

PARTICIPATION AS SOCIAL SCIENCE METHOD AND THEORY

PARTICIPATION AS SOCIAL SCIENCE
METHOD AND THEORY:
INDIGENOUS RESPONSE IN THE MACKENZIE DELTA

By

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ABSTRACT

The thesis has two primary components. The first is a theoretical overview of the standards of conduct for the participant-observation method in social science. It is argued that these "rules" for the method are predicated upon a set of epistemological and ontological assumptions about the process of knowing and the nature of cultural knowledge. Both monist and dualist approaches to knowledge are examined in relation to participant-observation, and it is argued in the thesis that both confuse ontology with epistemology. This involves a self-contradicting set of expectations about how a researcher can know another culture and about how objective knowledge can be reconciled with relative knowledge. An alternate framework, adapted from monistic traditions, is proposed for understanding the epistemology of participant-observation. This framework suggests that the intersubjectivity of dialogue in locating and evaluating cultural "texts" be utilized in methods of participation.

In the second component of the thesis, a case study of research in the Mackenzie Delta, NWT, is presented. This is based upon participant-observation fieldwork in two Delta communities in 1981. Native and non-Native residents (long-term and transient) were asked about their responses to the methods of research and participation used by 20th century investigators in the region. Both the methods used by social science researchers and the responses to them are presented, with an emphasis upon how local expectations of conduct and reciprocity shaped the application of method and the images held of social science researchers. The nature of conduct and response is presented within a broader context of research sponsorship and ethics for northern social research, as it was determined that the nature of northern response to research, as well as methods themselves, were in turn shaped by the methodological and practical

expectations of primary sponsors: universities, government, and Native organizations. The case study research fills a gap in understanding how host communities contribute to the creation of cultural texts, and how this contribution is in fact influenced by social and political factors outside the community itself. It is concluded that, by using the proposed alternate theoretical framework, the active role of the hosts and their political expectations in bounding, contextualizing, and validating cultural knowledge can be recognized. This role can also be acknowledged through the development of research ethics codes which recognize the responsibilities of both hosts and visitors to account for the learning process.

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CHAPTER ONE: INTRODUCTION TO PARTICIPANT-OBSERVATION

A. INTRODUCTION to THESIS

This thesis really began with a field trip made in 1979 to the town of Inuvik in the Mackenzie Delta of the Northwest Territories. My purpose then was to investigate women and work, but I found that I was only one of several anthropologists present in the area that summer. As a consequence, residents were prone to make suggestions about the ubiquitous and annoying presence of anthropologists. I came to realize that, not only had Inuvik and the surrounding region been heavily studied by social scientists, but that the people had singled out anthropologists and their methods of participation in local society as being particularly objectionable. Anthropologists were nosy, foolish, bothersome, and even potentially dangerous, the residents of the Delta, Native and non-Native, suggested.

On my return from the field I began to investigate reactions to anthropologists in other cultures, realizing that where massive investigation had accompanied the changes of colonization and industrialization the people were often resentful of scientists whom they presumed to have a role in perpetuating unwelcome changes, and who had immediate presence in their daily lives. Other responses were more positive, commendations of anthropological contributions to cultural knowledge and appreciation of individual efforts and friendliness. I began to ask several questions: What is there about the participation of anthropologists that people were reacting to? How is the Delta situation similar to or different from those in other cultures where anthropologists have worked? I also realized that there was very little literature which could tell us directly what host

peoples thought about social science done in their communities; most reactions were reported by the social scientists themselves.

My own study began with two distinct parts. One was an investigation into the nature of participation itself. I learned that the general method known as "participant-observation" (abbreviated to P-O in much of the thesis) actually encompasses a variety of techniques, including observation and interviewing; the anthropologist may actually "participate" in activities very little. Yet there are a series of commonsense understandings which have developed surrounding participant-observation which revolve around the concept of "participation" as a superficial involvement in the rules, structures, values, and social relations of host societies. Hosts also experience the visiting investigator as a presence in their communities even if there is very little evidence that the anthropologist is doing more than quietly sitting, watching, and talking. They have reacted to the fieldworker as an "invader" whose scrutiny somehow penetrates the fabric of relationships and private knowledge. Therefore I use the idea of participation in a metaphorical sense, to encompass both the techniques of participation and observation and the conceptualization of research as an active process and presence.

The other part of the investigation consisted of further fieldwork in 1981 in the Mackenzie Delta communities of Aklavik and Inuvik to ask people just what they thought of anthropologists and other social scientists who have generally come to "study" them. My methods are described in Chapter 5. Once people were aware that I, too, was a "studier," they were often reluctant to comment, but nonetheless they were sufficiently fascinated by one who would dare to "study the studiers" that they cooperated generously. It is possible that I would have obtained more "gossip" about past social scientists by pretending to be something else, but I chose not to do this for several reasons. I am a bad actress and northern Native people are acutely perceptive about role-playing; and I felt that to do so would be perpetuating the deceptions that people blamed social scientists of

having practiced on them. I also did not want to collect gossip, as this was an invasion of privacy of those who had gone before, but I did feel that I obtained a good idea of the range of reactions held by Delta residents.

I spoke with Native and non-Native residents, with those who had met anthropologists (and other social scientists doing similar investigations), and with those who had not. One result of my inquiries was the realization that I needed to understand the entire context of social research in the North, including the sponsorship and utilization of research. The "problem" of study and overstudy was inseparable from the general experience of northerners with southern-based governing institutions, and I felt that I needed further information on this. The result was an expanded case study which has included data on this broader context.

A final aspect of the study involved interviews with many of the social scientists who had worked in the Delta and in adjacent regions of the North. This was supplemented by research in the Public Archives of Canada on government correspondence and records on northern social research, particularly that sponsored through the Department of Indian and Northern Affairs.

The product, the thesis to follow, integrates the two portions, the general treatment of participant-observation and the case study. In this chapter, the concept of participant-observation and its history are explored. In Chapter 2, I outline the rules of P-O as developed in the anthropological tradition and the way in which they are assumed to lead to the creation of an ethnography or an analysis, referred to here as a "text" of interpretation and knowing. Monist and dualist traditions of knowledge and their influence on P-O and the means of "knowing" another culture are also explored, with the result that some central problems with the concept of participation are identified. The discussion focusses on the ideas of objectivity, relativity, and culture in relation to participation, and suggests that the development of these ideas in both dualist

and monist traditions is fraught with confusions between ontology and epistemology. An alternate framework for viewing participation through these ideas, and which attempts to separate the means of knowing from what is known, is suggested at the conclusion of the chapter. This framework is derived from, but presents modifications of, the monist tradition, and it suggests that the intersubjectivity of field participation in locating and evaluating cultural "texts," or versions of local reality, be recognized and used in methods of participation.

Chapter 3 takes the reader into the case study. A brief description of the aboriginal peoples of the Delta begins the chapter, followed by a discussion of the successive entries of outsiders who effected changes in aboriginal culture and society, including researchers. Beginning with the first explorers, fur traders, and missionaries who visited the Delta, a series of southerners have visited the North for various purposes, taking with them ideas about culture which have affected the way they have regarded northern peoples and how northern peoples see themselves. The chapter follows these ideas through to the 20th century government workers who came North in large numbers by mid-century, and who established non-Native enclaves in Delta communities (particularly Inuvik). A capsule history of the development of government presence and of the town of Inuvik is included, but I have omitted comprehensive descriptions of the Delta communities themselves. My purpose was to show that the "removal" of the village of Aklavik to the government-planned town of Inuvik began a pattern of distrustful response by local people to government intervention in the Delta; this was to have consequences in the ensuing mistrust of scientists.

The fourth chapter is included to give the reader an idea of the major institutions involved in the sponsorship, conduct, regulation, and use of social science in the North, including the federal and territorial governments, academic and Native institutions. Chapter 5 traces the history of social science methods in the Delta (and, to some extent,

elsewhere in the North), with an emphasis on how the concept of participation was defined and used by anthropologists and other social scientists who came North as "studiers" (local term), students and/or professional researchers. This evolved from the long-term practical involvement of the explorers, to a pattern of classic P-O undertaken in summer seasons, to a more recent trend to consultancy and hearings as investigative methods. These methods have been noted by northern residents, because they have set the rules by which they have acted as hosts, teachers, and informants in the evolution of the portrayal of the North and its culture by outsiders.

The sixth chapter presents the substance of what northern people told me about participation. I found that many individual researchers were liked and respected, but that in general Native people, and some long-term non-Native residents, have come to resent the whole pattern of forced participation by southern institutions in northern life. The phenomenon of northern science is seen as part of the political process by which the government learns about the North and proceeds to continually impose new policies and programs; this really began with the first "participants" who tried to understand Native culture and transformed it in the process. Anthropologists in particular are rejected because they have come to symbolize the worst of this invasion: the loss of privacy and cultural integrity, the failure of southerners to understand northern life and to listen to what is said to them, the failure to involve northerners in their own destinies, and the failure to use information about the North in a way that both involves and benefits northern residents. There was in effect an ethical dilemma here which joined politics, truth, and morality; when social science failed to meet local standards of accountability to people, it could not be true, and thence could not be productive.

Chapter Seven draws back from the Delta to outline the general context whereby social research in the North is regulated, thus elaborating this political and ethical

dimension of science. Several aspects of regulation are outlined, including licensing procedures, the past and present utilization of social science results by sponsoring agencies, and the development of coordinating and prioritizing frameworks for northern science. The perspectives of government, academic, and Native people on research regulation are compared in regard to access to resources, research priorities, concepts of participation, accountability of results, and validity of results. The chapter ends with a description of community-based research and the potential for Native people in communities, in the North and elsewhere, to take a greater role in all aspects of the research process.

The last chapter, the conclusion, links the case study with the theoretical portion of the thesis. The growth of participant methods, the response to that method, and the political dimensions of regulation all have contributed in the Delta to the expectation that the researcher should be primarily responsible for the validity of knowledge through his/her methods and dispositions of results; this is seen as similar to the expectation within the rules of P-O, in both dualist and monist versions, that the individual's epistemology is responsible for the location and validation of the ontology of culture and meaning. Ethics is explored as an aspect of fieldwork in which conflicts arise about the researcher's accountability to the rules of P-O and to the rules of his/her hosts, and a brief discussion of a northern Ethics Code is included. It is suggested that the alternate framework of Chapter 3 can be adapted to help explain the way in which the "problem" of anthropological images has developed and may be resolved. The alternate theoretical framework, like the ethics framework suggested, recommends that the active role of hosts in the research process and "knowing" and validating knowledge be acknowledged. Regulation as political process is integral in fact to investigation and interpretation and needs to be recognized as such in such a way that the contributions of hosts in

"participating" in the broader institutional context of research design, execution, and utilization be facilitated.

At the outset of the introduction I posed the question of how the Delta is alike or different from other research areas. I was obviously unable to fully answer this question, since I did not do similar investigations elsewhere, but it informed my queries through the research process. The case study is not intended here to "represent" all field situations; in some ways it is unique, and in other ways it is similar to the evolution of colonization elsewhere. It is possible that in areas like northern Canada where the building of rapport has often been a hesitant process (for both political and cultural reasons) that anthropologists have been more concerned with the meaning of rapport than they have in other areas. Indeed, the theoretical framework I have applied, and the questions about participation and regulation I have posed, are suggested to be useful in explicating other settings, and for comparing method and response. I took one situation and used the framework to try to understand it, and this could be done elsewhere for comparative purposes.

Several notes about thesis construction should be made before proceeding. Although my researchers focussed on the Delta, I found that it was difficult to draw rigid lines between phenomena and institutions here and elsewhere in the North, in some cases. I have tried to indicate where I am referring to the Delta only, and where I am referring to policy and practice in other areas of the Canadian North. Similarly, the term "northerner" is used to refer to all northern residents. Where appropriate, I have referred to Native and non-Native northern residents separately.

In addition to references to published literature and archival material, the reader will find references which look like this: (p.c. Oct. 6, 1981) and (researcher p.c. Oct 5, 1982). The first refers to a personal communication, usually an interview, with a resident of the Delta during fieldwork. The second refers to a communication with

a researcher or government official connected with northern science. Dates refer to the times when the communication took place. Names are not used to protect confidentiality. A list of people who provided information is included as Appendix II. Many acronyms and abbreviations also appear in the text. I have tried to spell out full titles at the initial usage and periodically thereafter, but the reader can refer to Appendix I to find the full title for each acronym.

Throughout the rest of the thesis I have primarily used the conventional expression "we" to refer to ideas generated by the writer; the usage is intended to invite the reader to participate in the evolution of ideas. In the description of methods I have reverted to the use of the pronoun "I" to describe my personal involvement in the research. I have also taken the liberty of using the masculine pronoun "he" in most instances which might refer to persons of either gender; occasionally "she" is substituted. The use of either pronoun in a non-specific sense should not be construed as a limit on the gender implied.

One of the primary actors in the thesis is the Department of Indian and Northern Affairs. This is referred to throughout as either DIAND (Northern Development), an earlier designation, or as DINA, a later one. At the time of writing the official name of the Department was Indian and Northern Affairs Canada, or INAC, but since DIAND and DINA are still more widely used acronyms, I have not changed the designation here. A final note relates to this. Most of the original and library research for the thesis was done in 1981 and 1982, and the data presented should be taken as relevant for that time period, particularly in the first two chapters and in Chapter 6 on response. The final writing for the thesis took place in 1988 and 1989. In some cases where I felt it was important I have included information from the period 1982-88, with a reference date given to indicate the time period from which it was taken. This has resulted in some unevenness of reference, but the reader should be aware that the focus is primarily on

the early 1980s, that few of the substantive issues have changed remarkably from 1982-88, and that updating has taken place in matters of fact and detail where relevant.

Certainly my original questions are still important; there is still widespread concern in the North about the impact of social science and its methods. On August 23, 1986, an article in the Toronto Globe and Mail (Steed 1986) on the community of Snowdrift, NWT, reported widespread resentment of an anthropological study done in 1959-60; although the reasons are not clear, it seems that the resentment stemmed from a portrayal of aspects of community life and history that the people would prefer the general public not to know about. An interview with the anthropologist yielded similar confusion: 'perhaps it was because I drank with them,' he said, 'but I had many friends there.' Nonetheless, one resident reported that if the anthropologist were to return today, he would be "trussed up, tarred, and feathered." Indeed, one researcher was recently forced out of the community. A book by Kenn Harper (1986) on the life of Minik, a Greenlandic Eskimo, includes an emotional critique of the treatment of Inuit by social scientists earlier in the century. And a survey conducted through the Science Institute of the Northwest Territories in 1986 (Lange 1987) revealed that Native organizations still identified anthropologists as the most dangerous and disliked of social scientists, in spite of the fact that the amount of anthropological social science in the North today is far less proportionally than it was 20 years ago. A listing of 1988 summer researches licensed by the Science Institute reveals that only 23% were in the "human and health sciences," for example (Science Institute 1988).

B. PARTICIPANT-OBSERVATION

In the remainder of this chapter, we will introduce the concept and practice of participant-observation. The emphasis will be on the use of participant-observation as a research method in anthropology, although mention will be made of its use in sociology

as well. In the first part of the chapter, we will briefly describe the history of the concept. The remaining sections will be devoted to an analysis of the fundamental rules of the method, of the cross-cultural response to the method, and of the primary images of the fieldworker which accompany participant-observation.

Throughout this and the following chapters, several terms will be used frequently to denote process and role. Participant-observation will often be referred to as P-O; the terms "fieldwork," "investigation," and "research" will sometimes be used interchangeably to describe the general process by which the social scientist interacts with the community of study, including P-O as an investigative tool. Anthropologists and sociologists collectively will be referred to as "social scientists" although where necessary a distinction will be made. They will be known as "investigators," "researchers," or as "fieldworkers" while performing P-O. The community of study will sometimes be known as "informants," using a common term for the informing and teaching role in P-O, but more often they will be known as "hosts" or even as "teachers".

Participant-observation is but one of the methods which may be used collectively in the course of a social science investigation. In our discussion, however, we will not limit it to the status of a technique. Rather, P-O is described as a set of interrelated methodological expectations about interaction in the field and its outcome, and which combines epistemological with ontological concepts about the nature of reality and its discovery. Hence P-O has personal, social, and cultural meaning for the fieldworker who seeks to learn another culture through his own; it is method underlain with assumption of theory and ideology.

1. Defining Participant-Observation

There has been a spectrum of definitions of P-O in anthropology and sociology, corresponding with the range in the way that the method is performed. Participant-observation is a way of interacting with a culture or subculture or culture other than one's own, but it can involve varying degrees of participation versus observation. In some cases, one observes cultural life from a distance; in others, one participates in some of the hosts' activities; in special instances one tries to immerse oneself in the entire cultural pattern of living. In all cases there is communication and interaction for active learning, rather than passive reception of knowledge. Information is processed by the fieldworker during interaction.

Although the anthropologist Bronislaw Malinowski did not "invent" participant-observation, he is generally regarded as the first advocate of the centrality of the method to the theory of anthropology. He described the "ethnographer's magic" in 1922 as "the patient and systematic application of a number of rules of common sense and well-known scientific principles" in fieldwork, including "living right among the natives" and applying a "number of special methods of collecting, manipulating, and fixing his evidence" (Malinowski 1961 [1922]: 6) in order to grasp the native's point of view" (ibid.: 25). More recently, G. Berreman summarizes these rules as "the practice of living among the people one studies, coming to know them, their language, and their lifeways through intense and nearly continuous interaction with them in their daily lives" (1968: 340). These definitions stress participation, but this activity is broadly construed as "living," thus employing a continuum of learning actions.

Within sociology, the derivation of P-O comes only circuitously from Malinowski's pronouncements. Early definitions emphasize the cautious, "systematic" sharing of hosts' lives through role-playing and manipulation of circumstances, whereas many of the more recent ones focus on the cognitive and emotive "experiencing" of people and settings obtained through this sharing:

The observer of human events listens to how persons in given situations present to themselves and to others...the "realities" and contexts of their lives. Meanwhile, he correlates what he himself sees with what he hears from those persons who stand in different relationships to each other and to the whole situation. The observer is then able to develop an abstract, logical, and empirically grounded representation of the observed situation (Schatzman and Strauss 1973: 13).

Whereas fieldwork in anthropology has become associated with non-western cultures, sociological fieldwork is more often carried out among subcultures of the fieldworker's own environment; and its methodological guides place more emphasis on the social process of learning than on its cultural creativity. Many of the rules and the objectives of the method are shared, and indeed have been cross-fertilized, between disciplines, however.

Participant-observation, as collectively defined by practitioners in social science, has a core of defining features which marry method with theory, and which we will briefly identify before going on to the rules of its practice. These features will be further explicated in discussion throughout the thesis:

1. The investigator enters into a field of social relations, the natural context of culture, and, like any member of the host community, manipulates these relationships to interact and to obtain particular information through interaction.

2. The investigator must structure these social relations in such a way that he is a mediator between his own and his hosts' communities; the marginality which ensues from this intermediary positioning tends to structure both access to and processing of information. This is the "duality" of P-O, by which the investigator simultaneously enters these social relations through participation and stands apart from them.

3. Participant-observation is bounded by a set of rules for its practice, but which themselves display an emergent, flexible quality. In other words, the

investigator enters the field with presuppositions about how to structure interaction, but the very process of interaction modifies and defines the rules further.

4. Participant-observation is essentially a circular inductive method, whereby the researcher induces generalizations from the data and then tests them in the field, through further P-O, for validity and applicability. It combines scientific principles of empirical observation with the intuitive assessment of situations and the intersubjective creation of conclusions.

5. Because of this combination, participant-observation is often conceived as the creation of an environment for the employment of specialized techniques, such as questionnaires, archival research, or formal interviews. P-O can be used to "test" results against the daily requirements of experience and living.

In the next section, we will summarize the historical development of P-O and its defining principles. Then, we can identify the primary rules of practice and the responses of the hosts which have helped define those rules.

2. History

Most historians of the method of participant-observation agree that it emerged in its classic form only within the current century as the discipline grew professionally and academically. It is also customary to trace the roots of the tradition of systematically observing another culture back to the Greeks, particularly to the Greek-Persian scholar Herodotus of the 5th century B.C. Herodotus shared Greek philosophies of the "natural" integration of societies, but, unlike his peers, he had a keen interest in how cultures other than his own were constructed and vitalized. His Histories of the Greek-Persian War contain observations of the ethnology of the peoples among whom he travelled which in turn reflect elements of the P-O method: objectivity, intuition, and curiosity (Voget 1975: 5-7; Hodgen 1964; Malefijt 1974: 5-7; Darnell 1974: 13-4; Rowe 1974: 62-4).

The travels of Herodotus did not set any lasting precedents, for although the extended period of the Roman Empire through the European Middle Ages produced some commentaries on the cultures of the exotic, these were not founded upon intentional observation. This lack was not the result of failure to encounter other cultures, since the growth of trade, the repelling of barbarian tribes, and the spread of the Christian faith entailed sojourns like that of the famed trader Marco Polo in the East in the 13th century, but, as Hodgen (1964: 102) has pointed out in her analysis of the period, there was little curiosity spared for revelation by most of these travellers. The compendiums of knowledge produced in medieval times mixed fact and fantasy, exploiting popular interest in the exotic and grotesque but making little progress in either tools or theories of observation.

