources accessed and solutions generated. This approach was essential because of the differing values that Indian communities have (149).

The particular focus of the project became women, since their development needs were being neglected. It was decided that an intensive women's participatory research project would be done for these communities involved. With funding for a collective research process from several governmental and non-governmental sources, a team of women researchers formed (154 -5).

⁸⁷ Judging by her use of the words like 'our communities', Hudson appears to be a native woman from one of the communities involved in the project. However, it is unclear as other times she uses 'they'. As well, she also says "Native women are very shy" (156). This seems an unlikely statement from a native woman.

The process was truly collective. Rather than focusing on individual responses to questions (the way questionnaires do), or using outside researchers, they decided to instead use collective data gathering methods. This included the use of group meetings, community dialogues, workshops, music, stories, drawings, photographs and more. Bull-shit sessions were used to encourage conversation, as were lunch and tea gatherings. At these meetings the coordinator⁸⁸ of the project confessed that she did not know how the project would end. With these approaches combined the groups were able to reflect on their own reality and draw on their culture for strength and knowledge (156).

What was important for the women was to link the study to action. They did not want to treat the symptoms of their oppression but instead develop their own economic and political capacity (157).

The results, among other things, produced a statement of needs from the women in the community. They wanted a training centre in the region that would offer specified courses and training in, for example, hairdressing, baking, and adult education. In addition, the women researched and determined the type of sewage disposal they wanted in their communities (152-3).

These practical grassroots issues and their solutions differ from the research topics chosen by academics studying

⁸⁸ Unclear who the coordinator is.

Native communities. It was only because women from the communities controlled the process that such useful findings were arrived at.

Is it then inappropriate for white researchers to work with native women? Enough is Enough (1987) is the result of a participatory research project which seems to successfully combine the efforts of native women and a white researcher. The native women involved in the work had control over the project and a clear sense of accomplishment, as indicated by the book's success.

The book is a collection of the life stories and conversations of Native women from the Tobique reserve in New Brunswick. Their stories, as told to a white⁸⁹ woman named Janet Silman, recount their experiences when young and their successful fight for recognition as Native women under Bill C-31.⁹⁰

Silman, who had been doing work "informally as a researcher and writer for the Tobique women for about a year" (15-16), was asked to help them get their stories published. They choose her to do the work as she did not

⁸⁹ With 'Indian blood' from her Scottish Metis grandmother.

⁹⁰ Up until 1985 Aboriginal women lost their status as Indians, and hence their right to live on their reserve, when they married a white man. If they were divorced or widowed they could not regain their rights whereas white women who married Native men got Indian status. It was the protests of the Tobique women, in particular, that brought public attention to the injustice of this law and its eventual evocation.

come into the community to get information from them and then leave never to be heard from again. Instead she lived and worked with the women and supported their cause for several years (15-6).

Unlike some life histories (for example, Shostak's Nisa), Silman's comments in the book are minimal. Only in the introduction do her words enter the book. She gives a brief history of Aboriginal people, in particular the Tobique people, and the women's political activities. She also states that the story is "not about them but by them, in their own words" (9).

The introduction also explains why the book was made. The women decided to take a risk and tell their story so they could tell other Native women about their potential and educate white women about reserve life (15). The book does exactly that.

It is not specified who gets the royalties for the book but the copyright is registered with the Tobique Women's Group. Nor is it clear why the story is 'told to' Silman rather than having the women's group listed as editor.

In her review of the book, Lynne Phillips (198) describes Enough as Enough extremely flatteringly. She sees it as important for it "demonstrates the vitality and political potential of native women...(122)." At the same time it does not portray them as helpless victims. It also effectively combines the issues of race, class and gender

(something Maguire doubted could be done). The only weakness she sees in the book is Silman's failure to discuss just how much control the Tobique women had over the book. For example, how much control did Silman have over editing and translating? Overall, however, the book is a valuable contribution to Canadian literature and a successful example of a work, between Native women and a white researcher, that seeks to empower women and work for change.

My Feminism and the Innu People

It was exactly that which I hoped to do with my research - work for change and empower women. It took some time, however, I have learnt how to do this. As mentioned, three years ago, when I entered the Master's program in the Department of Sociology and Anthropology at Carleton University, I decided I would write a thesis that would in some way contribute to feminist research.

Allow me to go back even further in my life. From the time I was a child, growing up during the 1960's (a high point in the women's movement) I was a feminist, and an angry feminist. However, I had no focus or outlet for these sentiments until I was several years into my university career at which time I discovered women's studies. Until then I was taught by men about men. The few descriptions of

women I did encounter described them as passive objects.⁹¹

Despite my status as an undergraduate who still believed that what we were being taught was supposed to be 'the truth', I had a nagging feeling that, despite the renown of Levi-Strauss, women were not simply exchanged⁹².

Near the end of my undergraduate work I discovered that women's lives and their particular issues could be studied through women's studies.

It was a propensity for experiencing other cultures as a result of my parents' relationships with people from other nations, combined with a pattern of running from my own issues, that fostered my interest in Third World studies. When added to my feminism (flourishing after the end of an abusive relationship) and a Catholic upbringing that insisted we help others, I decided to make my speciality Third World women and development by studying anthropology.

As mentioned earlier in this thesis, at the Master's level I had initially planned to focus these interests by doing a study of prostitution and militarism in Southeast Asia. Circumstances changed however and I ended up proposing to do work with Innu women in Labrador.

⁹¹ So much so that I use the word 'them' rather than 'us' to describe women in this sentence. The university has facilitated this process of alienation from my gender by teaching us to study others, not ourselves.

⁹² I refer here to Levi-Strauss theory of kinship which is based on the premise that men exchange their sisters for wives with other men.

My hope was to contribute to the feminist literature on women's forms of resistance and women in other cultures. Despite my decision to do participatory research with the Innu, I hoped to either record how the Innu women had organized their protest against NATO or how the presence of NATO effected them. This after abandoning my decision to explore the connection between prostitution and militarism among young Innu women, a topic I discovered was politically incorrect for a feminist.

After contacting Rose Gregoire by phone and outlining my proposals for research my energies were diverted as I attempted to deal with life in a foreign culture.

Even before going to Labrador, I had decided that I would focus on the lives of women. I wanted to avoid men since I thought that as a foreign white woman it would be important for me to stay clear of any compromising situations if I were to gain the respect of the community. I was also interested in learning about other women and acquiring a respect for my own gender.

Despite the best of feminist intentions, I failed to focus exclusively on women. In fact, during my first summer with the Innu I spent more time with men than with women. In the country I learnt women's work. But I also went hunting with Jack Penashue, my later-to-be translator, and his brother Bart. Most of my time in the country was spent in mixed company as the focus was on family units, not

gender groupings.

In the village (which they call 'the community'), once the nun had left her house for the summer, I spent most of my time with Jack who, unlike Anne, was willing to spend time with me working on Rose's research topic.

Despite my initial disappointment that I would not be focusing only on women's discussion of life in the country, as opposed to the problematic life of the community, I accepted her decision that men be included too. Interestingly, even before Rose told me to include women and men in the project I had accepted Jack's services as a translator\interpreter. I did not question the appropriateness of his working on a project with and for women as we had a good rapport and I was new enough in the community to be grateful for any interest in me and my work.

Unfortunately, because I had decided that Jack should interview men and Anne should interview women, and because Anne had decided that she would do her interview without me, I visited more men with Jack than women. A few times this guideline was abandoned as family ties led us to, for example, interview his aunt whose opinion I felt was worth recording but whom Anne was not going to interview (likely because she was not related to her).

I continued to maintain a rapport with Jack's mother and his sisters because of my stay in the country with them. And, Rose continued to visit me at the nun's house to

see how I was doing. But most of my time was spent with Jack or alone.