The Renaissance in Europe of the 14 to 17th centuries marked a transition into the scientific explorations and explanations of cultural diversity of later periods, as the philosophies of the Greeks were embraced anew and the Age of Discovery made an economy out of the discovery and exploitation of other cultures and their riches. Data began to come to the writers and scholars of Europe from explorers, traders and missionaries who travelled the seas east and west, and from the captives they brought back home to make claims to the royal families of the Old World. The observations varied from the bias of the fantastic to the cautious descriptions of appearances and behaviours, and the writers of travel books began to organize them under the systematic categories used by natural historians (Oswalt 1972: 12-8; Rowe 1974: 61-76; Fowler 1975: 15-22). During the Renaissance, new societies of man were gradually placed into the monogenetic order of the Great Chain of Being and science began to use the data to contemplate renewed theories of natural order and human development -- whereby the "savage" of the colonized lands inevitably fell below the European in advancement (see Hodgen 1964: 386-426; Lovejoy 1964). Michel de Montaigne of the

16th century and John Locke of the 17th were prominent among those who advocated a more relativistic interpretation of the data, inaugurating an alternate trend toward the belief that the "savages" embodied particular and environmentally suitable cultural adaptations, even purist ones which Europeans could well emulate (Honigmann 1976a; Malefijt 1974: 46-8; 66-70).

By the late 17th century the growth of science had entered a new phase, now deemed the Enlightenment, wherein anthropology as a discipline truly began to emerge theoretically and methodologically from casual observation and ordered compendia of data. Field observations, still made primarily by traders, missionaries, and explorers, improved in quality and quantity, sometimes assisted by field guides produced by natural historians and antiquarians. The writings of the explorer James Cook, and of the Jesuits in North America, are notable examples from this period; there were also studies "at home" of folkloric remnants and English social classes (Oswalt 1972: 18-41; Slotkin 1962: 225; Fowler 1975: 18-9). It was, however, the emerging rationalist belief of the Enlightenment in human unity, evolution, and perfectability which primarily led scientists to the development of the foundations of fieldwork in anthropology and sociology.

Many thinkers, such as the French sceptics (the Encyclopedists), rejected theological interpretations of a pre-ordained "natural order" and began to argue that, through reason, all men have the capacity to improve culture and society along the evolutionary path of predictability. Science, including fieldwork, was seen as the prediction and realization of this development, and the debates surrounding the place of the "savage" or "primitive" in the progress of evolution sparked the formation of theoretical evaluations which would guide P-O. There were ambiguous messages about the nature of the primitive. He was like the European, yet different; he was inferior, and temporally prior; and in his society one could find some of the elements of

individual equality and freedom which would also be found in the eventual cultural utopia. S. Diamond (1974) asserts that cultural anthropology, and particularly the P-O tradition, has antecedents in this Enlightenment paradox as scientists began to compare and derive the Other in relation to themselves, both in the field and in the study. These "enlightenments" gradually steered theorists toward a recognition of the concept of culture as being non-somatic yet a powerful shaper of social form: the potential environmental flowering of diversity from unity.

In the 19th century, anthropology came into its own as a discipline, and as an emerging profession. Professional societies emerged; and an institutional support structure for the collection and analysis of data was formed. A background for research was formed by the colonial expansions of western powers overseas, and the discipline was first, though temporarily, united through the theoretical paradigm of evolutionism. Scientific attention shifted from systematic observation and classification of man as one of the natural forms to a more intensive look at the nature of historical man himself and his culture (Foucault 1970).

The doctrine of evolutionism, espoused in various forms in Europe and the U.S. by scholars such as Henri Saint-Simon, Auguste Comte, Edward Tylor, Lewis Henry Morgan, Herbert Spencer and John Wesley Powell, improved upon the Enlightenment doctrine of unity and perfectability by suggesting that all cultures of men could evolve, given a fruitful environmental stimulation, into higher forms of social development. Field data were used as positivist, empirical supports for the classification of societies into stages, and the biological evolutionary theory of Charles Darwin late in the century were used to back culture with physical and mental development. Evolutionism was countered, however, by the growth of the ambivalences also created in the Enlightenment: while some scholars used it to account for diversity, others argued that diversity was the product of finite forces of race, history, ability, and circumstance. The

positivist evolutionary theories centered in England were accompanied by the growth on the Continent of an interest in idealism, historicism, and relativism, accompanying the Romantic movement. Both mechanistic and phenomenological frameworks were constructed as questions continued to be asked about the relation of the culture of the Other to one's own culture (including the transformation of one to the other), and the frameworks themselves recommended more intensive fieldwork (Harris 1978: 60; Honigmann 1976a; Stocking 1968: 111-32; Malefijt 1974: 98-115).

Fieldwork expanded, again, through the efforts of missionaries, traders, and explorers, but also through the organizations and institutions of the emerging discipline. Ethnological societies were formed in Britain, France, and the U.S., and, with the museums and universities, began to sponsor researches in the western and non-western worlds. Since most work was done by self-trained amateurs, like Lewis Henry Morgan and Henry Schoolcraft in the U.S., the recognized need for guidance sparked a series of field guides, beginning with the guide to cultural observations produced by Joseph-Marie Dégérando of the Société des Observateurs de l'Homme in 1800, and culminating in the sophisticated guide produced by the Anthropological Institute of Britain (Notes and Queries on Anthropology) in 1874 (Urry 1972: 45-7; Stocking 1968: 15-41). Most field inquiries were guided by an empirical, inductive approach, using observations and interviews with key informants. They were encouraged by the need to salvage information from societies damaged by colonialism; these data could be used to reproduce earlier stages of cultural development (Freilich 1977a: 6-7; Oswalt 1972: 82-90). For example, John Wesley Powell of the Bureau of American Ethnology (Smithsonian Museum) sent fieldworkers to American Indian tribes to do such salvage work.

As they did so, however, they gradually realized the relative need to place field information in a living cultural context, necessitating both a change in theoretical

orientation and, for some, a more participatory, intensive style of fieldwork. Franz Boas was to become a leader in this change, as he combined geographical and anthropological theoretical training with the realities of the field encountered first among the Baffin Island Inuit (1883-84) and later among the Indians of the Northwest Coast (1886-1900). Although Boas' fieldwork continued to be oriented more to the typical method of the time -- observation and the use of key informants -- than to actual participation, he used his background in German idealism and relativism to challenge the inductive separation of fieldwork from theory (Stocking 1968: 136-60; Rohner 1966: 208-12; Kardiner and Preble 1961: 12-39; Bunzel 1960: 403-4). Boas' encouragements to look at field data within the natural cultural context of their existence, rather than to place them into pre-ordained schemes of development, were to be an important stimulus to 20th century P-O as empirical and phenomenological paradigms met in the search for the Other in the process of knowing itself. We will further define these approaches in the next chapter.

The form of P-O that we will describe in this and the next chapter arose in the present century as anthropology absorbed these theoretical ideas and perpetuated itself through university training and widespread field research. The method was given its present name, "participant-observation," by Joseph Lohman, a sociologist, in 1937, and by that time it had achieved the fundamentals of the form we describe. This form recognizes a need for long-term, intensive involvement in the culture of study, using rules which blend the intuitive arts of the subjective with the scientific rules of the objective which had their roots in positivist Enlightenment science. Used by both sociologists and anthropologists, P-O became a hallmark of anthropology, almost a rite of passage into the discipline, despite the fact that it was not the only research method used (Spradley 1980: 3-4; Agar 1980b: 1-3; Freilich 1977a: 15; Georges and Jones 1980: 147-48; Briggs 1973: 24-7).

It was British and American anthropologists who primarily gave the method its form, building on the accumulating returns of field expeditions to colonial settings in the late 19th century (including American Indians), and on the recognized need to study social form and change in its own setting. These emerging traditions, American and British, sociological and anthropological, affected each other, and were particularly cross-fertilized through the genesis of a sociological "Chicago School" of P-O at the University of Chicago in the 1930-40s. A few of the early methodological mentors deserve mention here.

W.H.R. Rivers, a British physician/psychologist, was a member of one of the most notable cross-disciplinary expeditions of the late 19th century, the Cambridge Expedition to the Torres Straits in 1898-1899. There he did some interviewing and genealogical research, becoming converted to ethnology; he undertook a solitary field trip to India in 1901-02, resulting in a classic ethnography, The Todas. Rivers' own field methods revolved primarily around the surveying of chief informants rather than participating in host culture, but he did follow some of the basic rules of P-O. He came to realize and publicize the need for this kind of involvement, ideally one to two years of residence among one's hosts, in order to perceive the integration of the elements of society and to test the validity of one's results. Rivers contributed to a 1912 revision of the field guide Notes and Queries, including specific advice on techniques, role playing, and the collection of genealogical information (Urry 1972: 51; Slobodin 1978: 25-30, 46-7; Lowie 1937: 172; Kuper 1973: 19-20).

Rivers went on to teach other British anthropologists, then, about method. Another scholar who was to have great influence in teaching the method, although not necessarily practicing it, was A.R. Radcliffe-Brown, who was himself trained by Rivers. He did fieldwork early in the century in the Andaman Islands and in Australia, again primarily interviewing of key informants. His primary influence was not so much

in the realm of technique, however, as in theory. Here we must look at the alternative to the evolutionary theories which evolved in the 20th century, structural-functionalism. Like the evolutionists, Radcliffe-Brown and others believed in the existence of empirical natural laws for the operation and integration of society, but he perceived the primary purpose of fieldwork as being the discovery of these laws. Only by a close look at the functioning of a host community, through P-O, could these be truly grasped. Radcliffe-Brown taught this theory, and the method which, to him, emerged from it, to students in Britain (Oxford) in the 1940s, and to U.S. students in the 1930s at the University of Chicago; hence his influence was widespread.

By the 1920s and 1930s, then, functionalist theories become popular, and with them came a stimulus for more participatory field methods. The scholar most associated with the new P-O is Bronislaw Malinowski, a Polish scientist who was also to influence both U.S. and British anthropology students as anthropology grew in the universities. Malinowski did his fieldwork in the Pacific, in Papua New Guinea and the Trobriand Islands. Although he realized the value of living among his hosts, he was not inclined to do so until he was stranded in the Trobriands by World War I, and was forced to live for a year's time (1915-16) among the 'natives'; he repeated the experience a year later. His ethnographic descriptions of the Trobriand Islanders became classics, and in them he advocated and described in detail the method of P-O he used when living with his hosts.

It is difficult to sum up these contributions in a few lines; for convenience we will call Malinowski's approach to P-O "contextual." He combined the pre-existing inductive method used by science with the deductive logic of functionalism, perceiving culture as integrated and layered; field research would unveil these layers, leading ultimately to insight into culture: "how the 'natives' think." Good P-O would involve participation in daily activities, learning the language, extensive visits, role-playing,

and the careful recording and cross-checking of data. Malinowski, unlike Radcliffe-Brown, emphasized and popularized the concept of culture as the vehicle and the objective of P-O; by doing so, he consolidated ideas of functional integration and ideational cohesion which became ideological as well as structural foundations of P-O. And, he injected the idea that the fieldworker was not a passive recorder alone, but indeed an active strategist in the creation of culture through his participation in context. Text -- the fieldworker's interpretation of culture -- emerges from the context of his involvement in that culture; culture, to Malinowski, resided in the collective intelligence of his hosts (R. Wax 1971: 35-7; Oswalt 1972: 65-8; Kuper 1973: 26-50; Young 1979: 1-9; Malinowski 1935, 1961 [1922]).

There is debate about whether functionalism emerged, in this century, from intensive fieldwork, or vice versa (Oswalt 1972: 65-8; Burridge 1973: 34-7; Young 1979: 9). Certainly it was compatible with the method, and served alternately as impetus for data collection and rationalization for interpretation of data. Malinowski did not invent either the theory or the method. Like other scholars of the day, he taught method through theory rather than technique, but he brought them together in an ardent and flamboyant manner which impressed itself upon students at the London School of Economics and elsewhere who were eager to listen, eager to encounter the Other in the exotica of colonialism; hence Malinowski's widely acknowledged contribution to the propagation of the method (Powdermaker 1966: 33-40; Kaberry 1957: 71-91; Xiotang 1980: 116-17; Jarvie 1964: 2-3; Firth 1981: 122-27; Leach 1957: 119-35; Rosemary Firth 1972: 11-2). Some of these students, like E.E. Evans-Pritchard, went on to more fully explore the potential of P-O as participation and insight into culture, rather than as simply a reflection of a theoretical orientation.

Malinowski's ideas also spread to the U.S., reaching an audience prepared for "culture" by the work of Franz Boas. Boas never ruled over a "school" of anthropology

or P-O, but he brought both into the universities, sending students from his courses at Columbia University out to other institutions; they (anthropologists like Robert Lowie, Margaret Mead, Ruth Benedict, Clark Wissler, Paul Radin, Edward Sapir, and others) in turn taught it to further generations in Canada and the U.S. Boas encouraged his students to do systematic fieldwork encompassing all four sub-disciplines. Like Malinowski, Boas described the concept of culture as an integrating process; he also stressed the relativity of culture to context. But his approach to the merger of theory and method was more "textual", and, like the contextual approach of Malinowski, Boas's teachings have been influential in shaping the version of P-O that emerged in North American anthropology and sociology.

For Boas, culture resided more in the individual bearer than in the collective entity. Careful fieldwork entailed close attention to individual "texts" or informant accounts which could thus reveal the rules of culture with a minimum of abstraction and representation. His students recall that he told them little of technique, but much about the scientific attitude of natural history, encouraging the careful, inductive, bias-free collection of data - an approach begun by American predecessors like J.W. Powell, with whom Boas disagreed on theory (Marian Smith 1959: 51; Mead 1972: 140; Freilich 1977a: 11; R. Wax 1971: 30-5; Stocking 1968: 155, 204; Boas 1960). P-O was just one technique to be used when appropriate, he taught, and it was often most appropriate for meeting and exploring individuals and their "texts." In particular, Boas utilized key informants and the rules of rapport and reciprocity (Rohner 1966: 209-12). In his own fieldwork he participated in Indian dances and feasts, but his primary immersion was always in the texts rather than in the societies. Fieldworkers should be cautious about the imposition of theory on these texts during P-O, he taught; theory will emerge gradually from the conclusions of data and method but should not ignore the integrity of the primary interpretations of hosts themselves. He cautioned against rigid

adherence to any paradigm, such as evolutionism, which failed to respect the relativity and historicity of individual cultures; thus Boas was less an advocate of theory than of the application of science and insight into its determination (Landes 1970, 1973: 44; Mead 1959 a: 12-16,b; Herskovitz 1953: 66-7; Radin 1933: 3-29). It was only later, through the endeavours of his students, that American P-O took the stamp of inductive objectivity and added Boas' ideas (and those of Malinowski and others) on the subjective validity of culture to produce a monistic field method which hinged on the fieldworker's intersubjective creation of cultural texts through interactions with hosts, and on the replication of the internal validity of culture through the external objectivity of the observer.

By midcentury the work of these mentors had produced a version of P-O taught and practiced by sociologists (beginning with the University of Chicago structuralists) and anthropologists. Some of the rules, such as those pertaining to role-playing and reciprocity, were explicitly taught and adhered to, whereas others, which focussed on the creative task of the researcher, were informally understood and taught more by example and theory than by classroom guidance (Freilich 1977a: 12). Not only did P-O need to be flexible to accommodate the varying circumstances of hosts and fieldworkers, but it gradually had to respond to a divisive burgeoning of theory; this latter contributed to an increasing emphasis on the integrity of method as access to culture rather than as merely an access to any particular theoretical persuasion.

By the 1960s, the increased availability of funding and university access produced scores of students in the U.S. who went to all areas of the globe to rescue cultural information from societies undermined by the crumbling colonial empire. P-O began to be taught more explicitly as method; it became popular as the hallmark of anthropological research, and resourcefulness in the field was applauded. But, as numbers of researchers in foreign cultures increased, and as these societies gained

independence, they began to respond to anthropologists in their midst, calling in the 1960s and 1970s for accountability in fieldwork. This response, in combination with the growth of phenomenological paradigms in anthropology, led to a "self-consciousness" within anthropology (and sociology) wherein practitioners began to seriously examine both the method and its underlying ideals (Nash and Wintrob 1972: 528-32; Emerson 1981: 352-53, 1983:9-11). Some concluded that fieldwork needed to become more scientific and objective (Honigmann 1976b), whereas others explored the role of the researcher's subjective bias in cultural discovery. Most have taken an intermediary stance, declaring that P-O is both art and science, with its own cultural rules, and that its success depends upon awareness of the researcher's marginal role as both scientist/outsider and participant/insider -- and the insights afforded by this role. It is this interpretation of P-O that we will pursue more intensively in the thesis.

One final note on the history is necessary before undertaking an examination of the rules of P-O, since our case study is set in Canada. Canadian anthropology, like American, began with the observations of missionaries, traders, and explorers. By the late 19th and early 20th centuries, self-trained ethnographers (amateurs and scientists) like Horatio Hale, Marius Barbeau, and William and George Dawson were making notes on Native Indian and ethnic cultures. Some researches were done by foreign expeditions, like the Danish-sponsored Fifth Thule Expedition in the Arctic, and varied explorations sponsored by the U.S. and British governments. The British took a particular interest in directing researchers in their former colony. Some of the earliest systematic investigations were encouraged under the auspices of the Geological Survey of Canada, which was a partial sponsor of expeditions like the Canadian Arctic Expedition of 1913-18 under V. Stefansson; this one produced ethnographic studies of the Inuit by Stefansson and Diamond Jenness (see Diubaldo 1978).

In 1910 the Geological Survey instituted an Anthropology Division, and Boas in the U.S. was consulted about personnel. He recommended Edward Sapir, one of his students, who came to Ottawa and brought the Boasian four-field subdivisions of anthropology to Canada during his tenure from 1910 to 1925. Sapir outlined a program of survey and P-O researches intended primarily to salvage Indian cultures and languages; the Royal Ontario Museum also sponsored some projects. Under the influence of the museums and the British ethnological societies, the discipline thus developed with both British and American characteristics by the time it entered the universities at the University of Toronto in 1925 (see Cole 1973; Darnell 1976b; Carpenter 1979; Preston 1980, 1983; McFeat 1976). Anthropology did not gain much stature in the universities until mid-century, however, and, while Canadian anthropology has been praised for its productive researches, it has not, according to some critics, developed a distinctive Canadian perspective in either method or theory (Maranda 1983; McFeat 1980; Preston 1983). Instead, references to the growth and development of P-O in these chapters can be said to include Canadian examples.

3. Rules

As P-O developed a distinctive existence as a method, and became widely practiced in sociology and anthropology, it incorporated a set of implicit and explicit rules for its practice. Some of these came to be taught as explicit directives of technique and strategy, whereas others were implicit assumptions shaped by the theoretical paradigms which influenced the development of P-O. In this section, some of these rules will be outlined, with a brief explanation for each; it is not possible to detail their full development here, however.

A. *One can only learn participant-observation by doing it.* We mentioned in our historical sketch that it was not until at least mid-century before P-O began to be taught, at least in American universities, as a set of methodological rules which could be applied flexibly to varying situations. Before that time, it was generally believed that, if a student was trained in general science method, if he was observant and adaptable, and if he was familiar with applicable theory, then the student could "learn on the job" how to do P-O. The rules of method would flow from its practice. Teachers such as E.E. Evans-Pritchard believed fieldwork was best taught, then, by the application of theory and the worth of personal experience: the sink or swim approach (Beattie 1965: 5; Middleton 1970: 2-3; Nakane 1975: 16-7; Beals 1970: 32-3, 38). Students were given only sparse, practical advice and some have since confessed their own ignorance of field methods when they began fieldwork (R. Wax 1971: 66; Norbeck 1970: 245-46; Berreman 1962: 4; Nader 1970).

B. *Upon "entering the field," one must establish relationships with several key persons, including local leaders or officials who will grant permission or sanction, key informants, and interpreters/assistants.* There is a substantial segment of literature on P-O devoted to the process of field entry, once the student/researcher has decided upon a field research site. This includes both official and unofficial permission to work in the community; initial contacts may be important in legitimation, in establishing factional liaisons, in arranging one's status and roles, and in finding assistants, informants, and mediators.¹ Finding and establishing relationships with one's key teachers is also critical to the on-going access to information and knowledge, and, indeed, to the intersubjective activity of cultural reconstruction. Often anthropologists have found that their best teachers are people who are perceived as (or perceive themselves as) marginal to their own communities, and thus often more objective,

reflective, and articulate than other residents (see the volume by Casagrande, ed., 1960 on informants). While these relationships are often productive, they may also sever the researcher from ties with other mainstream segments of the society.

C. One must not only establish relationships, one must establish rapport, or acceptance by those individuals on a personal as well as professional level. The building of rapport is both an objective of participatory immersion in the community and a technique for attaining it, depending upon one's perspective. No simple definition of "rapport" exists in the literature, but it is widely accepted as a prerequisite for successful communication with one's hosts. Laura Nader (1970) refers to it as the ability to cope so that work is possible, establishing a personal milieu of effective survival which includes positive communication with informants. Early writings on P-O described rapport as a simple by-product of the role that one plays in the field, but now it is recognized as more than that: both an approval of one's person and a legitimation of one's purpose. This legitimation places one within the natural context of the culture and offsets the "unnatural" effects of one's acts by accepting them as valid -- because the person performs social relationships in a valid manner. In a curious fashion, the anthropologist seeks to neutralize him/herself so that not only is insidership but outsidership acceptable; he seeks this through an effective -- if fictional -- neutrality between himself and his hosts. Although most fieldworkers agree that social and political neutrality and equality are rarely possible for the "foreign" (and often elite or marginal) researcher, rapport is part of the illusion that they can exist in normalized social interactions, and that the researcher is "participating" in these relationships rather than merely observing their dynamics.² Indeed, it may at times be this very inequality, the social gap between teacher and student, which makes social relations possible through a unique rapport of circumstance and status (Mead 1972b: 120-21).

D. One way of obtaining rapport is to immerse oneself in the daily lives of the people, and to learn the rules of behaviour which make this lifestyle possible for group members. As we have seen, the "participation" aspect of P-O can vary from simple observation of activities, to participation in social relationship and the observance of etiquette, to involvement in the daily tasks and festivities of the hosts; some participation is endemic to the process of interaction and learning itself.³ Full immersion would mean becoming a member of the culture indistinguishable from other members; this empathy exists as an ideal of fieldwork for some, but is rarely possible or desirable for either host or guest (Liebow 1967: 232-56; Chilungu 1976; Pelto 1970: 220). The researcher must choose which strategies will be most effective.

To do this, he must learn what constitutes valid participation, learning the cultural rules by which people interact and communicate. This is not only part of the building of rapport, but is simply part of learning culture itself. Along with the participation of becoming an insider comes the learning of the knowledge of the insider. Those who take a structuralist view of P-O stress the participatory immersion of role-playing, whereas symbolic interactionists, on the other hand, emphasize the learning and use of the rules themselves. In either perspective, and regardless of the degree of immersion, use of the daily rules of participation serves as a check on validity and legitimacy of one's findings, tested in conjunction with one's participatory role (Cicourel 1964: 40-72; Sykes 1978: 154-55; Becker and Geer 1969: 133-41; Lofland 1976; Bruyn 1970a: 318-24; Emerson 1983: 14-5; Denzin 1972: 185-216).

E. One should learn the language spoken by the hosts, appropriate to their culture or subculture. This is an obvious rule and needs little explanation. Obviously understanding of language, dialect, and visual communicative cues are integral to effective interaction and understanding. Language learning has been, however, a major task in the past for fieldworkers encountering a language previously unfamiliar to them; according to fieldwork guides and teachings, it is often a first step toward building relationships and rapport, and as such is both a tool and an objective.

F. Various forms of reciprocity are to be used in establishing relationships and rapport, and in negotiating one's roles and access to information and other resources. This is another critical tool of the fieldworker, and often inseparable from building rapport and establishing roles (see *G* below). Through reciprocity in social interchange, the researcher can establish the illusion of equality and neutrality which allows rapport to operate effectively, and the practice of reciprocity may itself be bound by the rules of culture (hence participation). Reciprocity may involve paying or giving gifts to one's informants, or simply sharing of social activities and knowledge, but it is always a two-sided process whereby hosts and guests both negotiate acceptable terms (Paul 1953: 434-41; Pelto and Pelto 1973: 251-60; Dumont 1978: 49-59).

To some extent, manipulation of the terms of reciprocity has been part of P-O method since its inception; early fieldworkers quickly learned what they had to "give" in order to obtain information and cooperation, and they learned how to manipulate the terms of the agreements: even "bullying" informants or researchers (see Nadel 1939: 323-24; Middleton 1970: 64-6; Richardson 1975: 524-27; Lowie 1959: 16-7; Nash 1975: 234). Terms are often conditional on whether the researcher is perceived as dependent upon his hosts, or whether he has an elite position; some anthropologists deny that true reciprocity ever supercedes these gaps (Levi-Strauss 1974: 381-82; S.

Diamond 1974: 69-90). Reciprocity is, however, both part of the mutually-created fictions upon which social relationships are always based (from an interactionist perspective), and an assertion of the independence of each actor to try to control and manipulate those relationships (Geertz 1968).⁴

G. One should play one or more roles viable in the society. We will treat the practice of "role-playing" as a central aspect of participant-observation, and, in the next chapter, we will explore its conceptual correlates. Roles are empirical and phenomenological summaries of the aspects we have already described: rules, rapport, reciprocity. Some of the earliest attempts to describe roles in P-O literature emphasized their static, structural quality as positions held within the natural context of the society which delimited access to information and clarified expectations for behaviour. If one played the role of a European teacher, for example, one could get certain types of information from the hosts, and one would be expected to act in predictable ways (Paul 1953: 431-34; Nadel 1939: 325-26; F. Kluckhohn 1940). As fieldworkers began to explore the complexities of role creation and performance, however, attention shifted to the interactive dimensions of roles, whereby roles were perceived to be relative to the actors' needs and interests, to pre-existing roles and statuses, to mutual expectations, and to circumstances of setting. Roles were then prescribed as negotiations of identity and performance, the setting of rules for consistent expectations, which did indeed structure social relationships so that rapport, reciprocity, etc. could take place; key words were flexibility, legitimacy, credibility, and responsibility as defining characteristics of role performance (Cicourel 1964: 40-9; Haas and Shaffir 1980: 245-52; Fine 1980: 119-25; Agar 1980b: 53, 81-8; Lofland 1976; Katz 1983: 146-48).

By combining these perspectives, we will define roles here as "sets of cultural expectations regarding behaviour appropriate to given activities or events, which may be to varying degrees crystallized around a social structural position." As such, they are construed as (1) part of the natural context of social relations and cultural order; everyone, including the researcher, plays one or more roles in context. They are (2) relative to situation, in that they emerge or change relative to the engagement of the actors involved in role-playing, including the degree of participation a researcher has in the natural context; similarly, a researcher may find that one role she plays may conflict with another.⁵ Roles allow people to predict what will happen if they release certain open or "backstage" information about themselves (3) in these settings; thus they have structural qualities. Roles are usually negotiated, like reciprocity, by the mutual efforts of hosts and guests (4); fieldworkers may be assigned roles on entry to the field and then find that the roles change as perceptions of their purpose and personality change. And, performance of roles may have political or ethical consequences if expectations are violated (5). A researcher who pretends to play an insider role, such as a student in a school, may face censure if deception is suspected.

One of the most important aspects of role-playing is that roles allow one to test the validity of findings, as we discussed in regard to rules (roles can be seen as rule and expectation). If one is able to perform a role in the "natural context" of one's hosts' culture successfully, then one is correctly learning to predict and manipulate behavioural settings, and one can use what one learns from roles to create cultural texts. Similarly, hosts assess the adequacy of the researcher's insight by evaluating his/her role performances. An extensive literature in sociology and anthropology exists on the types of roles one plays in relation to validity.⁶ Some of it relates to overt/covert roles, as writers suggest that some information could not be obtained if hosts are aware of the researcher's "real" role as researcher, so that the fieldworker

must go "undercover" in another role. This is both an ethical and a legitimacy question, and the advisability of either overt or covert role-playing depends upon an assessment of the outcomes in rapport and in validity of results; this latter in turn depends upon the definition of 'natural' context which exists between hosts and guests.

In some situations outsiders have effectively created and used "researcher" roles, as we will see in our case study (Lowie 1959: 60; R. Wax 1971: 79; Carter 1972: 146; N. Diamond 1970: 126-27; Dumont 1978: 43-66; Whyte 1981: 300-05; Berreman 1968: 346, 356-61; Dentan 1970; Read 1965: 97-98). In others, they may take or be assigned to other roles considered appropriate: perhaps a stranger (or merely an unknown European) about whom expectations are only partially crystallized, so that the researcher takes a liminal and insightful role half-in and half-out of the culture which is both threat and intriguing ambiguity (see Nash 1963, Schutz 1944; Simmel 1950; Wood 1934 for extensive discussions of the parallels between the ethnologist and the stranger). The researcher may commonly be deemed the equivalent of a child or a student, someone to be instructed in essential etiquette and who is a potential family member and quasi-insider. He may be a friend to some hosts, thereby acquiring the intimacy of knowledge which flows from that role. And there is a panoply of roles played by westerners in non-western societies to which the researcher may be matched: teacher, missionary, government worker, spy, social worker, healer.⁷ Again, these roles have been described in the literature on P-O, but the applicability of each depends very much upon the actors, settings, and objectives of each situation, so that generalizations are difficult.

H. Time is knowledge. Beginning with Rivers, Radcliffe-Brown, Malinowski, and other methodological mentors, one of the most fundamental rules of the field is that one must spend an extensive time in the field in order to truly get to know the people -- to

build rapport and test validity of interpretations. Malinowski and others have even recommended taking an analytic break after a year and then returning to check one's assumptions. The recommended time frame has tended to be more extended in the British tradition, up to two years or more, whereas in American anthropology field terms are often one year or less. Amount of time relates not only to scientific objectives, however, but to visas/permissions for entry, funding, and the responses of hosts. Extensive stays may not guarantee insight, however, but only endurance (Hayano 1979: 101-02). When we discuss the case study, we will return to the importance of time as a measure of knowledge gained.

1. The researcher must bound the unit of study, in order to study it holistically. Here we reflect upon an implicit assumption in the above discussion: in order to participate in a culture, and hence to interpret it, the fieldworker assumes that a viable, integrated culture (usually different from his own) exists independently and holistically. When P-O became associated with functionalism, it also became associated with the idea that cultures/societies can be bounded as units of study, and this assumption has come to underlie both P-O and the related concepts of natural context and culture. The bounding of the community of study is often a first step in research planning, assessed through the literature and through contacts, but it may change or emerge once the researcher learns more about the situation in the field. In the modern context, where no culture is isolated, researchers may have difficulty separating host communities or groups from their environment, and it may be hazardous to do so in terms of comprehensive insight. But the assumption of integration and cohesion remains regardless of complex circumstances, even if the unit of study is only a single individual, since the researcher must delimit his text or context before interpreting it. ⁸

J. One must maintain a dual stance in the field; not only is one participating in the social relations of the natural context, but one is learning and interpreting the internal validity of of the situation by maintaining conceptual, even social, distance.

K. One must ground one's explanations and understandings in the natural context, yet develop generalized abstractions from it; epistemology is dependent both upon the self as research instrument and upon the sociocultural duality of the social role. These rules also follow from previous discussion. If one assumes a bounded field of study, one often also assumes that there is a "natural" validity to its independent existence. The strategies of role, rapport, etc. are geared to respecting this natural context, so that the observer's presence will not cause "unnatural" effects. Only by perceiving a valid "natural" culture can one describe it in a valid manner, and, as we will see, in P-O the replication of the insider's "natural" validity is often an ultimate objective. The process of reaching conclusions and interpretations about the host culture, the derivation of text from context or epistemology, is influenced by the researcher's dual position as insider (in the natural context) and as outsider (analyzing that context). The burden of replication is placed on the researcher: does his/her (outsider) interpretation have validity to the insider?

The effectiveness of the strategies by which the researcher plays these dual roles in context determines, then, the adequacy of the abstractions made about that context. At the same time, P-O is often judged to produce unique, non-replicable results, since each fieldworker may interpret these roles differently. The fieldworker must discover, therefore, how to "check" the truth of his observations against himself and others in order to make the internal validity of culture externally credible; this is done through both empirical and phenomenological epistemes of cross-checking "facts" via multiple methods and testing rules situationally.⁹

This duality is related to the image and role of marginality which the fieldworker often occupies in relation to the hosts; we have mentioned the liminal role of stranger. Regardless of the degree of immersion into culture that the fieldworker seeks, there is a need for distanced reflexivity in order to comprehend the dimensions of culture (the bounding of context). The fieldworker may be limited in participation also by host expectations that he is fundamentally an outsider (S. Diamond 1974: 77; Pelto and Pelto 1973: 248, 259; Paul 1953: 436-38; Agar 1980b: 58-9, 85-8; Schwab 1977: 69-70; Freilich 1977a: 2; Bowen 1964: 26, 290-91; Read 1965: 6). Even insiders trained as observers must play a dual role.¹⁰ Anthropological P-O. in particular, has made a structural pillar of this a-structural marginality. Titles of fieldwork volumes incorporate the appellation "marginal" and its equivalents (see Freilich, ed., 1977; Powdermaker 1966). Marginality is expressed in field accounts as dual conflicting roles, e.g, stranger and friend, person and scientist, child and adult observer.¹¹ Marginality becomes necessary to retain duality of perspective; both the image and the role are encouraged as badges of the discipline.

L. The researcher must use him/herself as a research instrument.

M. Both problem and method will tend to emerge from the process of doing fieldwork, regardless of prior research design. We can link these rules back to the first one, that the fieldworker must learn through practice, and to rules J and K. Again, the burden is on the fieldworker to balance his subjective intuition, and the relativity of his experience, with the objectivity of replication and external credibility. Sociologists and anthropologists (and their hosts) have both been concerned, since the 1960s, with the concepts of relativity and subjectivity within P-O. To respect the holistic integration of culture, it has been argued, it is necessary to respect the relative

operations of that culture; this functional relativity is often extended to moral relativity, whereby the fieldworker must withhold judgment about his/her hosts. These practices involve suspension of the fieldworker's subjective opinions and attitudes in order to open her mind to the experience of culture, or in other words, to experience culture objectively. Many anthropologists were shocked in 1967 when Malinowski's field diaries were published by his wife, more than twenty years after his death. The diaries revealed the writer's alienation from his own and his hosts' societies, his role conflicts, his stresses, and his prejudices; they also revealed that Malinowski, the arch-proponent of P-O, was probably influenced by personal ideas and intuition (Young 1979: 11-14; Firth 1967; Forge 1972: 292-96). Scientists asked, is it possible or desirable to suspend this subjective element?

One response to this question, in the past 20 years, primarily by positivists, has been to try to make P-O more objective. This is done by training researchers to control biases, and to use alternate methods, like surveys and other formal research tools, to cross-check the impressions of observations and to reduce the unique, non-replicable dimensions of P-O (see Honigmann 1976b). Presumably these methods would also make problem definition less dependent upon the serendipity of circumstance and personality. Clyde Kluckhohn and Margaret Mead, among others, have even recommended psychoanalysis before fieldwork in order to learn to control biases, but most fieldworkers have traditionally been expected to do this without training (L. Nader 1970; Mead 1973; see also Korner 1959; McCall 1969: 128-33; Bruyn 1970a: 305-21).

The other response has been to incorporate the subjective element more fruitfully into field practice, since many fieldworkers have found that, not only could they not suspend all judgment, but that intuition and interpersonal communication played a central role in both discovery and testing of patterns in their own research. 12

One can bring one's assumptions, as well as one's theoretical presuppositions, to the fore and thus become aware of them and, in some cases, make use of them. Awareness can clarify the direction of thought, and can help to reduce the stresses of fieldwork.

We will return to this in the next chapter when we discuss value in objectivity. As phenomenological theories have gained credence in performing and evaluating P-O, however, the awareness of subjectivity has taken on a more creative dimension than that allowed in more dualistic, empirical paradigms. Personal stance is regarded as a vital ingredient in the intersubjective dialogue through which culture is created and utilized. Hosts react subjectively to researchers, and shape their teachings accordingly; the same process must happen in reverse. Rather than interpreting this uniqueness as a drawback of bias, as would happen in a more traditional positivist P-O paradigm, phenomenologists point out that the objectivity of culture is itself an illusion. By participating in cultural creation the researcher can more fully "know" the hosts than he can by distancing him/herself from them in the false impression that culture has existence independent of the participants. In chapter two, we will explore the concepts of objectivity, relativity, and culture more fully, comparing and contrasting these two "solutions" to the role of the person in P-O practice.

N. Successful completion of fieldwork is often hailed as a personal mark of initiation into the discipline of anthropology. We mentioned this point before in our history. By the time P-O became widespread in the 1960s it was often regarded informally as a hallmark of the discipline, and completion of P-O was used as an evaluation of the researcher's personal qualification and abilities -- fieldwork was a rite of passage by which one "proved oneself" as an anthropologist. This was never, of course, a formal requirement of professional training, but observers within the discipline have often stated that they felt the presence of the rule in fieldwork preparation and recounting. ¹³

IV. Response

It has only been within the past 20 years, in the "self-conscious" period of P-O practice and reflection, that anthropologists have begun to pay serious attention to the responses of their hosts to P-O rules. In the first accounts and guides to P-O, it was taken for granted that the hosts would have some part to play in assigning roles, demanding reciprocity, or building rapport. Comments scattered in the literature reveal that this was so, but most of the information we have on host involvement has indeed been filtered through the perceptions of the fieldworker reporting the experience. It is only recently that researchers have detailed this involvement, and that hosts have made direct statements about their responses. As non-western fieldwork became more enmeshed in the political independence movements of the 1960s-1980s, persons in other cultures have often become outspoken about what they expect from fieldworkers doing P-O. This kind of response is a major part of the case study to follow later in the thesis, so that these few remarks will help to build a context for that study.

Certainly the circumstances of fieldwork vary enormously world-wide, and fieldworkers who have worked in several different cultures have often pointed out that the response they received differed according to the experiences and predispositions of each culture. Some had never been studied and welcomed the researcher, whereas others, over-studied, may even have turned him/her away. Always there is some response, however, and the factors of response do show similarity cross-culturally.

i. Hosts help determine roles, and the rules and reciprocity which go with them, and thus set limits on the kind of participation the fieldworker can have. The outsider may initially be placed into a role with which the hosts are familiar, or which other Euro-

Americans may play, as we have seen. Suspicions are not uncommon, especially when the researcher is a stranger and his/her activities are not understood: is he a government spy; is she sponsored by a rival village (Freilich 1977a: 2-3; Agar 1980b: 58-9)? The spy is capable of doing anything from increasing taxes, to changing government policy, to starting factories, to illicit trading, to stealing and eating babies.¹⁴ After all, the fieldworker typically does come from a more advantaged society, and hosts wonder why he would bother coming to their community (Rabinow 1977: 91). But as the outsider becomes more familiar, roles may change, along with demands for behaviour. A "child" may be expected to grow up and act more like an adult; and hosts find out what the limits are on what they may obtain from the fieldworker in return for teaching. A negotiating and testing process is underway as hosts search for the "real" reason for the fieldworker's presence. Eventually, the researcher may gain credibility (although perhaps not legitimacy) for the researcher role itself; many host cultures who have experienced repeat visit by fieldworkers have come to expect certain behaviours, such as question-asking, for this role type (Pandey 1972: 332-35, 1975: 204-09; Fenton 1972: 114-15; Xiaotong 1980: 117-19; Watson 1972: 180).

ii. As noted, hosts may demand a return for their involvement. These demands have always been present, but hosts have become more vocal about their expectations in recent years. This may involve straight payment for services rendered, but it may also include favours, political advocacy, and some benefits from the research itself, such as perhaps a favourable change in government policy. Many fieldworkers have commented in their account on the kinds of requests made and the manipulation that has sometimes accompanied it; Lowie (1959: 59) reported that a Crow Indian friend thought that Lowie would profit from the information, and wanted to share in the profits; this

reaction is common also when informants do not understand the way information is used in the researcher's world.¹⁵ But, as hosts become more like ourselves, they do understand the research context, and their demands for return, particularly for tangible applications of research findings, have become more oriented to this context (Richardson 1975: 526-31; Macquet 1964: 47; Geertz 1968; Nash and Wintrob 1972: 531; see also the volume edited by Hymes, 1974).

iii. As younger generations of hosts have obtained more education and more exposure to Euro-American societies, they have increasingly read fieldwork reports and commented upon their accuracy, or lack thereof. There are concerns that fieldworkers have invaded privacy, have included judgmental biases, and have misrepresented cultures. The first, invasion of privacy, has been a particular concern of hosts from the beginning of P-O. Frank Cushing, whose apparently complete acceptance by his 19th century Zuni hosts has become almost mythical in American anthropology, has revealed that the Zuni Bow Priest society threatened his life on one occasion when he was collecting sacred ceremonial information (see Pandey 1972: 322-26; Gronewold 1972: 33-49; Georges and Jones 1980: 5-17). This "invasion" can have serious implications for both the hosts and the fieldworker; the Omaha believed that one informant had died after telling sacred lore to the anthropologist Alice Fletcher, and Ruth Benedict was accused by another anthropologist of "killing the Indians" by slowly taking away the heart of their spiritual culture in her documentation (Mead, ed., 1959a: 298-314). More recently, informants have reported having been deceived or betrayed by the reporting of private information, and public statements of this are increasing in frequency (see for example accounts by Adair 1960: 491-92; Pandey 1972: 335-36; Golde 1970; Madan 1975: 138-40; Lopata 1980; Watson 1972: 172-73; Fenton 1972: 112-18).