Intellectually I justified my work not focusing on women by recalling the Black feminist scholars I had read who stressed the need for women of colour to work with men of colour and not be divided in their many struggles (Hooks, 1984). I did, however, make a conscious effort to realize the importance of getting the words of women included in our project. I had a great admiration and respect for the older women in the community and when I went back for a visit at Christmas I made a point of visiting with them, not just spending time with my friends.

However, in terms of my definition of feminism I was not doing feminist work. I was not furthering the study of how women's lives and oppressions differed from those of men. Instead, we were looking at problems that faced whole families. Nor were we researching the ways in which women resisted their oppression. Instead, we recorded the way the Innu **people** regained their health and culture by returning to the country. All of our questions concerned women, but not women exclusively. And, as mentioned earlier, the women Anne interviewed found it troublesome that the questionnaire prevented them from discussing their families, forcing them instead to focus on themselves.

In Sheshatshit, as in other Aboriginal communities, the extended family is extremely important. Family relations

determine access to resources, support, and culture. Many of the services that people in urban centres rely on the state for are provided by family members among the Innu. Had we excluded men from our study of the differences between the country and the community we would have painted a skewed picture of the problems the Innu face and their ways of coping with them.

As mentioned my research plans for my second summer in Labrador were also thwarted. I had come to understand and accept the need for me to speak with women and men while doing research yet when I suggested asking people about our research the summer before the idea was rejected. The alternative, to collect the life histories of Rose and Elizabeth, was a feminist project.

I was familiar with the life history method of research and the issues that accompanied it as women's scholarship was extremely prolific in this area. I hoped to have our book published by The Women's Press and distributed nationwide.

As stated earlier, the women were interested in the project but suggested that it be expanded to include the stories of other women in the community. In addition to men's stories being excluded from the book I decided, with an okay from Rose, that we⁹³ would only hire women as

⁹³ Perhaps the word 'I' would be more appropriate here as I was paying the translators.

translators, no men would be involved.

I felt nervous presenting the project as several men in the community had expressed disapproval of the women's group that formed to resist NATO. As well, one man had discussed with me his uneasiness with my being a feminist. He was apprehensive that I would encourage women in the community to separate themselves from men in the community and thereby weaken their struggle against imperialism. I had tried to reassure him that I was not in Sheshatshit as a radical feminist but to do work for and with the Innu **people**. It became more difficult for me to make this statement when the book we were doing was to only include women. Fortunately by the time of my second summer in the community I had the support of several influential women in Sheshatshit who wanted their stories told to Canadians and Innu children. So we cooperated to facilitate the meetings and stimulate discussion.

From what has been translated and told to me, the women are talking about their early life in the country and have plans to talk about their protests against NATO. How much editing of the work will be necessary is unclear at this early stage.

As mentioned earlier, our work had been limited by the demands placed on our time. Women's work such as child-care, cooking, cleaning and visits to outlying camps slowed our work down enormously. This summer I hope our work will

be more prolific.

Another difficulty I encountered was the issue of translation. Because the women in the community were so busy with their children and schooling and employment, very few were available to transcribe the tape we made into English. Those who did agree to the work could only commit to a few hours here and there. There were however a few men willing to do the work. Several times I denied them the work, with the okay of a couple of the women I was working with. One time I conceded but the work was not done. Perhaps in the future we will open the job to both genders.

But the stories will remain only women's stories. Men's stories will not be included. My⁹⁴ initial decision to include only women stemmed from my belief that we had a good chance of being published by a women's press since there was a realization in the women's movement that we needed to understand women in other cultures, particularly within our own borders. Hence we needed to exclude men.

I also felt it was appropriate for a woman from another culture (myself) to minimize her dealings with men in a culture not her own. My first summer with the Innu, when Jack and I were going to people's homes to do interviews, he told me that because we were walking around together people would assume we were in a relationship. At the time I

⁹⁴ Like Maguire's difficulty I had to suggest the life history project. It was not suggested to me, as it was to Silman.

thought he was joking but later I discovered he was not.