Comments on the accuracy and representation of anthropological accounts are also appearing more frequently. Later generations of Trobriand Islanders have been asked to interpret Malinowski's work, and some have noted that he was not always accurate in regard to social organization (Young 1979: 14-5). These inaccuracies and biases may be interpreted politically as exploitation; hosts have frequently confused the personal and professional motives of researchers, sometimes accusing outsiders of deliberate bias and political manipulation of data results. When hosts have been asked for their comments on P-O methods and writings, misrepresentation appears frequently as a major complaint, as we will see in the case study.¹⁶ It is central to the insider assessment of validity, credibility, and objectivity, and as such is vital to any evaluation of P-O methods and assumptions, as we will see in future chapters.

In sum, hosts have come to demand a more complex kind of reciprocity from fieldworkers, one incorporating both tangible and intangible factors. Some simply turn their back on anthropologists, refusing to cooperate or evincing little interest in the offerings of the outsider. Others get involved by asking for more than payment and favours; they ask for accountability and commitment. By doing this, they are influencing the rules and structure of P-O; for example, some demand greater commitments of time in the field before they will acknowledge that the fieldworker has gained insights (Pandey 1975: 207). Again, the demand for conformity and commitment varies by community and by individual, but a general overview of host response and commentary cannot fail to convince the reader that many hosts are no longer willing to believe in the "illusion" of equality between fieldworker and host that P-O has often rested upon (Geertz 1968; Xiaotong 1980). Each party realizes that he is from a different cultural world than the Other, and that these are not merged in a year's interaction regardless of the amount of personal accord. The illusions may still exist, but they are carefully managed and evaluated by participants.

4 Images

We will conclude this chapter by describing a correlate of P-O rule and response, the images of the fieldworker held by himself (about himself) and by hosts. Images bring rule and response together, revealing their interaction; images are descriptors of role, status, and personal performance, and they tell us how performance and practice is evaluated in relation to culture: fieldworkers' and hosts'. As with response, much of the information we have on images is filtered through the reporting of the investigator. And, although some images are relative to the culture of origin, some image "types" can be identified cross-culturally in association with both application of rules and the general circumstances of political and cultural response.

We can divide those images held by hosts into those referring to status and those referring to personal performance. Status images are descriptors of group social status and membership, and they can denote the degree of acceptance of the fieldworker as like/different from hosts. The most frequent images describe the race, power, and wealth of the researcher. Neither guest nor host was, in many cases, quite able to overcome the fact that the researcher was usually white (Euro-American), came from wealthier social settings, was educated, or was associated with a more powerful class. Some fieldworkers have found that they were continuously distinguished as 'white' when working with non-white populations, a quality which may in turn take on associations of power or liminality.¹⁷ Once a fieldworker is accepted, on the other hand, they may be told, as H. Powdermaker (1966: 148) was by the U.S. blacks with whom she worked, that they are "really" black, or native; Lowie was told by an Indian man that although he looked white, he acted Indian (1959: 46). Fieldworkers have sometimes found, however, that they were never fully accepted by either local native or local Euro-American residents, so that they occupied a marginal status/image as well as role

(Pandey 1982: 72; D. Turner 1979: 21-6; Whitehead 1980: 42-5; Middleton 1970: 67-70; Beals 1970: 46).

There were few generic labels for anthropologists as a profession (rather than personal performance) reported in this literature, however; the Navajo, one of the most-studied North American groups, call anthropologists "leeches" and "summer birds," referring to their transience and their tendency to take information away without reward (Pandey 1975: 203-04). M. Mead reported that the people of the island Manus called them "look looks," in reference to their roles as ubiquitous observers (Mead 1970b).

Personal images held by hosts refer to behaviour and role performance by fieldworkers. Some of the descriptors most frequently encountered are incompetence, foolishness, stinginess, laziness, aggressiveness, and lack of fully human behaviour (lacking a spouse, or religion, or proper gender behaviour, for example). These are obviously evaluations of how well the researcher can meet cultural expectations, or how well socialized he is. More positive images are also found -- brave, generous, friendly -- but in some cases these are idealizations of fieldworkers which appeared retrospectively a few years after the fieldworker's visit, when the individual has already entered the status of local legend (Fenton 1972: 6; L. Nader 1970; Gonzalez 1977: 129). Although the sources of these images, as reported in field accounts, are too numerous to cite, it should be noted that the same kinds of images tend to recur cross-culturally; for example, incompetence and foolishness are frequent labels for one who appears awkward in an unfamiliar cultural setting.

Self-images accord with these personal images; it is often difficult to discern, in reporting, the differences between how the observer saw him/herself and how he thought the others saw him. The emotions one feels, and the prior expectations one brings, help to mold these images, which are mediated by reactions of others to the Self

as the P-O process ensues. A few central images recur in the literature with great regularity: marginality, incompetence, helplessness or inadequacy, and guilt. In 1952 Ann Roe reported on a study of anthropologists, with a small sample of eight; she suggested that anthropologists were predisposed by self-selecting factors of personality and ambition to have certain responses, such as detachment, rebelliousness, and sensitivity (Roe 1952; Nash 1963: 160-62; see also Wintrob 1969). And, again, similar factors of fieldwork practice, including the encounter with the unknown, help also to shape these images.

Marginality seems, for example, to be an inevitable descriptor of both performance and status in the field for many fieldworkers who never feel fully a part of either their own or their hosts' cultures. Such prominent fieldworkers as M. Mead (1977: 194-96), Ruth Benedict (see Mead, ed., 1959a: 84-93), and Levi-Strauss (1974) felt it, among others. Similarly, self-images of incompetence and helplessness are ready counter-responses to host responses to the fieldworker's awkward gestures; it is not uncommon for researchers in some settings to be literally physically dependent upon hosts for survival. Again, references to these self-images are too frequent in the fieldwork accounts to list. The fourth image, that of guilt, arises both from marginal stances and from competing demands on the fieldworker: to be loyal to the discipline, to respond to hosts, to be both involved and detached. The amount of self-expurgating literature exploring this guilt has increased as host demands have become more open and as the discipline has become more self-conscious. Richardson (1975: 527) lampoons, "Anthropologists race each other in their willingness to accept the most devastating criticism." The guilt image is not new to P-O, but it is becoming structured into the pillar of marginality as rationalization for its own being. The image of the participant-observer is thus relative: self relative to Other, Other relative to self, self relative to self. It is worthy of comment because of this relativity and its

relation to the aims and procedures of cultural insight and epistemology, as will be discussed in the next chapter.

CHAPTER TWO: EPISTEMOLOGY AND ONTOLOGY IN PARTICIPANT-OBSERVATION

In this chapter, we will complete the preparation for the case study portion of the thesis, moving from the structure of fieldwork to an examination of the conceptual basis of understanding how it is possible to learn about another culture through observation of, and participation in, its living embodiments. The central concepts of objectivity, relativity, and culture will be analyzed as epistemology and as ontology, as they have been used in P-O evolution in both dualistic and monistic interpretive traditions. Some of the inherent confusions in the uses of these concepts, deriving from both traditions, will be identified, and an alternate framework for interpreting the conceptual framework of P-O will be proposed. This framework will in turn provide a restructured "window" through which the case study can be interpreted and understood.

In the preceding chapter, we discussed how P-O evolved from foundations in both positivist, empirical perspectives, derived from methods in the natural sciences, and from more phenomenological influences in interpretation of the cultural process associated with P-O. The "classic" fieldwork which grew under the wing of early 20th century functionalism and relativism, in both British and American variants, developed a set of implicit and explicit rules of practice and evaluation. These rules were then explored in the chapter, with emphasis on core ideas of role, reciprocity, and image. All of these ideas relate to the establishment of a natural context of social relations. Roles are sets of expectations which facilitate social performance; reciprocity is the exchange of expectations which allows the manipulation of social relations under the guise of coordination and conflict. Images are reflectors of how well

roles and reciprocity are working, according, again, to the evolving expectations of the social actors.

Underneath the functionalist paradigm of context and social performance, then, ideas about "culture" as expectation have guided both the practice of fieldwork and its evaluation by fieldworkers and hosts. The "objective" parameters of rule and role are affected by the "subjective" experience of the fieldworker, so that P-O has had difficulty living up to its original framing as positive science. And, however objectified in fieldwork and ethnography, culture has remained fluid, "subjective," and often elusive in the intersubjective meeting of host and fieldworker. Participant-observation has remained "relative" to its own experience, and this has been repeatedly contrasted with the empirical evidence of culture as objective expectation and natural context.

We also saw that, by the late 1960s, the practice of some variant of P-O in anthropology and the production of an ethnographic description and analysis were considered to be defining contributions of the discipline. At the same time, however, the increasing complexity of field situations, the challenges of hosts, and the diversification of competing theoretical frameworks in anthropology began to cause a re-evaluation of P-O as science, and as humanism. Although the same expectations remain that P-O is an access to an empirical reality, questions have arisen about the composition of that reality by cultural actors and by fieldworkers themselves. While still trying to make diverse societies and experiences relevant to world political and economic systems and issues, many anthropologists have delved more deeply into the interpretive aspects of culture as seen through fieldwork, rejecting prior inclusive theoretical paradigms for an emphasis on the micro-processes of culture construction and praxis. Influenced by trends in literary analysis, by phenomenology, by Marxist critiques, by psychological individualism, by varieties of linguistic and symbolic discourse, by hermeneutics, by symbolic interactionism, and by other analytic styles,

anthropologists (and hosts) have asked themselves to look reflexively upon the processes underlying the products of fieldwork, as experienced by the producers (Sass 1986: 50; Myerhoff and Ruby 1982: 27; Marcus and Fischer 1986: 27-30).

A historically and politically sensitive interpretive anthropology, preserving relativism as the method of engaged inquiry that it was in its inception, reconstructs fieldwork, the cultural other, and the concept of culture itself as the framing points for the field of ethnographic representation. Constantly matching the familiar against the unfamiliar, ethnography finally encourages a radical questioning of what the scope of its own reception, or, for that matter, of any work of social science, should be. Any work of ethnography becomes a historically self-conscious document that recognizes the possibility of multiple receptions, and of relevances to several possible discourses (Marcus and Fischer 1986: 166).

The current analysis is part of this recent movement in interpretive anthropology; we evaluate P-O rules through four of these philosophic traditions, using the concepts of epistemology, ontology, method, and ideology as comparative constructs. **Ontology** refers to the nature of existence of being. There is argument, as we will see, about whether there is an absolute, ultimate reality of the world shared by its participants; is reality given in the material facets of existence; is it reified into the social or the cultural; is it located in transcendent consciousness? **Epistemology** is the means of knowing that reality, as perhaps, through reason and consciousness, or through dialogue as interpretation. Thus epistemology and ontology are linked; the nature of reality is always tied to the way in which we can know it.

And, where epistemology is a set of assumptions about the process of knowing, **method** is the actual process of learning about and experiencing reality. P-O is a method underlain by a series of epistemological assumptions about knowing reality in its ontological manifestations. Objectivity and relativity, two of the key concepts in our analysis, can be understood, through the methodological dictates of the various traditions of knowing, as either ontology or epistemology. They can also be interpreted

as **ideology**, such that they become instruments of the evaluation of knowledge systems as relevant or accountable for the purposes of particular science or lay communities.

A. APPROACHES to KNOWLEDGE

Before proceeding with our treatment of the key concepts of P-O, the approaches to knowledge which we will use in our analysis need to be briefly introduced. Two primary paradigms in the interpretation of ontology and epistemology will be compared and contrasted throughout. One is the dualist paradigm, derived from the 17th century science initiatives of Descartes, which in the present context would direct us to suppose that the observer is separate from the observed reality. The participant-observer goes into the field and perceives a cultural and social world which has a separate, empirical existence, and which can be described more or less accurately and objectively, according to the tools of the observer. The dualist approach to which we shall pay particular attention is positivism, in its applications to social science method.

The other paradigm is monism, in which reality is perceived as continuous; therefore the observer is part of the reality of the observed, rather than being apart from it. Phenomenology, hermeneutics, and, to a lesser degree the sociology of knowledge (which occupies almost an intermediary position between the paradigms), are the monistic approaches selected here as most relevant to P-O process and interpretation. The fieldworker must construct his version of reality from the social processes of interaction in the field; cultural versions or texts are products of method, then, and cannot be viewed as belonging to a separate reality other than the shared dialogue of observer and observed.

These descriptions are, of course, somewhat simplistic, as will be the descriptions of the various approaches below. It is necessary for our purposes to limit discussion to only the most general points of variance, since a full analysis would

overwhelm the focus of analysis, P-O, in historic and epistemological detail. In addition, none of the approaches is uniform. Each has in itself multiple varieties of interpretation; there are several schools of phenomenology, for example, and several varieties of positivism. It is important to note, however, that the general approaches share points of similarity as well as variance, as we will see. One of the major arguments here is that monistic and dualistic approaches differ less in their epistemological and ontological assumptions, in regard to P-O, than is commonly assumed.

1. Positivism and Empiricism

This label is loosely assigned here to the varieties of empirical science methods which social sciences have borrowed from the natural and physical sciences. In Chapter One we described how P-O is rooted in the methods of natural science in which the observer describes and classifies, inductively, the world as observed; these methods were merely extended to the study of man. Modern positivism is often traced to Bacon and Descartes and their ideas of empiricism and dualism, and to consequent efforts to effect a rational control of nature. In the 19th century, theorists such as Henri Saint-Simon and August Comte, followed by Herbert Spencer, Karl Marx, and others, called for a positive social science which would provide for the ultimate civilization of man through the accumulation of empirical knowledge about humanity. The inductivism which went with these early approaches was challenged by Hume and Kant, however, on the basis that the nature of empirical reality cannot be confirmed independently of our perceptions and theories. A new school of positivism emerged in the present century, the "Vienna Circle" of logical positivism, which combined a dualistic view of reality with the deductive logical models of mathematics and physics. These scholars perceived

science as a rational, deductive endeavour whereby theories and universal principles could be tested and verified against a separate reality.

By the time P-O developed fully as a method in the 1920s and 1930s, it had absorbed the inductive dictates of natural historians, including Boas, and combined them with the deductive theories, such as functionalism, of British empiricists. In the U.S., there were several circles of thought deriving from logical positivism: at Chicago through Radcliffe-Brown and pragmatism, ¹⁸ for example, and at the Harvard School of Social Relations, through the functionalism theories of Talcott Parsons, for example (Leaf 1979: 150-207). The reification of socio-cultural reality, dualism, and the use of methods from the other sciences are evidences of these influences. We have already seen how the modified inductivism of P-O combines inductivism with deductivism in a circular mode. A model of social relations such as that of Radcliffe-Brown or that of Talcott Parsons could be proposed as an explanation for a pre-existing social field. It could then be tested, through P-O, to see if the empirical actions of living people confirmed the synthetic, a-historical, model of functioning social parts. ¹⁹

2. Sociology of Knowledge

The sociology of knowledge is concerned primarily with the influence of ideology and theory on epistemology, or the way in which we know reality; it argues essentially that the ideas that we (a group of people) hold about the world, including our ideology, influence the way in which reality is known and perceived. The approach has had a less direct influence on fieldwork practice than the other approaches discussed, and in fact is only a peripheral subdiscipline of social science. It illuminates, however, some of the central epistemological problems surrounding objectivity and relativity, and is often considered basic to more recent interests in how knowledge is constituted. It encompasses a range of approaches to the relationships between ideas and their environment in the social context (Berger and Luckmann 1966: 1-18). The framework

was named in the 1920s by the German philosopher Max Scheler, and has been developed both in Europe and the U.S. from several antecedent streams: German historicism and idealism; Marxism; French sociology (particularly Durkheim's ideas of the collective); from the pragmatism of James, Mead, and Dewey; and from Weber's ideas of intentional action. Indeed, it has roots both in empiricist and more monistic models (Curtis and Petras 1970: 1-26; Wolff 1970: 545-53; Berger and Kellner 1981: 59-70; Hamilton 1974).

Max Scheler and Karl Mannheim are among the central figures of the sociology of knowledge who have provided the integration of influence. Scheler sought a science which would transcend both positivism and the idea of socially relative knowledge; he suggested that the appearance of theories and intellectual constructs was relative to social context, but that the content of theory -- the nature of ideas -- was not. These latter could be apprehended through a transcendent phenomenological theory which would access the nature of apparent reality, thus freeing truth from its social environment. Mannheim, influenced by Scheler, Weber, and others, broadened the basic assumption of the sociology of knowledge, that knowledge of the world is affected by the social context of knowing, introduced the subdiscipline to the English-speaking world, and based it more firmly in sociology as well as philosophy. Mannheim argued that ideas are rooted in historical settings, but he, too, rejected the idea that all knowledge therefore is situationally relative. He suggested that there is in society a class of intelligentsia (scientists) who are able to critically surpass relativism and offer a comparative critique on the distortions of ideological, and relative, knowledge. Only this critical function can bridge the various partial realities of social existence (Berger and Luckmann 1976: 9-18; Goff 1976: 47-50; Hamilton 1974; Macquet 1951; Scheler 1970: 170-83; Mannheim 1970: 109-30).

The sociology of knowledge has not succeeded in adequately bridging content of ideas with their context, but it has focussed attention on the issues for other related approaches, like symbolic interactionism, ethnomethodology, phenomenology, and social psychology. Berger and Luckmann, for instance, use the fundamentals of the approach, in combination with symbolic interactionism and the phenomenology of Alfred Schutz, to address the lay realm of commonsense knowledge -- thus taking the sociological study of knowledge beyond its "traditional" applications to theory and ideology to look at how ordinary actors construct reality out of the messages surrounding them. This, and other derivations, take us closer to the concerns of P-O for describing and creating versions of cultural reality.

3. Phenomenology

Like the other approaches, phenomenology is manifold in its antecedents and orientations. There tends to be more unity in method than in philosophy, and it is in method that it has had the most relevance to P-O. Phenomenology provides some assumptions and procedures for deciphering how cultural reality, as inductively described, is constituted in interaction with hosts and reconstituted in analysis (Bruyn 1970b: 283-87; Natanson 1973).

The term "phenomenology" was first utilized by Hegel, but the main figures of phenomenology as approached by anthropology are E. Husserl, Jean-Paul Sartre, M. Merleau-Ponty, and A. Schutz. The term refers to Kant's distinction between phenomena, or the appearances of reality in consciousness, and noumena, or "things-in-themselves" independent of consciousness. Several of its central tenets help explain its relationship to P-O. It is considered (1), by Husserl and others, to explain the constitution of reality which dualism takes for granted; thus it is ontologically and metaphysically prior to empiricism rather than antithetical to it. Reality (2) is constituted by the consciousness of the actors; to the extent that such consciousness is

shared through communication, it is intersubjectively rather than objectively given. This has several consequences for the fieldworker: he must attend to the actor's meaning or perspective to get at his consciousness and thus his essential reality; he must reflect on his own consciousness and assumptions about reality to have access to understanding the essence of reality; in addition, he must realize that the product of interaction in the field is an intersubjective construct, an apprehension of essences through intuition. It can be argued that all of these consequential processes underline the empirical rules of P-O.

And, finally (3), in the constitution of reality there is no clear separation between subject and object; hence the monism. We can know an object only as it appears to consciousness, and essential being is located, in Husserl's formulation, in a transcendent ego which affords us access to consciousness and to the pure state of being in which subject and object can truly blend. The work of Husserl is often taken to be the "purest" formulation of phenomenology as philosophy relevant to anthropology, but Husserl's focus on finding the ultimate objectivity of reality in the transcendence of the ego from interest and bias does not help explain the intersubjectivity of the social setting of P-O -- although it highlights a fundamental problem with reconciling this pure ego of reason-as-reality with the dialogue of communication between actors, as we shall see (Bidney 1973: 133-40; H. Wagner 1970: 5-9; Kultgen 1975: 375-76; Giddens 1976: 24-7; Gadamer 1979).

The work of Alfred Schutz brings phenomenology from philosophy to sociology, from ontology to epistemology; thus it has more relevance to P-O as method. Schutz also absorbed ideas from Weber, Scheler, the pragmatism of James, and from the social psychology of G.H. Mead;²⁰ he in turn influenced the school of ethnomethodology led by H. Garfinkel.²¹ He paid less attention to the possibility of consciousness-in-ego than to the understanding of how actors construct their social worlds of meaning (lifeworlds),

and, in turn, to how the scientist puts aside (brackets) his own models and assumptions in order to comprehend the commonsense lifeworlds of his hosts. The good phenomenological fieldworker uncovers the assumptions which his hosts take for granted, and thus reconstructs their culture more holistically. Although the scientist must reflect upon his own presuppositions in order to do this, he must pay less attention to his own intuitive consciousness than to the "recapturing" of the lived experiences of his informants as they intentionally constitute meaning through symbols and typifications (H. Wagner 1970: 43-6; Giddens 1976: 27-34; Bauman 1978: 172-88; Schutz 1970: 265-93; Bidney 1973: 137-38; Kultgen 1975: 375-80).

Critics of phenomenological method argue that it cannot truly "understand" actors' reality, but can only substitute analytic models for hosts' own constructs. There is an overemphasis on subjective coherence and relativity of knowledge, and a lack of connection in analysis between this subjectivity and the intersubjective social world. The latter has real consequences, intended and unintended, which must be accounted for beyond the subjectivity of epistemology (Jarvie 1975: 259-61; Seung 1982: 236-40; Cunningham 1973: 102; Bidney 1973: 136-39; Kultgen 1975: 380-84). We will explicate these criticisms in the next sections when we examine whether the epistemology of phenomenological method can truly provide insight into a socially complex and diverse ontology of culture.

4 Hermeneutics

Hermeneutics began as a method of Biblical exegesis and philology in the 17th century; since that time it has undergone modification and variations and has come to influence P-O both through method and through the concept of culture. As part of the monistic paradigm, it is now considered a major stream of interpretive anthropology, and it has been used to examine the mediation of cultures through dialogue between Self and Other, fieldworker and host.

Hermeneutics developed in modern form in the 19th century as a part of nationalist and idealist approaches to culture extant in Germany; F. Schleiermacher and W. Dilthey are considered central figures in its development. Schleiermacher broadened hermeneutics to apply to the linguistic understanding of all knowledge (Little 1979). Dilthey used hermeneutics to argue that the methods of the human sciences are and should be distinct from those of the natural sciences because of the difference of subject matter: human cultural reality cannot be taken as "given" in the same way that the natural world is, and is constituted by reflexive actors who interpret the world relative to their historic circumstances. Dilthey also looked for "pure" objective truth, beyond relativity, in the spirit, or *Geist*, of the cultural mind (Little 1979; Giddens 1976: 54-6; Rabinow and Sullivan 1979: 13-8; Bauman 1978: 32-47; Radnitsky 1970b).

In the early 20th century, the followers of Dilthey's and Schleiermacher's searches for truth began to focus on the social context behind the "text" of cultural interpretation. This re-structuring of hermeneutics drew life from phenomenology, linguistics, literary criticism, existentialism, and the "Frankfurt School" of social criticism in Germany in the 1920s and 1930s (Marcus and Fischer 1986: 30-1, 119-20). It is this modern form of interpretation which has had the most direct influence on anthropology; scholars such as Paul Ricoeur, Clifford Geertz, Paul Rabinow, Michael Agar, and Robert Scholte have applied it to the field experience and to the creation of culture from the context of social relations, as we will see below.

The hermeneuticist may interpret the field setting of P-O as an attempt to understand the text (individual, society, object, or cultural version) in relation to the context within which it is formed, e.g., the individual in relation to his culture. Meaning resides in the context within which the text is located; the interpreter must go beyond the author's [of the text] intention to understanding the context behind this intention. Why does the informant portray his culture as he does; what is the meaning

behind his choice of meaning? The informant, in this approach, serves less as text (Boas) or context (Malinowski) than as the hinge for understanding the relationship of one to the other, text to context (Dwyer 1977: 148-50). In order for the fieldworker/interpreter and his hosts to establish a dialogue for communicating the text-in-context, they must each be able to reflect upon their own cultures, their own suppositions about the situation -- to see themselves as objects. In this way, a dialectic or a "hermeneutic circle" is established by which each examines his own belief context, learns from the other, and reformulates his own system and symbols of meaning.

Thus hermeneutics can reveal the intimacy and self-reflection of the process by which the investigator learns about the culture of the Other by pondering it in relation to his own, eventually deriving a new interpretation - text- of culture in relation to this mediating dialogue. The actors reconstitute meaning through context and symbol, via discourse (Rabinow 1977: 150-62; Dumont 1978: 4-5, 70-2; Marcus and Cushman 1982: 25-66; Agar 1980a: 264-71, 1982: 781-86; Scholte 1974: 431-44). Although informant and investigator need not reach consensus in their versions of truth, they must create a common frame of reference by which they understand, if not empathize with, each other (Geertz 1979).

Note that this circular process of guessing and validating, and of reflection and reformulation, describes essentially the modified inductive method of P-O by which the fieldworker derives hypotheses from his observations and then tests them through further observations and dialogues; we will return to this when we discuss culture and validity. It does not, however, always provide adequate insight into how the interpretations of text are validated. Hermeneutics has been criticized for failing to deal with the need for objective references for validity; Dilthey sought "truth" in a transcendent cultural spirit, but this does not readily transfer to the intersubjective context of dialogue (Gadamer 1979; Bauman 1978: 32-41). Even where Dilthey's *Geist*

has been rejected, as it has by many more modern hermeneuticists, there is a tacit assumption that mediation is only possible where a common frame of reference for establishing validity (not just understanding) is possible (Radnitsky 1970a). Yet there are many different versions of where validity can be found: personal insights, subjective coherence, intersubjective commitment. Critics are confused as to how one determines which of many interpretations is strongest or most valid, and to whom (see Freilich 1975; Hayano 1975; Hanson 1975b). In rejecting a transcendent "objective" truth, these social scientists have not made clear whose "subjectivity" is being objectified and understood -- the researcher's, the host's, or a consensual one.

As we will see, this leads to confusions of epistemology with ontology, method with theory; this is a problem often shared with other monistic approaches. Researchers in these traditions, as in the empiricist one, may bound a cultural reality as text and assume a kind of false reification or integration in order to establish validity (Agar 1980a: 267-71). The alternative to this bounding is, however, a failure to provide any definitions in which to ground dialogue, reflection or the relative intimacy of subjectivity. The hermeneutic circle can become a circular maze if text and context are defined only in relation to each other (M. Hobart 1982: 40-52). In our ensuing discussion of the concepts of objectivity, relativity, and culture, we will see how both monistic traditions (including hermeneutics and phenomenology) and dualistic approaches must deal with problems of validity, bounding, and definition. Once the ontological assumptions made by each approach are reduced to their epistemological foundations through the methods of P-O, it can be seen that, indeed, both paradigms have similar definitional problems.

B. OBJECTIVITY

In chapter one, we suggested that objectivity in P-O method is most often associated with the inductive empiricism which underpins the method. It is associated

with an attitude of detachment, an etic knowledge, and, most importantly, a set of external, scientific standards of validity by which the products of the meeting of text and context are evaluated. The objective researcher maintains a universalized norm respecting the nature of a reality given in culture, building a textual interpretation inductively and circularly out of an immersive context. Objectivity is thus most commonly associated with the reification of the natural context and social relations of the field, and of the ability of the fieldworker to be both part of and distant from this context. It is also, however, associated with monistic challenges in P-O, since they, too, must grapple with issues of validity in the matching of epistemology with ontology.

Rather than exploring the many definitions of objectivity, we will broach the assumptions underlying the concept. Objectivity is most often perceived as a quality, an ability of the observer to assess the hosts' culture using standards of analysis and validity derived from his own science and culture. Two objective observers would provide reliable assessments of the same situation, whereby they would propose similar descriptions and interpretations. Yet objectivity is not solely the result of observer qualities; it relies on assumptions that there is a continuous reality that both observers could access in the field. And since, in P-O, validity is often thought to ultimately lie in the observer's ability to create a version of culture which is emically plausible to cultural insiders, objectivity assumes an ability to translate one cultural reality into another. We will first look at objectivity as ontology, then as epistemology; then we will propose an alternate perspective on the concept.

1. Ontology

Objectivity as ontology is more developed in positivism than in the other approaches. The "realism" of the objective, or of culture, is seen as the source of the validity of descriptions of it. While this validity is therefore ultimately situated in

independent realism, it is universally accessible to skilled observers, so that a reliable science method will produce equivalent, and equally valid, reproductions of reality (Macquet 1951, 1954; Bauman 1973: 107-117; Jarvie 1975: 256-60; Gudeman and Penn 1982: 89-92; Cesara 1982: 212). "Reality," however, can be assumed to exist at different levels.

One level is that of social relations, the Durkheimian collective. The various functionalist theories of Radcliffe-Brown, Malinowski, and Parsons, for example, posited a reified social context with autonomous functioning (Gudeman and Penn 1982: 92-6). Although the fieldworker is sent to describe this context inductively, the assumptions which guide him/her are essentially deductivist premises about the nature of the context which cannot be verified by any amount of empirical evidence, critics note (Goff 1976: 50-5; Popper 1970: 654-59; Stove 1982: 56-84). Sociologists of knowledge, such as Scheler and Mannheim, modified the positivist approach by focussing on the social constitution of these premises, but in doing so have not always adequately addressed the relationship of the social ontology to the constitution of knowledge about it (Macquet 1951; Goff 1976: 384-409). They may presuppose an autonomous social reality in order to explain it.

The more monistic approaches have tended to ignore ontology in favour of discussions of epistemology, but in doing so have exaggerated the problems faced by the sociology of knowledge. Indeed, these theorists largely reject the dualism of positivism and detached objectivity in favour of a participatory intersubjectivity more appropriate to P-O. The idea of the dialogue replaces duality, and the holism of the social or cultural system as bounded in P-O empirically is replaced by the integration of this dialogue (Dwyer 1977: 143-51, 1979: 7-13; Jarvie 1975: 256-60; Fabian 1979: 21-4; Agar 1980a: 265-69, 1982: 783-93). Nonetheless, a socio-cultural background to this dialogue is presumed to exist although it is seldom explicated

(Bauman 1978: 238-46). The dialogue takes place within a narrow, democratized realm and tends to have no reality outside that perceived by participants. This may be a consistent epistemology, but it is seldom free from ontological assumptions, including contradictory ones, as theorists have not freed themselves from the search for "objective" sources of validation.

The other level of validation/ontology, which often substitutes for the failure to locate ontology effectively in the social realm, is that of the cognitive individual. If we assume a social reality which exists independently of the observer, as the dualists do, but we as observers cannot prove its existence, then we must rely upon the observer's rational ability to convince us of its existence. Goff (1976: 387-90), Rabinow and Sullivan (1979: 8-13), and Popper (1979) argue, with others, that positivist knowledge often resorts, in association with empiricism inductivism, to a Kantian *a priori* causality located in reason. We observe the "natives" in much the same way that other observers do, because we possess this rationality. And, we can describe the "natives" because they, too, possess a form of this rationality and thus we share with them a common frame within which to understand their culture.

Rationality and empiricism are conceptual tools used to break out of relativist interpretations, and phenomenological as well as positivist explanations seek this escape in transcendent objectivity (Taylor 1979; Dwyer 1979). Scheler, the sociologist of knowledge, suggested that there existed a realm of basic ontological ideas from which social factors selected; this realm was the source of objective validation and was thus cross-cultural (Scheler 1970: 170-83; Goff 1976: 34-7). Validity lay in the "mental structure of the society" (Curtis and Petras 1970: 17), but the structure itself was ultimately to be located in the individual in interaction with society.

Similarly, the phenomenologist Husserl argued that the world of appearances (ontology) as recognized by the participant-observer can be further reduced to

consciousness; ultimate reality, the essences of objects, can only be known through the mind (Natanson 1973; Bruyn 1970b: 283-87). Epistemology approaches ontology when, for Husserl, pure "objective" theory is achieved through the bracketing of interests (Habermas 1970: 40-2; Bidney 1973). Even Schutz sought the basic tenets of a transcendent rationality with which to understand the principles of the production of knowledge, although he largely rejected the ontological concerns of phenomenology (Schutz 1970). And, for hermeneuticists, there is often an implicit transcendence asserted, if only an epistemological one, in the claim to cross-cultural understanding, and intersubjective understanding, as will be argued, can often only be interpreted as subjective knowing (Seung 1982: 183-212).

The assumption of an independent ontological reality, for field culture or any other object of analysis, can potentially contradict the need for an "objective," replicable knowledge of that subject. In order to hypothesize, as positivists often do, that this reality is open to reliable perception by all observers, and yet account for supposed empirical differences between autonomous cultures, one must relocate the ontology from the social to the individual level. It is thus a property of the observer, an innate rational principle of knowledge, which allows us to independently, and consensually, validate that interpretation. One cannot prove the autonomous existence of a social reality, only hypothesize that it exists. And, the only demonstration of its existence is in our understanding of it. If one feels the need for an ultimate source for validating this existence, one must replace the needed empirical proof with a source located within the process of understanding itself. Both monists and dualists pursue, then, an emancipated "pure" knowledge, and both finally look for it in the rational freedom from social interest -- failing to demonstrate the nature of the social ontology itself apart from its interest-laden epistemology. So we must now turn to epistemology, whereby we "inform" an ontologically-given (or created) cultural context by forming,

epistemologically, a text. The context comprises the holism, objective or intersubjective, against which the duality or dialogue of knowing is comprehended.

2. Epistemology

Objectivity as epistemology is usually conceived as a means of establishing the validity of generalizations (of text) both cross-object and cross-observer. We have already seen how objectivity as ontology contains an inherent problem -- placing the source of reality in the object makes the ontological source of transcendent validity problematic, unless the source is located in the observer, or in a rationality which transcends the individual observer. An epistemology must be devised which gives this rationality a formal methodological existence, but, in doing so, the role of the observer in providing the **means** of validation often becomes confused with the role of the observer in providing the **source** of validation. One of the main points to be made here is that dualism and monism do not differ as much in epistemology as they seemingly do in metaphysics, just as both provide supports for the circularity, and the self-reliance, of P-O method.

The dualist heritage of P-O in the inductivism of the natural sciences supports objective epistemology as being a quality or skill of the fieldworker, whereby the observer attempts to rid himself of personal factors (non-scientific) impeding insight into the nature of objective reality. This implies that the investigator can know the meaning of his hosts, Malinowski's classic objective (Johnson 1975).²² The link, as we have argued, between the observer and the observed, as both objects and as knowing subjects, must come from reifying epistemology into a shared ontological reality. Since the rationality of this impersonal process is attitudinal and procedural, by the very definition of the process itself a strict inductivism is impossible; even in the application of method itself -- bounding context, applying norms -- one is imposing a degree of

deductive rationality onto the situation (Bauman 1973: 109-110; Macquet 1964: 53-4; Barnes 1974: 22-44).

The debate about the imposition of the observer's norms, assumptions, and values on the observed situation is broad-ranging, as noted in Chapter One. Even the strictest advocates of scientific objectivity have come to realize the need to recognize values in objectivity. But critics attack the idea of objectivity itself as separate from the knowing subject. Instead, they suggest, the subject and the object define themselves and each other reciprocally. Therefore, the next step off the ladder of "pure" epistemological objectivity is toward intersubjective definition, and the process of objectification itself. The latter concept provides a link among all the approaches to objectivity, and helps to identify the problems of the "objective" as a social *versus* an individual attribute.

One form of epistemological intersubjectivity refers to the science community itself. P-O is informed by rules of procedure, criteria of validity, implicit expectations of rational processes, and normative judgments relating to objectivity, as we have seen. It is through this intersubjectivity that the community of social scientists evaluates the quality of interpretation/text: Does the researcher conform to these expectations? Several scholars, particularly with sociology of knowledge and phenomenological approaches, have broached the creation of meaning by scientists, viewing science as a system of second-order abstractions comparing and interpreting common-sense formulations of theory (Schutz 1944: 500-07, 1970; H. Wagner 1970; Simmel 1950; Scheler 1970: 170-83; Popper 1970: 649-54, 1979; Berger and Kellner 1981: 150-71; Jarvie 1972; Winch 1958). The rules of science replace the transcendent and provide socially intersubjective guidelines for culture. The well-known ideas of Thomas Kuhn (1977) on scientific revolutions of paradigms,²³ of Karl Popper (1979) on science as critical knowledge²⁴, and of Michael Polanyi

(1964) on the tacit dimension of critique ²⁵ are all variants of insights into the implicit and explicit expectations held by scientists about objectivity as intersubjectively-informed judgment.

Intersubjectivity can also denote the creation of interpretation **across** the boundaries between the scientist and his hosts; this is the intersubjectivity by which monists and interpretive anthropologists describe the P-O process, and it links the logic of the observer with that of his hosts through dialogue, discourse, and the "hermeneutic circle." The nature of the intersubjective, and its relation to the internal rules of science, is not always fully addressed in interpretive anthropology, however. We noted this above when we argued that monists insufficiently support ontology with epistemology. A reality may be assumed to exist which informs the dialogue of the actors, but monists cannot account for how this reality both pre-exists and is created intersubjectively by the dialogue itself. An emphasis on consensus, coherence, and democratized dialogue can reduce the complexities of interaction to an idealism approaching rationalism, and objectivity can merely become an exercise in attaining effective, reasoned communication (Agar 1980b: 115-22; Gadamer 1979; Taylor 1979; Bauman 1978: 27-46). Bittner (1983: 149-55, 194-98) argues against the exaggeration of the claim to "true" intersubjective validity; the "truth" is ultimately located in each observer's own perspective, which may or may not be shared, and the use of the method may lead to an overemphasis on surface agreement about appearances.

Objectification as a concept tends to link the two kinds of intersubjectivity, as well as text and context, fieldworker and hosts. Essentially it is the epistemology whereby Self is associated with the Other, and the association may become ontologically reified into rationalism. Thus it underlies both intersubjective and more dualistic approaches, to the extent that both exemplify social processes validated and realized in

individual technique. The epistemological duality or dialogue deemed necessary for critical abstraction is justified through a transcendent rapprochement. One projects a contextual content into an objectified, rational form or text. Once the text is detached from socio-cultural context, as in ethnography, one controls and authorizes it; one attains a dualistic detachment from it by re-inventing the cultural context oneself (R. Wagner 1981: 1-14; Ricoeur 1979: 78-100; Parkin 1982: 23-4; Rabinow 1977: 150-55; Myerhoff and Ruby 1982: 24-26; Marcus 1982: 171).

The textual translation that the anthropologist undertakes in the field is therefore an objectification, either a representation of or a replacement for context, but, socially and intersubjectively, he also creates a context for it which may involve, as R. Wagner (1981: 9) suggests, a change in understanding one's own cultural context --perhaps a further ideological alienation, or perhaps a more competent praxis and self-criticism. Jules-Rosette (1978: 567-68) criticizes the "veil" of objectivity by which the discipline bounds itself and validates its predictions ontologically. Method becomes ontology through the objectification process; the circular method of investigation is transformed into the circular reasoning which inductively supports our own (the observers') rationality. In other words, we project an ontological validity onto the text through the epistemology of dialogue; but, in the absence of an ability to establish the reality of this emic validity, we must rely upon our own rational insights (including those intersubjectively informed through science) to posit validity.

3. Alternatives - Toward a Bounding of Form

In sum, then, both monism and dualism tend to give science somewhat of a special status through objectification, critical or self-validating. In P-O and other investigations, the rules of assuming and bridging duality or dialogue intersubjectively become reified into a rational ontology. Means become confused with the source of validity; both are rational, individualistic, and formal. As discussed above in the

conclusion of the section on objectivity as ontology, neither monism nor dualism succeeds in establishing a social ontology to ground their epistemology, although such may be assumed to exist. The praxis of the researcher is representative of this context, and is held accountable for it; validity is seen ultimately to reside in the epistemological process of investigation, using Self as tool. The objectification of Self thus serves as the ontological source of rational interpretation, explanation, and understanding. Monistic philosophies have not really challenged the basic goal of objectivity in dualism, the development of a science free of culturally-induced biases and relativism, but have sought it on an essentialist plane of history, humanism, and, ultimately, consciousness (Bauman 1973: 115-16, 165-73; Cesara 1982: 212-28; Dwyer 1977: 143-51; Goff 1976: 404-09; Macquet 1954; Hartung 1970: 686-93; Popper 1970: 649-59; Jarvie 1975: 259-60).

When objectivity is used as both source and means of validation, a conflict ensues, therefore, which is usually resolved through the unification of ontology/epistemology in the observer. Objectivity then becomes a conceptual opposite of relativity, as we will see. **One can combine the approaches to objectivity, rather than oppose them, if one regards objectivity as essentially process, as objectification -- not as reification, but as bounding.** This is indeed a cultural process involving an intersubjective agreement as to what constitutes a bounded level or type of knowledge, but it is a separate process from the validation of that knowledge as "true" or legitimate. This latter process, we shall argue, is integral to the idea of culture itself, and operates in cooperation with both objectivity and relativity, rather than in conflict with them. Looking at objectivity as the bounding of form allows us to reconcile the relativity/objectivity duality without tautology, since it no longer connotes validity.

Bounding of form through objectivity is an epistemological process; it is not integrally linked with a particular symbolic content. Boundaries may be taken as "given" in their social aspects, even though they are created and re-created. The fieldworker creates a text in a particular form, such as an ethnography, which is recognized by the disciplinary community as distinctive in form and amenable to certain symbolic associations. Objective knowledge is not the product of science alone, but is known by all cultures insofar as it represents a process of typification and categorization of knowledge about reality (not of reality itself) integral to the strategies which make participatory endeavours meaningful. Objectivity is simply the explicit, self-conscious realization of new or tacit knowledge as a kind of "information," given form by a community (or dialogue) through translation or innovation. Sacred knowledge, private knowledge, or esoteric knowledge may not be shared by the entire community, but they are products of recognizable processes rooted in discourse and social praxis. Thus when fieldworker and host establish a "fiction" of a reciprocal relationship (see Geertz 1968), they recognize that the relationship bounds a form of mutual knowledge which may then be imbued with meaning and validity. It is this alternative which will form the root of an alternative paradigm for understanding participant-observation.