The second summer I followed through on my desire to develop my relationships with women in the community. And I actually spent little time with men. Jack no longer working with me and the work I did was only with women. As well, I stayed in Rose's house so I became closer to her and her daughter and her daughter's friends. Jack's sisters and I also became friends. What also happened was that I did what women in the community did - I cooked, cleaned, shopped, and socialized.

As a result, I learned the richness of Innu women's lives and how they were oppressed. I also learned how they survived and fought back. I discovered what the women's interests were and what they feared. At a particularly enlightening forum, which I referred to earlier, several women outlined their issues to the community and to land claim officials.

Their concern with maintaining their culture was not a typically feminist issue. They did not, however, claim to be addressing women's issues but were instead concerned with matters that effected everyone in the community: women, men and especially children.

Elizabeth Penashue spoke of the need for her people to be able to continue returning to the country. She did not want state developments to interfere with this essential part of their culture. Her sister, Anne-Philomena Pokue,

wanted better housing in the community. Some of the houses did not even have plumbing facilities or running water. Kathleen Nuna wanted the Innu people to have control over the education of their children. They were tired of being educated in a white system with a white curriculum in a foreign language (English). Lastly, Rose Gregoire spoke of the need for a shelter to house women and their children when they were being abused by their partners.

These women know what they want and will fight to get it as was made clear by their successful protests against NATO. My respect for them grows each day as I learn more about the hardships they encounter and their determination to not only continue but also appreciate life. However, I also see strength in the men. For me, men can no longer simply be classified as villains who oppress and exploit women. The family system and the community is tightly interconnected and what affects one person affects many.

Years of abuse by the Canadian state, by Euro-Canadians coming to the community, and by non-Innu people in the area have effected the Innu people in many ways. Despite the amount of abuse that women, children and men have experienced, healing is taking place. To encourage this process, rather than disrupt it, I need to listen to the desires of those in the community I have chosen to work with (and who have chosen to work with me) --for now the women of Sheshatshit.

Chapter 5

CONCLUSION: Learning and Changing

Answers to Research Questions

What then is appropriate research? In chapter two we saw that inappropriate research has been done in anthropology for over 100 years. Chapter three described the infamy of anthropological studies among those studied by anthropologists. Both native and non-native writers have criticized the discipline for carrying out studies on aboriginal peoples to assist colonial regimes (and neo-colonial) and to produce work solely for the academy; the research failed to benefit those studied.

Participatory research was posed as a possible alternative to an exploitative approach to research. We discovered, however, that how the methodology was applied varied from practitioner to practitioner. And, although I explained that particular principles in participatory research are important, the assumed need for conscientization is problematic.

Is our work irrelevant work to those studied? Often it is. This has been particularly true for women in the communities studied. Often, as shown in chapter four, the lives and concerns of women are overlooked by visiting

researchers. When feminist scholars do the work, however, another difficulty arises out of the dangers with doing cross-cultural research. The chapter illustrated that being a woman and being a feminist does not ensure that our work would benefit women we study in other cultures.

From chapter two we can conclude then that if research is to be appropriate for those in the targeted community it needs to address issues that concern community members. There needs to be a purpose to the work other than the production of knowledge for the academy or for big-business.

Chapter three shows us that not only should we take our agenda from those we are studying, but we should step back, if possible, to allow community members to carry out research, analyze it and use it. The assumption here is that whether we take the initiative and offer our services as researchers to a particular community or whether they approach us for assistance, we need to work very closely with community members. Otherwise, there is the danger that the work will be irrelevant or worse it will hurt those involved.

Participation and input at all stages of research is important, otherwise the limited control people are given acts merely to disguise the researcher's real power. Researchers need to understand that the work done is not theirs alone. Because it is done with the permission and support of community members, and because these people

direct the research which is on their lives it is their project. We merely participate in it.