C. RELATIVITY

Cultural relativity has been a strong foundation of twentieth century cultural anthropology, particularly in its American history; it has helped to establish both internal theoretical/methodological directions and external professional credibility. Relativity, like objectivity, has a number of different, ambiguous, and even contradictory implications in the ways that it is defined and used. Basically, it states that the anthropologists' fodder, the traits of culture, should be studied within their own context; they are given meaning by that context and are relative to it. From this

basic tenet, some anthropologists have generalized a methodological relativism which states that, in correlation with the objective stance of value-free science, the researcher should refrain from imposing his own values upon those he studies; and, from methodological relativity, some have gone farther to imply a moral relativity which states that all cultural practices have moral worth in context, and none should be condemned as universally "wrong" in a moral sense. These latter two developments, which largely grew from the teachings of the students of Boas, such as Melville Herskovits and Ruth Benedict, are controversial and will not primarily concern us here (see Benedict 1934; Herskovitz 1972; Bagish 1982; Hatch 1983: 66-8; Geertz 1984; Kroeber and Kluckhohn 1963).

Rather, we will look at the implications which are often used to contradict rather than correlate with the concept of objectivity. Relativity is examined as two sets of hypotheses. The first, which we will call internal relativity, proposes that the internal components of a socio-cultural system operate and take meaning relative to each other, as suggested above. The second, which is often derived from the first, we deem external relativity; it asserts that since culture has meaning within its own context, there are few objective generalizations about cultural reality as ontology which are possible cross-culturally. External relativity in this theoretical sense contradicts the fundamental objectivity of the dualists which asserts that objective knowledge is possible between cultures by virtue of the continuous nature of empirical reality, and it calls upon the intersubjectivity of the monists to create a transitory cross-cultural epistemological dialogue. In addition, we will briefly make note of the use of relativity as ideology in the comparison of cultures.

1. Internal Relativity

The concept of relativity has been developed in close contact with the concept of culture; this will be clearer when we discuss the latter concept. By the era of the

Enlightenment, writers were recognizing the importance of understanding culture within its context or habitat. Later, in the 19th century, the positivist social sciences were acknowledging the cohesive autonomy of cultures, and the German idealists, such as Dilthey, were forming ideas about the internal integration and justification of culture. Although the British functionalist theories of the 20th century supported the notion of the internal relativity of culture (Jarvie 1964: 11-22; Hatch 1983: 70-81) it was really Franz Boas and his followers on the American continent (like Herskovits, Benedict, and Mead) who coalesced the dualist and monist streams, both in formulating culture concepts and in elaborating the idea of relativity, which was used by them to critique both universalistic racial and evolutionary theories and the ethnocentric conduct of many Americans in culture and politics.

This movement dovetailed with a general mood of public scepticism about progressive innate purposes in man (beyond pragmatic self-interest) and with the Einsteinian revolution in science, and it has thrived best in periods of political liberalism (Hatch 1983: 35-65; Marcus and Fischer 1986: 19-20, 32). Since mid-century relativism has been challenged more as a moral perspective than as a theoretical/methodological supposition, as it has been supported variously by monistic and dualistic approaches. Recently, some writers have noted that, in spite of criticisms, anthropologists have productively used relativism to publicly promote the existence of alternate systems of culture and belief in an age of technological universalism and, increasingly, conservative opinion about cultural viability (Geertz 1984; Marcus and Fischer 1986: 116; F. Hanson 1975b; Berger and Kellner 1981: 56-8).

Thus the basic type of relativism was that associated with the bounding of cultures as natural, integrated contexts. It has been gradually extended, sometimes to an emphasis on cultural determinism, and, in P-O, to assertions that internal -

emic- validity is the ultimate source of truth. Since the bounding of culture was to be largely an analytic, etic task done by the fieldworker in P-O, the bounding itself came to substitute for this presumed internal validation. The inside perspective, then, deemed cultural, came to be held by some anthropologists as the essentially "true" one. The outside anthropologist may have access to a greater perspective on truth than the insider would, but this perspective should facilitate rather than contradict the internal rules of culture (Herskovits 1972; Benedict 1934; Kroeber and Kluckhohn 1963; see also Tennekes 1971: 2-4, 198-205; Giddens 1976: 144-46; Winch 1958: 181-86; and Barnes 1982: 120-26).

2. External Relativity

The next consequence of the extension of relativity from a theoretical descriptor of the nature of culture to the incorporation of validity was to use it to assert that validity was itself relative. Internal relativity, in conjunction with the bounding of cultures by fieldworkers and the discovery of cultural diversity, was broadened in context to external dimensions to argue that no objective generalizations across cultures could be valid. This is, of course, an extreme assertion, and not always followed to its full consequences, but it was certainly used by monists and dualists alike as a contrast to objective ontological knowledge. The external relativist must either extend his/her cultural context intersubjectively to incorporate a broader tradition (such as that created or shared with his hosts), as the monists do; or he must ontologically bound his internal relativity and extend the consequences of bounding to other entities, as dualists do.

For positivists, this has resulted in a radical empirical relativism which has been a source of an insidious self-contradiction within positivism, as critics have pointed out that an observer is never sufficiently free of his own cultural biases to achieve the detachment necessary to perceive relative differences -- relativity thus

becomes a function of the observer's belief and cognition. The sociology of knowledge has sat on the fence of this contradiction, since it posits both empirical cultural realities and the possibility of cross-cultural determination of knowledge validation. Karl Mannheim, for example, debated the issue of whether the genesis of knowledge in social existence also determined its truth or validity. While he held that most people, by their socio-cultural conditioning, perceive their own version of reality as "true," he deplored the idea that the possibility of objective knowledge (particularly by those trained in science) could be defeated by extreme ontological relativism. Mannheim substituted the term "relationism" to suggest only that knowledge is related to social structure, whether right or wrong, relative or absolute (1970: 123-24). Thus he sought recourse to internal relativity, renamed.

Monists have argued that "valid" versions of culture must be created in interaction which literally extends communicative validation procedures across boundaries; the problem here, as we have seen throughout, is that ontological boundaries are not defined, but are often presumed to exist in dialogue. This leads to a similar internal contradiction as affects dualists. There is an inability to account for validity beyond a slender sphere of the interpreter's own contextual awareness and subjective interpretation. The same point must be made for both monists and dualists. The idea of relativity, in any form, is inconceivable without a common frame of reference within which to understand it (Bidney 1953; Bauman 1978: 237-41; Watson-Franke and Watson 1975: 247-53; Giddens 1976: 18; Seung 1982: 198-212). Therefore external relativity taken to a radical extreme invalidates itself, but this ultimate negation is prevented by the fact that objectivity and relativity tend to create each other in a circle, which is intimately connected with the circularity of P-O process.

Participant-observation, with its concern for objective methods, bounded cultures, and subjective validation within culture, has bolstered the concept of external relativity. Ontological "facts" of diversity have been confounded with the sources of their validation through epistemology (Schmidt 1968; Campbell 1972; Jarvie 1975: 257-63; Barnes 1974: 154-57; Hanson 1975b). The researcher, with his dual stance (inside-outside), becomes the instrument of knowing and validation; he must extend the internal relativity of his/her own culture to comprehend that of another, logically or intersubjectively, as part of the extended circle of P-O relationships. This circularity can only be healed through a redefinition of the original internal relativity itself.

3. Ideological Relativity

Before proposing an alternate framework, mention must be made briefly of ideological relativity, as this idea will be integral to the case study. This is not normally identified as a variety of relativism, but it can be identified in current academic and popular writing about the survival of alternate cultures. In ideological relativity, writers stress the importance of the difference between their own cultures and those of other societies to the extent of making culture difference and relativity into a self-supporting ideology. Just as objectivity can be idealized into the special mark of science, so too can the relational boundaries between social and epistemological forms be idealized into popular doctrine emphasizing bounding and relativity. Advocates do not necessarily assert that cross-traditional understanding is impossible, but they claim a special status for their own view of the perceptive borders and relations between traditions --sometimes bordering on ethnocentrism, but emphasizing the ideology of maintaining one's own culture, often in the face of pressure to the contrary. Like the cultural relativity of P-O, ideological relativism has elevated the bounding of cultural content into pure forms which gain stature in comparison (relation) to other such

forms in a diversified society. We will further encounter ideological relativism when we discuss, in the case study, the re-creation of northern Canadian culture as a distinct, viable, and esteemed entity within the Canadian mosaic.

4. Alternatives

We have looked at the history and epistemology of relativity to see that it is interrelated with the foundations of objective knowledge and bounded ontology. The challenge of external relativity, as it has grown from the combination of epistemological duality with an internal subjective relativity, is extended by ideologization of relativism or is met by some form of rationalism. In the latter case, the observer of cultures (as in P-O) must use cognitive frames derived from his own culture to either posit an internal validity for another culture, or to utilize dialogue to create a cross-cultural context to transcend boundaries. The problem is much the same as that we encountered in our discussion of objectivity. The epistemology of the fieldworker is substituted for the ontology of culture as source of validation. The solution must be the same: **relativity as a concept must be rid of the ontological associations of content and validity it has gained through P-O and its extension to diverse anthropological field settings.**

If we redefine relativity back in the direction of its basis in internal relationism, we can reduce this contradiction as well. Like objectivity, relativity can be regarded as a correlate of, even an aspect of, the concept of culture, but not as a substitute for it. Mannheim's definition of relationism can be a starting point: ideas are related to "a certain mode of interpreting the world which, in turn, is ultimately related to a certain social structure which constitutes its situation" (Mannheim 1970: 123). Rather than assuming that only an elite group can attain a relational perspective, we must presume that such a perspective is a part of knowledge and knowing for all cultural actors. **The actor perceives the relationship between the different**

bounded forms of objective knowledge, either explicitly or tacitly.

Relativity is the frame of comparison --whether rational or irrational -- rather than its denial, and as such remains integral to the P-O process as dualism and dialogue. It is a function of epistemology, but not of innate ontological structures of rationality.

The expansion of one's intersubjective context of communication, by P-O or other means, may make one aware of new relations, as Mannheim suggested. Once one becomes aware of alternate interpretations, one must use each to establish perspectives on the other through acknowledging their relationship to each other as (objectified) forms of knowledge (Mannheim 1970: 123). Tacit knowledge becomes explicit when these relations are recognized (Polanyi 1964). In sum, this view of relativity accords with, and even perhaps partially explains, the view of objective epistemology as founded in the activities of the observer as research instrument. Objectivity provides a social context for the practice of relativity in understanding, just as relativity provides a cognitive context for relating and comprehending objectified texts. After the next section on the concept of culture, we will return to objectivity and relativity as correlates of role and image, respectively, in the field.

D. CULTURE

The idea of "culture" has always been integral to the fieldworker's perceptions of what he seeks in the host culture, and to how he translates his observations into a valid description and/or explanation. Our discussion here will not focus on the many definitions extant in the literature, but, again, on the assumptions used in the attempts to explain the nature of culture and the epistemology of knowing it. The treatment will be cursory, as this is an extensive theoretical and methodological area within anthropology and our discussion here cannot do justice to its breadth. After a brief note on the background of the concept, we will look at some of the early questions asked about ontology and epistemology as P-O developed within a positivist tradition. Then we will

examine some of the more recent alternate treatments which draw from the monist paradigm, before putting the concept into a framework within which it can be best understood in relation to objectivity and relativity in P-O.

A number of treatments of the history of the concept of culture are available (see for example Kroeber and Kluckhohn 1963; Voget 1960; Singer 1968; Keesing 1974; Vermeersch 1977) in addition to numerous analyses of its manifestations. "Culture" concepts share the same background influences as anthropology itself, as they quickly became a major focus of disciplinary theory and method. By the 18th and 19th centuries, scholars used the idea of culture to refer both to the universal, underlying rational processes shared by all people, and which underpinned the path of progress identified by the evolutionists, and to the empirical customs of particular groups of people, identified, for example, by Continental idealists elaborating on the unique spirits of each European nation. As evidence for cultural diversity grew, the second use became more prominent, in association with the empirical methods of natural history and classification. Both usages continued within the discipline, although in general definitions became, until recently, more concerned with "scientific" analysis of cultural criteria in context than with innate, "humanistic" components.

The British anthropologist E.B. Tylor is generally credited with producing the first "scientific" definition of culture (1873 [1976]: 36). Although Tylor believed that culture was the developed capacity of natural laws operating within man, his definition, which incorporated cultural holism, historical integration, ethnographic investigation, and social context paved the way for more pluralistic definitions (Singer 1968: 527-28, 537-41; Kroeber and Kluckhohn 1963; Voget 1960: 953-60). Yet these definitions did not really appear until the 1920s. The concept took a secondary role to the ideas of "society" in the work of most British functionalists, such as

Radcliffe-Brown, but Malinowski's explicit and implicit formulations were to have influence on investigations of both cultural ontology and epistemology through P-O.

We have already generalized Malinowski's approach to P-O as "contextual." Culture, for him, resided in the the social context of relationships within society -- a dualist ontology (Strathern 1981; Leach 1957). He saw it as pragmatic, utilitarian, a system of rules followed by the individual which could be discovered by the fieldworker. Culture was perceived as complex, integrated, and possessed of validity for practitioners (Kaplan and Manners 1972: 55-6; Richards 1939: 285-87; Kardiner and Preble 1961: 148-62). Malinowski's ideas about culture entered American tradition through his pronouncements on P-O and his treatment of Trobriand man as a kind of idealized cultural man. Boas's formulations were perhaps more influential in the U.S., however. His ideas were both similar to, and different from, Malinowski's; Boas never developed a full-blown definition of culture, but some of his students went on to elaborate the concept in mid-century.

Boas' approach to P-O, and to culture, was more "textual" in orientation than Malinowski's, we have argued. Like Malinowski, Boas wrote that culture resided in patterns which were empirical and integral to societal operation; they evolved by creation and diffusion in the history of the group. Yet, culture could be best approached through the individual and his/her version. The individual both represented the empirical existence of a complex, integrated culture, and provided a creative influence; thus Boas drew upon both dualist and monist intellectual heritage to reject law-like determinants of culture and to weigh the free role of the individual against the social context of societal function. Indeed, it can be argued that in abstracting culture from the creative acts of individuals, Boas gave culture a universalistic flavour at odds with his own particularistic relativity, and he had not yet completely come to grips with the

role of the fieldworker in creating cultural versions (Radin 1933: 3-29; Boas 1938: 159; Voget 1960: 946-48).

1. Ontology and Epistemology

Boas's students were also to devote more attention to the question of the role of the fieldworker. By mid-century there were many competing definitions of culture, as theory in American and British anthropology became less unified, and Boasian caution gave way to attempts to create new and diverse generalizations about the foundations of cultural existence, e.g., culture and personality theory and environmental determinism. Fieldworkers left the superficialities of salvage fieldwork behind and immersed themselves in complex, living cultures; this, too, influenced formulations of culture. Alfred Kroeber (1976) looked for culture in the repetitive patterns of history; Leslie White (1968, 1976) proposed a science of culture based upon symboling and its objective manifestations; and Edward Sapir (1949, 1956) looked at the role of individual and personal meaning in culture. ²⁶

The various theorists of culture debated several fundamental questions about the ontology of culture as discovered by the participating anthropologist: is it universal or particular; what is the role of the individual in culture; is it best perceived as abstract or concrete in form; what is the nature of culture in relation to other phenomenal levels of reality, such as psychology and environment; what is the role of communication in culture creation and sustenance; is it best approached through science or history? ²⁷ In sum, scientists debated whether culture was best seen as process or product, as objective or as relative knowledge. These questions are still debated. Once again, the insights of the fieldworker have provided the material for argument, and the epistemology of knowing culture through P-O has derived from assumptions of ontology. But, despite disagreement over these assumptions, the fundamental task of P-O, gaining insight into the insider's view of culture, has remained stable.

P-O was first grounded in inductive method, as we have argued elsewhere (see Pelto and Pelto 1973: 242-46; Pelto 1970: 90-3; Barrett 1976: 176-79; Silverman 1972: 224-28; Royal Anthropological Institute 1951; Powdermaker 1966: 285, 295-306). Assuming that culture exists as a separate empirical reality, the observer must find ways of gaining access to it. Several continuing directives, correlating with the P-O rules outlined in the previous chapter, have guided this process as the fieldworker tries to derive product from process. One is that culture must be portrayed in a holistic manner. A second is that the components of culture will be integrated into a function or pattern. A third is that this culture, because of its holism, is a key to other levels of investigation even if it is only a part of the researched reality. A fourth is that these patterns will have to be abstracted from their context in such a manner that the outsider, in putting them together holistically, may have a more comprehensive insight than any single inside participant. A fifth is that these patterns represent the ideal, and that real behaviour on other levels, even if "keyed" by culture, will exhibit more variation (one reason why insiders are thought to have only partial knowledge of their own culture). A sixth is that each element of culture has internal validity, as we have seen; a seventh is that the anthropologist must learn the nature of the ideal patterns in each culture before knowing what cultural features have cross-societal validity. 28

All of these propositions assume the movement of analysis from observation of data to construction of abstract pattern. And although they tend to remain intact in perceptions of P-O, both anthropologists and sociologists have come to realize that no fieldworker enters the field without some hypotheses in mind about what he will encounter. In addition, it is realized that he must continually create and test hypotheses in the field in order to apply scientific methods of objective reliability. This latter modification combines the strict inductivism originally associated with P-O with

alternate methods and techniques of analysis, and it has been advocated by both anthropologists and sociologists as a modified inductivism, as we have argued -- a circular process hinging on the ability of the observer to involve and distance himself. Silverman (1972: 220-28) calls this process ambiguation and disambiguation; Agar (1982) calls it breakdown and resolution (see also R. Cohen 1970; Agar 1980b: 122-34; Spradley 1980: 27-8). While sociologists have been less concerned with culture than with the social realm in their fieldwork, they have likewise combined positivist science with the more monistic frames of symbolic interactionism and ethnomethodology to create, as offshoots of the synthetic Chicago School of P-O, variants such as the grounded theory method of B. Glaser and

A. Strauss (1967). 29

2. Validity

The circular modified inductive method was gradually developed, then, in both disciplines as alternate theories about culture came to influence the discovery process. While fieldworkers continued to assume that culture could be discovered or constructed through a basic empirical epistemology, they came to realize that some of the ontological questions about culture could be asked, and tested through hypotheses, in the fieldwork process. The question of testing highlighted the question of validity, as we have already seen. Just as they asked, "is objective knowledge possible cross-culturally?" and, "is culture relative?", they asked, "on what criteria do we decide whether our version of culture is a valid one?" Circularity also highlights the dual stance of the investigator, wherein he becomes the hinge between internal and external validity. External validity comes from abstracting culture patterns from context: the building of text. It lies, as we have seen, in the trained skill of the detached observer who can replicate reality in his/her observations (Smith 1959; Human Organization 1950b). In addition, the text must be plausible by rules of scientific evidence as dictated by the discipline.

Essentially, external validity is thought to be the successful procurement of a text/interpretation that has validity to the insider; external versions must somehow be consistent with internal validity. Thus the external rules of observation are geared to evaluate the means to this end.

These rules may stipulate that the account be plausible to insiders, although not necessarily comprehensive (Agar 1980b: 77-81; Bloor 1983: 171-72; Johnson 1975; Baroe 1975: 20). It can also mean that the outsider can use the knowledge to "pass," or problem-solve, within the culture, according to the definition of the situation in symbolic interaction (Hayano 1979: 101-03; Lofland 1976; Glaser and Strauss 1970: 294-96; Emerson 1983: 11-13). Or, it can mean that internal (Other) and external (Self) interpretations are simply consistent with each other. In conventional P-O, there is a potential contradiction between using culture as the means to link Self and Other, and between using culture to define the "natural" differences between Self and Other. To address this problem, the epistemology of field relations has sometimes been reified into a cultural ontology, and objectivity as validating method has been confused with the source of such validation in this objectified ontology, as we have argued above.

Internal validity can also mean that insiders hold the text and the researcher accountable to their own standards and procedures of legitimation, rather than those of science (Bloor 1983: 156-57; Reinharz 1979). This latter is important, since it challenges these conventional views and takes the task of cultural creation and validation away from the researcher's sole responsibility. As practitioners of P-O increasingly incorporate the intersubjectivity of monism into the praxis of fieldwork, they increasingly realize the importance of creating a mutual context of social interaction which will allow a "true" or consistent interpretation, at least a temporary one. Guest and host must collaborate on re-creating agreeable standards of validity, including, in

many situations, conformity to accountability as well as, or in place of, the "passing" of participation in natural context.

In this they are explicitly re-creating culture, and more recent definitions of culture recognize this. Cultural reality cannot be uniform in significance for all insiders or observers, and it is host to the influence of situational factors (Lewis 1973a; Honigmann 1976b: 249-50). The centrality of any one account remains untestable, and emic/etic boundaries break down. The investigator remains pivotal, however, as he must be validated both by the norms of science and by the plausibility of insider knowledge. He becomes responsible for translating the commitment of social relations into the accountability of text. In the next section, we will look briefly at some alternate conceptualizations of culture before placing the concept into our framework.