I can say that I am doing a life history project with the Innu women of Sheshatshit but I am **not** doing a book on them. They decided to accept my suggestion that we do a book. The women decide who will participate, how the work will be done and what will be said. My job is to facilitate the recordings by providing the tape recording materials, organizing the meetings, and the translations. Later I will work (hopefully with several women) on the editing and seek a publisher for our work.

In summary, we see that the agenda selected must either come from within or be approved by the community. Several volunteers (paid or unpaid) need to work with the outsider on all stages of the project to ensure that the work remains beneficial to those involved.

For me, work is appropriate if it addresses the needs and concerns of women in the targeted community. This does not preclude work that focuses solely on the activities of men. I would prefer, however, not to do work of this nature. Due to the enormous hardship that women in aboriginal communities experience (sexism in addition to poverty, racism and imperialism), I believe that overlooking women's lives is inappropriate. If all members of a community are to benefit from a research project, the activities and the needs of women must be addressed.

Whether the work focuses solely on women depends on what those involved in the work decide. As described, my first project with the Innu included work with women and men, however, the second summer our book included only women.

As white researchers working with aboriginal peoples we need to acknowledge our power and our privilege. We must be clear on the motivation for our work and what we hope to get from the project. If we are to avoid the mistakes of the past we need to understand how racism has effected the people we work with and our own attitudes and behaviour.

After analyzing my attempts to do participatory research the first summer I spent with the Innu, I realized that racism and classism were preventing me from trusting the Innu with the research process. I assumed that I had a better understanding of how research should be done than did the two people I worked with.

Researchers need to assume a humility when working with people from other cultures. Merely because we are from a dominant culture and have the skills that facilitate success in our world does not mean that we are experts in a culture not our own. When working a community other than our own the best way to do appropriate research is to work with community members not on them.

I have found it most useful to have a small number of community members I can turn to when I am unsure of how I

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should proceed with the work or what is appropriate. Also important is time. For a trust and understanding between participants to be established the outside researcher needs to spend time in the community. I have also found it useful to have people from Sheshatshit visit me in my home environment. Otherwise doubts exist as to my intentions

My initial contact with the community was through an anthropologist who had lived and worked on the community on and off for several years. I recall, however, that when I first went to the country I was asked several times if I was working for CSIS.

A final word on a feminist focus. If those involved in the work agree to, or suggest, work that excludes men it is essential for those participating in the work remain involved at all stages of the research. Because women's issues are men's issues and children's issues we cannot afford to treat women as separate entities or as if they are at war with men. As shown in chapter four, particularly when members of an oppressed group, there is strength in numbers and in unity. Furthermore, difficulties in the lives of aboriginal women stem from a combination of poverty, racism and sexism. When an aboriginal woman is raped how can we determine if the violence was motivated by racism or misogyny? In fact, both are involved.

Methods

Many of the feminists cited in chapter four campaign against value-neutral and objective work. They advocate that the researcher bridge the distance between herself and her subjects and interact with them on a personal level. Certainly, in my work I did this, not only by living with the Innu, but also by developing friendships with people in the community, and by working with people during the research process. I tried to minimize the distance between myself and the Innu people during the interviews we did the first summer and later as we collected the life histories. Even with my thesis I have tried to engage different individuals in discussion about my work and what appropriate methodologies are. Any material I produce on my study of methodology, based on my work with the Innu, is sent back to Sheshatshit for approval before its release.

Based on the work I did with the Innu and the principles advocated by the feminists I quote, my thesis focuses on the need for qualitative work. The interviews we produced were few in number and although a schedule of questions was administered vast amounts of data was not gathered. The life history method is of course a qualitative technique. Is there room for quantitative research that is non-racist and non-sexist? And can quantitative research be participatory? Is quantitative research appropriate for aboriginal communities?