3. Alternatives

The concept of culture has recently been refurbished within both sociology and anthropology to convey rules, symbols, dialogue, praxis, or metaphors. The idea of culture as rule, for example, shows up in the work of the phenomenologist Schutz and his line of influence into ethnomethodology, in the situational logic of I.C. Jarvie, and in the institutional analysis of F.A. Hanson.³⁰ For the most part all of these theorists blend a relativist phenomenology with a more institutional analysis. Cultural actors work from a stock of knowledge or rules, some of which they create intersubjectively in action, and some of which they take for granted as part of the institutional context. Jarvie and Hanson, for instance, point out that one must look, in analysis, beyond the limit of rule to its consequences in social/cultural behaviour and the perpetuation/change of institutions. The field anthropologist can use the concept of "rule" to map out the cultural knowledge of his informant, and to test the accountability of his reconstructed texts within the context of consequence. He must still deal with issues of how the rules are validated, internally and by the scientist, or whether they

are situational guidelines or transcendent rules of logic accessible to observer as well as observed.

Clifford Geertz's symbolic, interpretive theory of culture has played a significant role in directing participant-observers toward understanding the layers of symbolic meaning in culture as observed.³¹ Geertz has attended to the problem of culture as both process and product, using symbols as central analytic constructs which signify both models *of* and models *for* action. Symbols in culture allow multiple interpretations and provide a vehicle by which a model which is essentially "given" may be reinterpreted in the context of its use; they are objectifications of meaning and exist in a part-whole relationship analogous to the relationship of the individual to his socio-cultural context. The fieldworker can use a kind of "hermeneutic" circle to observe the patterns within which symbols are embedded, interpreting the symbol in relation to the meaning which it represents situationally; fieldwork allows the anthropologist to discover the interpretive genesis of empirical behaviour. This is an over-simplified version of Geertz's ideas, but it illuminates both the interpretive work of P-O in relation to culture and the problems of reconciling an intersubjective epistemology as cultural process with a necessary context for translating science explanation into cultural understanding. While Geertz believes that such a reconciliation is possible, his work is loosely woven enough to leave many open questions.

Geertz adapted hermeneutics to create his interpretive theory of culture. Like Geertz, these theorists stress the primacy of text as model or symbol over the pure subjectivity of knowing; yet, this text is meaningless without a grounding in a reconstructed context of intersubjectivity and meaning construction. Thus culture becomes dialogue and the dialectic of discovery of meaning. Validation comes from the creation of mutually-intelligible rules of evidence; thus culture is located in the consistency of text (Agar 1980a, 1982: 798-91; Dwyer 1979: 14-6; Rabinow and

Sullivan 1979: 19-21; Watson-Franke and Watson 1975: 247-53; Marcus and Cushman 1982: 42-6). As we have argued above, this often means that the use of the concept of culture presumes a common frame of reference for the participants to the dialogue which underlies the consistency, and which may not be present (Radnitsky 1970a; Hayano 1975). As Geertz (1973) has argued, cultural patterning must lie mid-way between the familiar and the unfamiliar.

Critics have questioned the relativity of culture as dialogue; thus other approaches, like the praxis theory endorsed by R. Bauman (1973, 1978) and P. Bourdieu (1977), have combined hermeneutics with more conventional modes of analysis, including Marxism and the rule theory already discussed. Like the monists, they emphasize the active construction of meaning through acts which generate and use knowledge. But they steer away from the idea that cultural reality is bounded as reified context or as a frame of reference for communication. Instead, praxis theorists see reality as multi-faceted and multivocal. Culture is process, and not a single product; it is the tool of the social interests which create it. The fieldworker is not detached from social relations but participates in them in a reflective way which allows critique of the conditions of production (Bauman 1973: 173-78).

Pierre Bourdieu (1977) rejects the concept of culture as a fiction of P-O. and instead identifies a structure which, for the individual, is a composite of historic and unique models and solutions for social existence: the habitus.³² But Bourdieu, like Bauman, takes refuge in circularity, and, failing to clarify the relationship of the scientist to what he critiques, shows the observer to be only the ultimate relationist, able to see an endless circular relation of text and context within which one can only be criticized against the other. The only way out is, again, through the transcendent rationality of the investigator who, after all, must use the detachment which praxis theory eschews in order to comprehend the process.

Metaphor theorists take up the idea of relationism as a focal point for culture. If one builds upon this idea, in conjunction with the emphasis of the preceding approaches to the *production* of culture in context, we can begin to look for alternate ways to envision culture which might bypass the inherent contradictions of epistemology and ontology, relativity of knowing versus objectivity of knowledge. Metaphor is a way of understanding the process of culture, whether it be rule use, negotiation, praxis, or other forms of validation, because it proposes that culture be extended conceptually from one situation to another as members make sense -- and use-- of it. As insiders or as observers, we use frames or symbols of culture meaningful in one context to interpret another, including the culture of the field. We can also invent or extend contexts as models for knowledge. As a result, we gain a relational perspective on the juxtaposition of semantic domains (D. Sapir 1977: 27-31).

Some writers have emphasized the centrality of metaphORIZATION and relationism in doing P-O and in writing ethnographic texts (see Parkin 1982: 3-7; Gudeman and Penn 1982: 98-104; Salmond 1982: 65-82; R. Wagner 1981: 8-14; Marcus and Fisher 1986: 17-75). It reveals how we extend our cultural reality to those we study, thereby incorporating them into our sphere of knowing. Metaphor also provides some insight into how culture can indeed be both context and text, process and product, by revealing, argues R. Wagner (1981), how we can invent each in relation to the other. This does not mean that the work of the anthropologist is perceived to be endlessly relative; it simply implies that the fieldworker, like his hosts, must create the joint interpretive frame -- logical or symbolic -- which links him with the Other, and that he must take this as a "given" or pre-existent background form. We objectify by bounding form to encompass meaning; and this form takes on a quality of contextual knowledge from which yet new texts can be understood or invented (R. Wagner 1981: 35-70; Lakoff and Johnson 1980: 20-35).

Whether or not one accepts a view of learning culture in P-O as a metaphoric process, the discussion above reveals that the method is based upon a combination of epistemology and ontology which requires that the observer superimpose his mode of knowing upon a presupposed reality -- whether a universalist or a particularist one. The approach to culture through metaphorization brings us closest to the framework we will propose here, which makes use of these processual definitions to help overcome the static circularity found in conventional, empirical assumptions about the nature -- and knowing -- of culture. As long as we look only for consistency in interpretation as a source of validation, then consistency must appear only in the perspective of the observer, who, after all, has the job of hinging the external and internal validity of culture through his/her own methodological dualism. To truly acknowledge the role of hosts in creating the ethnographic interpretation of culture, we must go beyond consistency to comprehend the role of negotiation, commitment, and communication in fieldwork. We must acknowledge that the outcome is contingent upon intersubjectivity, while also recognizing that the context produced through intersubjectivity is granted the status of "reality" through the acts of the parties to negotiation.

Culture as process and as product must do two things, then. It (1) must contextualize understanding, or give it meaning against a background of knowledge and symbol, and (2) it must validate: culture is used intersubjectively to measure communicative understanding against a common context. Both scientists and hosts use their stock of knowledge to invent culture-as-interpretation, using it as a "habitus" to generate behaviour, and legitimating it by imposing an ideal. Each objectifies himself through the Other, reciprocally, to make sense of experiences, and culture aids this process. In this framework, culture incorporates both objectivity and relativity; it supplies the valid content for the objectified form; this content is extended to context through relationism. As such, culture is more malleable, less substantive, and more

creative than any particular form it validates. As Bauman (1973), Geertz (1973), and R. Wagner (1981) agree, culture helps us fulfill our human potential as "rational" beings who transcend the alienating limits of our own bounded forms.

E .PARTICIPANT-OBSERVATION AS SYNTHESIS

All three concepts - objectivity, relativity, and culture - are integral to P-O. We have noted that, in both monistic and dualistic inputs into P-O, the researcher is the validating, and validated, instrument of observation and participation. Professional rules of conduct, based upon epistemological and ontological assumptions, confirm a circular modified inductive epistemology which has been idealized into an over-reliance on the hosts' culture as the source of ultimate validity. Only the insider can produce a "true" view of what the reality is like, in the "natural" context. The outsider must tap into this internal validity through immersion. Since, however, he becomes the instrument through which validity can be assessed, there is a potential contradiction between the singular limits of his own cultural understanding, and the source of confirmation of understanding in the natural context. As a resolution, the observer's rationalism and the insider's statements become translated, transformed, and transcendent; rationalism as epistemology is reified into another form of ontology (besides the natural context), the universalism which links internal and external validity.

By using the alternate definitions of the three central concepts which we have proposed, we can re-evaluate the rules and performances of P-O which produce the above dilemma. We reiterated at the outset of this chapter that role-playing is a central rule; this was described in the preceding chapter. A role, as it is reciprocally or individually formed, can be seen as a correlate of objectivity. It represents a bounding of symbol and attitude into projected performance, and as such is a form of knowing in itself. As fieldworker, and hosts, have long asserted, information and meaning are often

bound to the role of the holder. Roles, as forms, define and control the context through which meaning is reciprocally created in social relationships.

Corresponding to this, image can be conceptualized as the extension and relation of cultural forms, including roles. The image of the fieldworker may be predicated on the relation perceived -- by Self and Other -- between roles played, or between role and performance. Because image, as relativity, is tied to both objectification and culture, its evaluative connotations may result from the validating -- and mediating-- weighting of the relationship of text and role. Imaging reveals how performance is related to expectation; both are forms of knowledge.

Validation therefore is the next step, after the fieldworker has adapted, in his fashion, to the socio-cultural situation, and has extended his perception of the relationships embedded in this situation into a setting for images. Imaging is also part of the process by which the fieldworker "invents" culture; validation of images as culture follows and is part of the contextualizing function of culture. Utilizing his duality, the fieldworker also validates his invention by detaching his interpretations and giving them form as text. This text is either considered to be representative of the "natural context," reflecting a natural outgrowth of immersion, or it embodies context to the degree that its creator, fieldworker or teacher, is the embodiment of cultural meaning and "rationally" bridges the circularity of inductive duality. Both the contextualizing and validating functions of culture, following role-modelling and imaging, connect the two sides of the dualism by continually re-inventing a context of relativity, rationality, or ideology to provide a background for the interpretive form.

The processual view of culture outlined above presents validating, objectifying, and relational processes together as part of epistemology. Ontology as source is not opposed to epistemology as means, but can be regarded as a product of the ways in which these three processes interrelate in P-O. The forms of knowledge -- the values,

symbols, roles -- are never reified into a separate existence capable of self-contradiction but not self-reflection. Instead, they are texts which can become contexts for the continual invention and reflection upon meaning. In the reflection upon P-O now found in the anthropological literature, there is a trend away from viewing the subject of field practice as autonomous, a mere practice of rules. Both hosts and observers rebel against the loss of the "creativity" of their culture in its reformulation into anthropological texts. Processual and experiential views of culture are intended to recognize this incorporation procedure, and to remedy it by illuminating the relationships upon which the incorporation has been based. This chapter has attempted to look at both conventional and revised theories of P-O, and to present a framework within which both can be understood and the fieldwork process can itself be re-invented in response to changes in theory, field ethics, and accountability.

In our discussion of culture above, we noted that hosts are beginning to redefine internal validity as accountability; and, in Chapter One, we saw that accountability has come increasingly to denote and represent changing attitudes toward fieldwork and theory. The weightings of relativity and image which contribute to the field context are not the same for all participants, and hosts are asserting their right to set the scales for assessing validity. By broaching this concept here, we will set the stage for the case study which begins in the next chapter. Old rules of detached reconciliation are failing as fieldwork is increasingly seen as a creative, intersubjective process, and culture is seen not as ontology but as a function of the inventive epistemology of all participants, guest and host.

Two kinds of results follow from this, each flowing from the same changing perceptions of commitment. Commitment (1) is viewed not just as a "natural" part of context, but as an aspect of accountability. As Maruyama (1969, 1974) has argued in some of his writings on research methods, fieldworkers and hosts must realize that text

creation and validation are two-way processes in which mutual manipulation cannot be transcended by any one cultural 'rationality,' but must be met in mutual commitment to the relevance of that rationality for meeting the needs of each party to interaction. The more "praxiological" framework for P-O offered in this chapter was created in hopes that it could provide the basis for modifying the understanding of field praxis in ways more amenable to recognizing the role of accountability in validity.

A second (2) kind of result is the ideologization of relativity discussed in this chapter. Just as anthropologists once saw cultural relativity as a kind of commitment, this new kind of ideological relativity tends to see commitment as a possible, but not inevitable, way of overcoming a futile relativity of knowledge. Hosts are beginning to perceive the differences, and the relationship, between their own and the fieldworkers' weightings of culture and content, and to relate those differences to backgrounds not only in meaning but in social-structural, economic, and political positions. Ideological relativity results from a failure to overcome the differences in field practices; external relativity is idealized into the impossibility of the achievement of trans-cultural understanding outside of particular ideological commitments. Hosts often ask that fieldworkers adopt these ideological commitments in order to overcome a negative image of marginality as a symbol not of the blending of cultures, but of the failure to do so. The anthropologist/observer gains an image which reveals lack of commitment and understanding between cultures rather than the achievement of it.

The special status given by both monists and dualists to science as a special, insightful form of culture can only exist insofar as science takes a self-reflective, self-critical stance which examines its own rules and the circumstances of their use. Scientists and hosts are both demanding this; reflection will perhaps help anthropologists overcome relativity to accountability (along with socio-political changes). In the next chapters, we will present a case study of science practice, and

examine factors of relationships and response which have helped to shape it. This will give the reader an opportunity to ground the preceding discussion and alternative framework in a series of actual examples. Hopefully, this will demonstrate more effectively both the ever-changing operation of P-O as culture, and the role of P-O in creating culture.

CHAPTER THREE: CONTEXT OF SOCIAL RESEARCH IN THE DELTA

With this chapter we begin the case study portion of our examination of participant-observation as method. In the preceding two chapters, we have elaborated the growth of a set of rules and assumptions associated with P-O, and we have examined the logic of these assumptions. As a result of these investigations, a framework for understanding P-O as the process of learning and re-creating cultural knowledge was proposed. This framework incorporates the concepts of objectivity, relativity, and culture as complementary aspects of the bounding, contextualizing, and validating functions of knowing and knowledge.

In the case study, we will gradually apply these concepts to an examination of one setting for P-O. Participant-observation will be used as a comprehensive framework for understanding the way in which visitors to this setting, including social researchers, have interacted with, and learned about, host cultures. By using the concepts as we have re-defined them in the preceding chapter, we can see that the rules of P-O, as they arose from exploration and investigation, encompass in a general way a spectrum of participating styles. This allows us to compare the superficial interactions of some early explorers and traders with the more intensive social participation of missionaries and specialized social researchers going to the field setting. In addition, we can view the rules of P-O within the larger context of the objectives of sponsorship and regulation of social research in the field area.

The case study is not presented here as representative of all field settings for P-O. Although we concentrated more on commonalities than on diversity in the preceding chapters, fieldworkers have encountered widely varying experiences in research, as

influenced by the cultures and temperaments of guests and hosts, the quality of previous field experiences, and the knowledge and attitudes of hosts toward research. Yet, as we have seen, these different experiences can be understood and compared by the application of a general comprehending framework. By examining only one setting here, we can only apply the framework to the setting to better understand the praxis of P-O, but we cannot hope for comparison and generalization beyond that setting. The purpose here, therefore, is to use the case study to explain the framework, and to use the framework to illuminate the case study. Without comparative data, we cannot use this situation to explain P-O in general, nor can we "prove" the "truth" of our alternate P-O framework.

The case study setting is the Mackenzie Delta of the Northwest Territories, in particular the communities of Inuvik and Aklavik. Data come from several sources, including 6 months of fieldwork in Inuvik and 5 weeks in Aklavik, interviews with social scientists who have worked in the Delta and elsewhere in the North, discussions with the sponsors and regulators of social research in the North, review of published and unpublished documents relating to social research in the North, review of published accounts of Delta investigations, and a survey of archival materials from the Department of Indian and Northern Affairs relating to social research in the region. In the fieldwork phase, conducted from January-March and September-December of 1981, informal interviews and discussions were held with approximately 50 individuals; these related to experience and attitude regarding methods of social research previously used in the Delta. These hosts included both Native and non-Native residents of the Delta, and both those who had had direct experience with social researchers and those who had not. Field methods will be further detailed in the next chapter.

It is important to note here, however, that the more intensively the writer investigated social research in the Delta, the more obvious it became that an

understanding of both methods and response to methods could only come from an understanding of the broader context of that research, including the cultures of the resident populations; from the sponsorship, utilization, and regulation of research not only in the Delta but in the Canadian North in general; and from the social, economic, and political settings of the communities themselves. In this way the research broadened beyond the original purpose of describing and discussing local response to field methods. Only after this broadening took place could the writer then focus back on the issues.

The purpose of this chapter is to offer some highlights of that context. It is impossible to fully describe it here, however. We cannot do justice to the aboriginal cultures, to the economic and political development of the region, or to the nature of the communities themselves. Some of this information can be found in appendices or in supplementary sources; these areas have been well-documented by other writers. The focus here will be on the interactions by visitors with host populations from the 19th century to the time of my fieldwork, with special attention to the rules of interactions, the roles played, and the concepts of culture which have correlated with or emerged from these interactions. Explorers, traders, missionaries, trappers, and government workers all have come north to the Delta with specific purposes of interaction and outcome. By the mid-20th century these people had significantly altered the social and cultural world of the Delta for both Native and non-Native residents. The researches of social investigators which began in this century were laid on foundations of Native traditions and and non-Native transformations.

Several further cautions about the material are necessary. First, we call the people who have come north "visitors," in spite of the fact that many have become permanent residents -- "northerners"; this is done because most have come from elsewhere and have immigrated to the Delta for specific reasons, such as trade,

proselytization, or administration. They, like any visitors, have undergone a process of approaching and comprehending northern society through some mode of participation. Second, a full analysis of Delta society in the 1980s cannot be incorporated here, despite its relevance; only highlights can be mentioned. Third, one major actor in Delta society and economy, also a sponsor of research, cannot be taken into full account here. From the 1920s to the present industrial developers seeking non-renewable mineral resources in the Delta and the adjacent offshore region have been a significant presence, and they have sponsored some of the social research. This will be presented here primarily in terms of research style and method; a full explication of industry-sponsored research would entail a separate case study in itself. The industrial world of research is business-oriented and private, and therefore quite separate from other government and private-sponsored investigation; the writer was unable to get access to much of this information. In addition, much of the detail of the industrial context is omitted here.

A. THE DELTA

The Mackenzie Delta region today has a culturally diversified population living in five primary communities. Inuvik, the largest, is a multi-ethnic community with a large non-Native population, and it is the regional (Mackenzie) government centre for the NWT administration. Aklavik is also multi-ethnic but is primarily a Native community; it is on the other side of the Delta from Inuvik. Fort McPherson and Arctic Red River are Indian (Loucheux) villages, and Tuktoyaktuk is Inuit (Inuvialuit). All of the communities lie in the broad deltas of the Mackenzie and Peel rivers, bounded by the Richardson Mountains to the west and by the Caribou Hills to the east. Four of them lie within the treeline, which veers sharply north toward the coast along the Delta.

Tuktoyaktuk is on the Beaufort Sea coast, on the tundra, and is somewhat intermediary between Delta and Beaufort Sea communities.

In the pre-contact period, the treeline formed a rough border between the Indian population, primarily Kutchin (or Loucheux), and the Mackenzie Inuit. The Delta itself was largely uninhabited and was used only for occasional hunting and fishing by both groups -- and for raiding into the other's territory. The marshy landscape of river channels and small lakes did not contain any major aboriginal economic species except fish; there was also a source of flint near Arctic Red River. Early visitors to the Delta have given us some information about the aboriginal populations of Kutchin and Inuit that they encountered and about the nature of early contact; some of these will be mentioned in the course of discussion.

The Indian population of the Delta today are primarily of Loucheux ancestry; these people are known anthropologically as the Kutchin, but were granted the name "Loucheux," meaning "squinters," by early visitors, either in reference to their physiognomy or to their "quick sighted" nature (Wolforth 1971: 9; Krech 1979: 102). They identify themselves primarily as Loucheux or as Dene; the latter refers to the Na-Dene language family of Athapaskan language speakers, including the Kutchin. There are also resident Indian people from other culture groups, primarily from other Athapaskan-speaking communities in the western NWT.

There were nine aboriginal bands of Kutchin, characterized by cultural and dialectical differences. The Peel River, Mackenzie Flats, and Upper Porcupine bands were those who occupied the eastern reaches of Kutchin territory and who came to orient their fur trade around the Mackenzie River and Delta fur posts of the late 19th century, thus becoming the nucleus of the present Delta population (D. Smith 1975: 5- 7). Resident groups, composed of nuclear families, varied in size and composition with the seasonal round of activities and with available game. The Peel River band, for example,

wintered in the mountains to the west hunting caribou, living in small family groups. In the spring they congregated at fishing sites, seeking whitefish, loche (lingfish), herring, inconnu, northern pike, and salmon. In the summer they travelled to fish, to trade, or to attend ceremonials; regional bands met occasionally for trade or ceremony.