Certainly statistical information can be of use to aboriginal communities. Like feminists who discuss the high levels of violence against women, it is important for people to realize the prevalence of structural violence against aboriginal communities. Statistics that show lower levels of health, education and housing conditions for reserves than in the rest of Canada may shame DIAND into action. High rates of alcoholism, suicide and abuse also illustrate the systematic nature of problems in aboriginal communities rather than claiming them as personal problems. Statistics then can support our analysis of racism as exercised by Canada against First Nations peoples.

There are, however, difficulties with administering questionnaires, when questions are written and administered by non-native people. The assumptions behind the questions and the motivations for the questions may lead to a research format that is yet another form of assault against aboriginal people. For instance, I recently showed Jack Penashue a questionnaire designed to access social services in Kahnawake. He examined it and told me he found it offensive and noted that clearly it was not written by native people. He illustrated one element of the cultural inappropriateness of questionnaires by explaining to me that it is rude and insensitive to ask elders direct questions. A couple of the questions were more appropriate, for example, what are the three most serious social problems in

Kahnawake and how should the community deal with these problems?. These questions assumed that people understood themselves and their community and if they had access to the proper resources would solve their own problems.

In contrast to this questionnaire, Jack found our work not as offensive. Likely it is because 1) the topic was selected by a community member 2) the interviewers were Innu and they likely edited their translations as they saw fit and 3) the work was being done for the community. It is also possible that because the work we were doing was to stay in the community, when writing the questions to be administered I assumed the Innu understood the problems they were facing and knew how to solve them. This rather than asking indirect questions, which surveys often do, to address the problem as the researcher sees and to assist him or her in later arriving at solutions removed from community input (except as data providers).

Had we been doing the interviews for a research agency funded by the government we likely would have got less response. Based on past experience with researchers they could have rightly asked how many more studies needed to be done before real change took place. They knew what the problems were and what the solutions involved. How many more times did they need to be asked irrelevant questions, or have their suggestions ignored? They may have perceived that studies were commissioned only to give the illusion

that conditions would improve.

It is not unusual for researchers to enter a community, either in person or by way of their questionnaires, collect the data and then leave to analyze it. Once the researcher has left three unproductive events may occur 1) those who provided information never hear anything more about the project, 2) the results take so long to get back to the community they are obsolete and not acted upon or 3) the results are useless as the questions were irrelevant due to a lack of understanding of community needs or because there is no funding to implement the necessary changes. These difficulties, however, can also arise from qualitative work.

Are quantitative methods okay and what about participant observation? Throughout the thesis I appear to disapprove of these methods. The answer to a method's appropriateness comes from other questions. Do the people agree with these methods, will they benefit from their application and are community members involved in the process? It is not only the end result of the research process that is important but the means taken to arrive at the results. The statistics obtained from quantitative work can assist in fights for better services in a community.

Participant observation can be used as a tool to help the outsider understand what appropriate research would involve. It can also help her establish a rapport with those she will work with. We need to develop a respect for

the people we work with and they need to respect and understand us if we are to work well together. It is not enough to 'respect' people merely because they are aboriginal. This respect is based on an exoticization of them and their culture and it fails to leave room for non-traditional elements in native culture and allows no room for imperfections on their part. Similarly, both we ourselves and the people we work with need to acknowledge that we are not experts on the cultures we study or on the methods we employ. Hence the theory that informs our methods needs to be flexible, as illustrated by the need to abandon my plans to study only women when I first went to Sheshatshit.

Relevance of the Work

The question could be asked of my work--what was the use of us doing the interviews two summers ago and what good will come of the life histories we are doing? Will the lives of the Innu people in Sheshatshit be bettered? In fact will the research process itself, one in which I tried too hard to be feminist and participatory, benefit anyone (aside from employing people for brief periods of time and providing me with a thesis)?

As I discussed in chapters three and four there have been benefits to our work. Even if material conditions for the Innu are not changed as a result of our work, it is

essential that aboriginal people be involved in the research process. If we are to change attitudes we need to take direct action and not just pay lip service to the need for power sharing and respect of other cultures. As researchers, we must assume that others do not need our help to develop but instead ~~we~~ need help to understand our ignorance. It is time we helped ourselves by listening to and working ~~with~~ others.

My work in Sheshatshit benefitted the Innu people in that they were able to have their opinions and stories recorded for others to hear now and for future generations. As well, doing the research in the manner that we did showed them that there are other ways to do research--that is how it is done (with their input) and who it is done for (the community).

What use is my thesis to them? Not much I am sure, as indicated to me by their lack of interest in my work and their impatience for me to be done with my thesis. I do hope, however, that those who do read it will come to understand the traditional research process and alternatives to it so that in the future they can make more demands on visiting researchers.

I can also question the usefulness of my work, both searching for appropriate methodology and doing collective work with the Innu, when there are so many more serious problems in the world (and perhaps more appropriate problems

for a white researcher). For instance, reading the newspaper I am reminded of the thousands of children the world over who are starving and the countless being killed in wars the world over. Is it a luxury my ignoring these problems and focusing on work with the Innu? I think not.

I have not chosen to ignore the problems of poverty, imperialism and sexism by working in, for example, a bank. By working with the Innu people I confront issues of poverty, racism, and sexism as is occurs in their lives. Certainly the threat of starvation exists in a community riddled with poverty and neglect. And even war-like conditions can be said to be present with low-level flying disrupting the Innu way of life. This is not to mention the effect that the military presence in Goose Bay is having on some Innu women (as was to be my original topic).

I did not confront heart-wrenching topics directly, but instead let the Innu speak for themselves. This does not make our work any the less valuable. And in terms of my thesis, yes its utility for the Innu is questionable. But that, I would argue, is the nature of most academic work. The value of my work, however, is that it has allowed me to understand more about myself and my discipline. I now know where I am coming from as a social scientist and hence can be wary of what I want to avoid, instead embracing methods and people who support the values that I developed these past few years. Now I am sensitive to the utility of

researchers' work, I ask what their motivation is and who the work benefits. Is it participatory, does it include women? How participatory is it and are women merely added on to work designed for men?

Another question posed is whether or not I have the right to do research with Aboriginal people? Perhaps I should be working with poor whites in Ottawa or Toronto. I argue that I have a right, in fact an obligation, to work with First Nations peoples for several reasons. First, I cannot ignore the racism that exists in our society, on an individual level and at the state. This leads to the need for me to work on my own racism. I see difficulties with North Americans criticizing South Africa for its racist policies but failing to look in their own backyard at the way aboriginal peoples are robbed of their land and their dignity in Canada.

I continue doing the work I am doing, rather than for example taking a lucrative job with DIAND, because I believe change can occur. One could argue that colonialism continues today as it has in the past and more research will not change things. However, I look at the changes that have come about as a result of the women's movement. There is an awareness of violence against women that did not exist before. Women are realizing that incest, rape, and wife assault are not unusual and are not the fault of the victim. In addition to attitudes being changed and awareness

increasing, concrete measures are being taken as laws are changing and support centres are being set up. Fifteen years ago wife abuse was not considered a problem and no safe houses existed for women and their children to flee to.

I believe that similar changes can be made in terms of the racism that exists in our world. Today there is a growing awareness of native issues. Canadians are realizing that aboriginal peoples are not just relics of the past. We need to channel this sympathy and the guilt that white people feel for native people into an awareness that change must take place. We can do our part in this process of change by using a participatory research approach and by not ignoring the needs of women in the communities we work in.

I also justify my work with aboriginal peoples based on the responsibility I have to the Innu people. The women in Sheshatshit want the life history project to proceed. It is their work that I am facilitating, work that we will all benefit from once the book is published. I am sure that some people in Sheshatshit wish that I would not return. One evening an intoxicated community member told me that I, and the other whites in Sheshatshit, should go home and leave the Innu alone. Nevertheless, others are anxious for me to come back. (They did, however, mention they want me back as Cathy not as a researcher.) But, I have an obligation to finish the life histories I have started. Were I not to return, once I receive my degree, I would be

like other researchers who got what they wanted and then disappeared.

Another justification for my work comes from women of colour who tell us if we are to fight racism we need to understand ourselves first. My work with the Innu has facilitated a self-exploration into my own history both familial and societal. I am looking at my own issues of abuse, what was done to me and what I have done to others. Through my studies and my work with the Innu I have examined my position in a patriarchal world and in a racist society. As a women I have been silenced and made fearful but also angry. As a white person I have silenced and perhaps made others fearful and certainly angry. As a white person working with the Innu I have a lot of power that I can use or abuse. By exploring these issues I have begun to confront myself and make changes in my life.

One further point to mention that makes it important for me to continue my research is the fact that I work with aboriginal people not on them. This approach is appreciated by people and is more and more in demand.

Perhaps, the reader may state, I should do only contract work for groups and pursue a career in applied anthropology so I can avoid producing material, such as this thesis, that can be considered navel gazing. These are, in fact, my plans. I hope to pursue a Ph.D. in applied anthropology. I do, however, enjoy the theorizing about

methodology and the intellectual stimulation that comes with academic work. My main priority, however, is to do work that is of use not only to myself but those I work with. Ideally all academic work should combine these two agendas. Like feminism advocates, we should be working towards not only understanding oppression but eradicating it.

Future Research

My future research plans, as described in chapter three, are to do work with the Innu concerning healing. Clearly research on healing will assist community members, how it will be worked into an academic dissertation is not clear. Fortunately, I now have an understanding of the do's and don'ts of methodology to guide my future work.

Regarding future research on appropriate methodology, I have no formal plans to pursue the work myself but I have several suggestions. I realize from the struggles I have gone through over the past few years that it is essential for researchers concerned with ethical research to communicate. Those doing participatory research need to meet and exchange stories about the difficulties and successes they have encountered in the work process.

Initially my only communication with participatory researchers was through written materials. Later I meet a few practitioners at CASCA. Unfortunately we were unable to meet and discuss the 'how to' of participatory research.

One woman, for instance, was hesitant to discuss the details of her participatory work with an aboriginal people since she did not have permission from them to do so. It is essential that we discuss the mistakes we made, the troubles we are having, and the approaches we have found successful if we are to use the approach successfully. For myself I could have avoided seeking to control a research process, that was supposed to empower those I worked with, if my first summer with the Innu I had had more guidance and support in my attempt to do 'alternative' work.

Feminists also need to continue their explorations into appropriate methodologies; the power we have as white academics and the difficulties we create with our focus on women. I would like to see more specifics on the struggles that researchers go through as they attempt to work with people rather than on them. Discussions of the pressures from within academic communities, aboriginal communities, and from within ourselves need to be acknowledged if we are to fight against pressure to produce 'useless' 'top-down' research.

Specific methods for research in communities also need to be described. How do we facilitate workshops? How do we work with people to narrow the focus of our work, meet deadlines, and collect data? What about the different agendas that vie for people's attention both in terms of what needs to be researched and in regards to the heavy

demands on peoples' time? Many many more research topics are possible when considering how appropriate research can be done. The more information we have the better we will be able to adopt our work to the needs of the project and the community.

When Are You Leaving

Two years later, after having spent seven months in Nitassinan and having had several community members visit me in Ottawa and Toronto (my hometown), I am no longer asked "when are you leaving". Now people want to know 'how long I am staying?' (did their English improve in a year?) or 'when I am coming back?'. Their questions make me feel that I must be doing something right. However, I also feel guilty for leaving or for postponing my arrival dates in Sheshatshit because of work and school commitments in Ontario. I guess I need to resign myself to a balancing act between my wants and needs and those of people in Sheshatshit. Hopefully we reach a compromise that is mutually beneficial.

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