There was apparently some stratification among the Kutchin, especially the western bands, by wealth and class, and there were also three matrilineal sibs in operation. There is some current argument that the Kutchin, like other northern Athapaskans, were originally and predominantly matrilineal, with a bilateral and bilocal organization arising from the need for flexibility in the pre-contact period, and from the effects of disease and the fur trade post-contact (see Krech 1978a, b; 1980; de Laguna 1975). Nineteenth century epidemic disease may have reduced Kutchin populations by as much as 80%. The combined effects of disease and the fur trade caused some realignment of band groups and social organization. By the late 19th century, the Kutchin were beginning to move into the Delta proper on a semi-permanent basis to exploit the fur resources there (Krech 1978a, 1979: 110-14).³³

Apparently the Kutchin traded in the pre-contact period with their neighbours to the north, the Mackenzie Inuit, and with other Indian groups. By the time of contact, we know that the Indians and Inuit were also engaged in a retaliatory hostility toward one another which resulted in periodic raids and skirmishes in the spring and summer when travelling (or fishing) groups encountered each other in the Delta (Slobodin 1960, 1962: 16-8; Krech 1979; Osgood 1936: 60). Information on the Inuit is less complete, and writers and observers differ about their social organization and location. Usher (1971b) and McGhee (1974: 7-24) divide them into five groups, each living in relatively sedentary villages separated along the coast but interacting with each other and with adjacent Inuit groups. The Mackenzie

Inuit may be ethnographically classed as somewhat intermediate between the complexity of Alaskan Inuit culture to the west and that of the Copper Inuit to the east. They maintained some elements of a whaling complex, and had permanent villages with men's houses. The economy was based on beluga whales and fish, supplemented by caribou and other game. Like the Indians, resident groups varied in size and composition with the seasonal round; the largest concentrations of villagers occurred with the summer whaling and with early winter ceremonials. In the autumn they dispersed to hunt caribou; in the late winter and early spring they lived in scattered multi-family dwellings near fishing sites. 34

The Mackenzie Inuit first encountered the fur trade through other Inuit and Indian middlemen. They did not enter it directly until late in the 19th century when they traded at Peel River and Anderson River posts. An 1865 epidemic killed many, especially around the Anderson River to the east, followed by a reorientation to the coast from about 1890 to 1907. During this time they became involved with whalers operating in the Beaufort Sea, whom they often served as hunters and crew. The whalers brought further disease; measles epidemics in 1900 and 1902 killed about 90% of the Mackenzie Inuit population (McGhee 1974). The remainder came to inhabit the coast between Herschel and Baillie Islands, eventually becoming trappers and retaining as much as their language and culture as they could.

The Delta was repopulated by Inuit from the interior of Alaska, who followed the whalers east. When whaling diminished after 1907, these people came to occupy the Delta as trappers, followed by two more waves of Alaskan Inuit in the 1920s and the 1940s and the immigration of some Mackenzie Inuit descendents from the coast (McGhee 1974; Usher 1965, 1971a; Hargrave 1971: 187-95; Nuligak 1966; Smith 1984: 347-49; Stefansson 1913: 60-79, 1919: 23-32). The present Delta Inuit population, who call themselves Inuvialuit, have diverse origins among both Mackenzie

and Alaskan Inuit; dialect and cultural differences are still recognized in spite of some unity around land claims, social interaction, and political organization.

The other aboriginal population of the Delta are the Métis, individuals of mixed ancestry. The only substantive study of this population has been done by R. Slobodin, principally in 1962-63 (Slobodin 1964, 1966). The definition of this group is problematic, however. Slobodin (1966:6) defines the Métis for his study as "persons of known Indian-European or Eskimo-European ancestry, for the most part occupying the legal status of Whites, who consider themselves and are considered to be Métis." The Métis of the Mackenzie River Valley include both descendents of the Red River Métis, who moved into the area after the 1885 Resistance, and "northern" Métis descendents of intermarriages between Indian/Inuit people and non-Native trappers and traders who resided in the area within the past 130 years. Most of the Delta Métis belong to the latter group. Many of the non-Native forefathers were Scottish Hudson's Bay Company employees. John Firth, for example, served in the western Arctic for over 40 years (1872-1920), including 20 years as manager of the Ft. McPherson post. He married the mixed-blood daughter of another Scottish HBC employee, Alexander Stewart, and his Kutchin wife. The Firths had 17 natural and adopted offspring; both the Stewarts and the Firths initiated extensive, localized Métis clans, as did the children of A.N. Blake, an English trader (Krech 1979: 114-16).

Until recently, self-identification as Métis was stronger in the Upper Mackenzie Valley than in the Delta, where many mixed ancestry people long-identified themselves as essentially "non-status" Indians or Inuit (D. Smith 1975: 8-9; Wolforth 1971: 9-10). There has long been, however, as Slobodin has described, a distinctive Métis lifestyle blending aspects of both ancestries. The people have often been mobile (moving residence and employment), have been associated with wage work around the posts and communities (often in conjunction with a mixed land-based economy), have

used the nuclear family as the basic social unit, and have perpetuated wide-ranging kinship and communication networks.³⁵ In recent years, with the growth of a regional Native sub-culture which has encompassed all aboriginal groups, and with the political florescence of a Métis Association in the NWT, more people have been adopting the Métis public identity. They are seeking to replace the marginality of being between two worlds with a positive image and political voice in land claims and self-government. Métis are found in all of the Delta communities today, maintaining social ties both within the Métis group and with other resident populations.

There is also a significant non-Native population in the Delta, who today form the majority in Inuvik and who are heavily represented in government and industry. In the next two sections, we will look at these non-indigenous visitors to the Delta who interacted with the resident cultures. In section B we will briefly evaluate three groups of early visitors to the Delta, the explorers, the missionaries, and the traders. Then, in section C, we will begin our discussion of the involvement of government administration/administrators in the Delta as contemporary communities have developed.

B. EARLY VISITORS

1. The Explorers

The first southern visitor to the Delta was the explorer/trader Alexander Mackenzie (1801), who worked for the Hudson's Bay Company's major competitor of the time, the Northwest Company. In 1789 he journeyed down the Mackenzie River to the coast, erroneously supposing it was the path to the Pacific he sought for commercial purposes. He encountered some of the Kutchin people, and, although he found house sites of the Inuit, he did not meet any. The exploration period extended through the next century, and both Kutchin and Inuit quickly became familiar with the visits of these

exotic outsiders; word spread quickly even among those who had never seen a "white man." The visits of explorers overlapped with those of missionaries and traders, and had similar effects. Some exploration, such as Mackenzie's, was sponsored by the trading companies for commercial interests, whereas some was sponsored by British, Canadian, and foreign governments to establish or challenge sovereignty, and to determine topography, natural history, and human habitation (Dorion-Robitaille 1978; G. W. Smith 1963; Phillips 1967: 48-110). Many parties included a physician/naturalist whose duty it was to record information on living specimens, including humans.

Some of the first accounts of Delta people come from the explorations of John Franklin and his associate, the scientist Dr. John Richardson; they surveyed the areas around the Mackenzie River, the Great Bear Lake, and the Arctic coast from Alaska to Coronation Gulf from 1825-27. The journeys of Peter Dease and Thomas Simpson extended the territory in 1837-39, investigating for the HBC, and several parties searching for the lost expedition (1845-48) of John Franklin made further explorations later in the century, resulting in accounts of the people by A. Armstrong, W.H. Hooper, and again by John Richardson. ³⁶

Most encounters with the Native people were superficial and purposive. Only the naturalists such as Richardson and Armstrong were committed to recording details of culture and economy; and only a few northern explorers, such as John Rae (who explored for the HBC east of the Delta in 1846-54), and the explorer scientist V. Stefansson of the early 20th century (see Chapter 5), endeavoured to live intimately with the Inuit people as part of their explorations. There was little public support for these efforts, as it was widely considered that the only reasons to interact with Native people involved getting cooperation for trading, information, and provisions. The visitors played first the role of the stranger, awesome and intriguing; once initial

contacts were made and interest aroused, they adapted the role of the trader -- already familiar in its terms to Delta residents. Most explorers offered gifts in a display of generalized reciprocity and manipulation in order to establish precedents from which to bargain for their objectives; some, like Dease and Simpson (Simpson 1843: 106-10) used gifts to set themselves into a position of patronage, whereas Richardson (1852: 146) refused to do so. In accordance with the precedent set by Franklin, the crews drew boundary markers in the turf to guard their physical and social territory. Communication was through sign language and interpreters, and relationships were established with key leaders. Armstrong's ship, for example, took on Inuit passengers who impressed them as singular, and treated them to tours and clothing.

Rule, role, and reciprocity were of course conditioned by the response of the hosts. Initial fears often gave way quickly to curiosity and attempts to manipulate and control the strangers. The explorers interpreted this as trickery and theft, and, sometimes, as malice; Franklin's party was subjected to an aborted ambush attempt by Inuit, for example. Images and roles for the "white man" were developing according to experience and cultural codes -- and, occasionally, through misunderstanding. The explorers with beards who rowed boats like women did, who had talking books, and who wielded firearms were indeed power figures. But it was Native experience to negotiate their relationships with the spirit world and with the partners of trade, and they soon began to make their own expectations for weapons, clothing, alcohol, and protection known. As they observed the explorers trying to survive in an unfamiliar land, and making mistakes, many quickly realized that the "white man" was only too human to rise far above the position of cultural inferiority attributed to all "foreign" groups who could not match indigenous standards of cultural competence.

The observations which the explorers made of the aboriginal peoples were generally superficial, confined to what could be observed in brief encounters, and,

sometimes, the greater acquaintance with guides and interpreters. They interpreted Native culture through the windows of 19th century ideas of man and nature. To them, culture could be located in the "group mind" exemplified in personality and character; it was a force located in the collective habits of the people and known through individual predisposition. As an empirical and objective process, culture was subject to molding by natural laws and the environment. Armstrong (1857: 192-3) interpreted Inuit head shape as demonstrating the dominance of the "animal passions" among the people, which in turn shaped cultural attitude. Native culture was more basic, more enduring, even more exemplary than the European's, the explorers wrote, but it was readily subject to evolutionary improvement and guidance.

They are keen and expert hunters, and afford ample evidence in their appearance, look and movements, of being possessed of all the essentials to ensure success in the chase; and when so much depends on the result of their exertions - nothing short of their existence as a race amid the dreary wilds of their abode - it may readily be imagined how keenly the perceptive faculties are exercised, when such powerful incentives are ever present to prompt them to exertion. In regions where nature is so sparing of her gifts, with the exception of the animals which frequent it, I need not speak of the enduring patience, hardships and privations, which this enterprising hardy race are compelled to undergo, along the inhospitable, snow-clad coast, of the Polar Sea (Armstrong 1857: 196).

Some of the first impressions of Inuit and Kutchin nature can be traced to contacts with explorers (Oswalt 1981: 54-60). Inuit were perceived as aggressive, curious, intelligent, and motivated by greed (see Richardson 1852: 209; Armstrong 1857: 197-98). Comments on the Loucheux were more sparse; Mackenzie (1801: 249), who met no Inuit, felt that the Loucheux were more courageous and quick than neighbouring Indian groups, but most explorer/naturalists (e.g. Franklin, Simpson, Armstrong, and Richardson) believed the Inuit to be superior to Indians in intellect, aggressiveness, and courage. This stereotype persisted among visitors well into the present century.

2. Traders and Trappers

The fur trade did not actively come to the Delta until the 1840s, although prior to that goods were received through Native channels from the Russians in Alaska, and from HBC posts farther south. The "fur and mission" period in the Delta extended for approximately a hundred years, until the mid-20th century, and it created a new culture, social organization, and economy which has since come to be known as the traditional, frontier way of life of the Delta. The changes during this period are enormous, and we cannot detail them here. Native people apparently reacted to the trade at first with little change in their economy; indeed, in the first posts the traders were dependent upon the people for provisions and guidance, and many Native hunters simply added trapping to their existing round of activities. As fur prices went up, as the steam-powered boats replaced canoe and York boat brigades for transporting furs and supplies, as competition among traders increased, and as a cash economy replaced a barter one, Native people increasingly altered their occupations, residences, and customs to accommodate to the new economy.³⁷ As with the explorers and other visitors of the time, we have much less information on Native than on non-Native perspectives on the fur trade.

The first trading party to the Delta, led by Duncan Livingstone in 1799, was attacked by Inuit near what is now Arctic Red River; most of the party were killed. This ended efforts to establish trade with Inuit for the time being, but the Kutchin were drawn into the Upper Mackenzie trade early in the 19th century with the establishment of a post at Fort Good Hope. They quickly became intermediaries to other bands and to the Inuit, and resisted efforts by the traders to establish posts in their territories. In 1840 the Peel River post, near what is now Fort McPherson, was established, and came to dominate regional trade for the eastern Kutchin and for the Inuit, who began to trade intermittently at the post after 1850. The Inuit resented Kutchin possession of guns

and their efforts to interfere with the trade, and asked for their own post; one was built east of the Delta on the Anderson River in 1861 (HBC). Because it was located in an area difficult to provision, and because a scarlet fever epidemic killed many of the Indian and Inuit hunters trading there, it lasted only until 1865; the remaining Inuit in the vicinity moved to the coast (Stager 1967: 45-54; Wolforth 1971: 21-5).

After 1870 the HBC's monopoly was broken, although it was considerably later before competitors actually reached the Lower Mackenzie. Steam transportation, as mentioned, changed the pattern of trading after 1885 as traders at the posts were then less dependent upon Native hunters for provisions, as prices began to fluctuate, and as a greater variety of goods became available. Cash replaced barter as the medium of exchange, and, with the 1898 Gold Rush and the incursion of non-Native whaling ships along the coast at the turn of the century, increasing numbers of non-Natives entered the region and began both trapping and trading. In addition, the Inuit people began to work for the whalers on the coast, as we have noted, and some Kutchin and Métis went to the Yukon to join the Gold Rush, returning with new experiences and expectations.

As the fur trade progressed into the 20th century, it began to flourish under competitive conditions. Native hunters moved into the fur-rich Delta to trap (and to Banks Island to trap white foxes); this altered their residence patterns and economy. Although they had to compete with increased numbers of non-Native trappers and independent traders who came in around the time of WWI, many had become cash-wealthy by the 1920s. The community of Aklavik, which began as a post in 1912, attracted missions, retailers, and trading competitors by the 1920s and became a centre of Delta activities, increasingly drawing both semi-permanent and permanent settlers from the Native people. Mixed ancestry people formed a new population centered around posts like Aklavik, Arctic Red River, and Ft. McPherson. Posts were extended to the coast; the HBC set up a post at Tuktoyaktuk in 1938, a time in which

they had once again eliminated many competitors and were expanding in the north. By the late 1940s, a new frontier culture centered around the trade and the new communities had incorporated Native and non-Native alike, but, after several years of fluctuating fur prices, the trade collapsed with a post-World War II downturn in prices (Wolforth 1971: 43-62; Honigmann and Honigmann 1970: 28-52).

The patterns of interaction by traders with Native people changed as the position of the trader changed from dependent, to patron, to competitive merchant. Although there is debate about the effects of the trade on the people, it is generally assumed that both Indians and Inuit became more dependent on the fur trade economy (although not necessarily on individual traders themselves) as the period progressed -- the inverse was true for the traders. In the beginning, then, traders had their influence limited by their need to participate in the periphery of Native subsistence patterns, to sustain themselves, but participation gradually changed as they acquired the power to direct rules and roles toward their own objectives.

In general, traders attempted to define and delimit their trading roles, keeping themselves socially and culturally apart but often assuming patronage or guidance roles in the young post settlements.³⁸ Interactions were therefore more directed than they were for explorers. Most learned the practical value of living, to a degree, like the Native people, not only to adapt to the immediacies of daily life but to have a greater influence; we have already seen that some of them took Native wives, and these women were important intermediaries in the key relationships of participation. Firth, for example, was anxious to spread Christian belief, western education, and non-violence, and he often intervened in local affairs to these ends (Stewart 1955: 258-71; Slobodin 1966: 23-4; Waldo 1923: 243). Thus the traders of the late 19th, early 20th century became patrons, and their participation increasingly utilized key relationships, leadership roles, and directed reciprocity to influence Native strategies

of involvement in the fur trade supply and market. We also find the beginnings of the idea that time spent in Native society was both a mark of distinction (endurance) and necessary for effective local relations and trade communication (Asch 1977: 49-52, 1986; Stewart 1955: 247-83; Krech 1976: 221-32, 1979: 106-19, 1981; Stager 1967).

With the era of competition in the 20th century, the rules of participation changed once again. The trade became institutionalized, reducing the importance of the individual traders in maintaining Native involvement but increasing the competition among them to establish marketable relationships; some traders and retailers continued to have great personal influence on local residents (Savishinsky 1972; Dunning 1959). Native/non-Native relationships took on a gloss of equality in the competitive democracy of rapport-building, and cultural traits mingled in the communities with the evolution of a new frontier culture.

As economic reciprocity became embedded in the market economy, cultural reciprocity began to operate independently of it, and new rules of social interaction emerged in the communities which differed from those of the bush. Some of the non-Native trappers who came into the Delta after the turn of the century, with the increase of competition in the free fur market, also became independent traders or retailers. Many of these were Scandinavian in origin; some were Scottish/Anglo/Irish in background, and they brought with them a variety of social class and occupational origins. Many married Native women and fit into Native networks of kinship and reciprocity, and, as the non-Native population increased, they came to be associated with the lowest social class of non-Native residents. R. Slobodin believes this class position was derived more from their participation in the physical labour of trapping than from their relationships to Native people, however, and from their independence of imported conventions. These men were important influences in creating and

maintaining the "subversive rules" of the fur trade frontier in this era, and in merging Native and non-Native cultural characteristics. To the extent that they ignored the emerging class structure of the fur-wealthy Delta, they also succeeded in helping to create it by their own participative creativity (Slobodin 1985, personal communication; see also Lotz 1970: 110-12).

Insights into Native culture varied with the inclination of the trader and with the type of interaction he had with the community. Some traders in the Kutchin region, such as A. Murray (Ft. Yukon) (1910), W. Hardisty (1867), S. Jones (1867), and Poole Field (1957) had some ethnographic interests which led to intensive observations and interactions as well as cultural analysis. As Saum (1965) has pointed out in his overview of fur trade accounts of North American Indians, the observations of most traders were influenced by the circumstances of the trade encounter itself; features such as generosity/greed, astuteness, and physical characteristics show prominently in accounts. Increased contact did not necessarily produce increased understanding, under these circumstances, and many traders, including those of the North, felt that the Indians suffered from prolonged contact with the corruptions of "civilization" (ibid.: 68, 244-45) Like the explorers, they tended to generalize culture from personality, and, in spite of the pronounced relativity of some observers, many enjoyed positing "universal" characteristics of the "Native mind," thus perpetuating some of the same stereotypes encountered in the accounts of others (ibid. 132-33). The Inuit continued to be thought of as fierce, aggressive, and intelligent (Waldo 1923: 200-29), and the Loucheux were compared favourably with Indian neighbours. Both were evaluated on the evolutionary scale of the late 19th century and generally rated below Europeans in morality and intellect, although there were glimmers of the "noble savage" image as well in the records they kept. Murray's (1910: 62-3) opinion that the Native people were basically friendly and useful if properly controlled and managed in comport of

Christian trade appears to have been typical; therefore the grounding of culture was in the concrete context of socio-economic interaction.

3. Missionaries

Missionaries came into the Delta slightly later than the other two groups, building on the relationships with non-Native visitors already established. Most came alone, usually with few skills for survival in the North, so that they had to cooperate with either fur trade post or Native communities in order to subsist. Their purposes, to proselytize and convert, were often complex and quite different from those of other visitors; missionaries often wanted Native converts to retain the economic structures of subsistence while abandoning certain social and cultural practices (e.g., Native religion and shamanism, polygamy, and infanticide) considered to be un-Christian. This ambiguity of motive caused some ambivalence -- and ambiguity -- in the nature of participatory relationships with fur traders, with other denominations, and with their hosts, in that the religious emissary would be dependent upon his hosts in some respects and an active patron/teacher in others.

In the Delta, Anglican and Roman Catholic religions were the primary forces, and both entered soon after the mid-19th century intending to stem the damage incurred by traders, whalers, and disease on Native society. In 1860 Father Grollier went to Ft. McPherson for a year, and baptized there 65 Loucheux and 4 Inuit. The Anglicans came soon after, and quick conversion of the Kutchin people began, eventually resulting in a Catholic stronghold at Arctic Red River and a Protestant dominance at Ft. McPherson, with competing missions in Aklavik. Apparently the HBC, which had an official policy by this time of favouring no particular denomination, gave slight preference to helping the Anglicans with logistics and conversion, perhaps because of the Protestant

connection, and perhaps because the Anglicans made gifts of trade goods (Grant 1984: 108; Stewart 1955: 290; Wootten 1966: 17-20).³⁹

The Inuit proved more resistant to mission efforts. Both the Anglican W. Bompas and the priest E. Petitot visited them in the 1860s and 1870s, and they met with both curiosity and hostility as the Inuit blamed them for disease and misfortune. There were problems with language interpretation and misunderstanding also. In 1913, near Coronation Gulf to the northeast of the Delta, two priests were killed when they got angry over the theft of a rifle, and their Inuit companions attacked them in fear (Finnie 1948: 52-3). The Anglican minister I.O. Stringer, who later became bishop for the area, successfully established a mission among the Mackenzie Inuit after 1892. After the turn of the century, when missionaries competed with whalers for influence, they were indeed able to achieve rapid conversion of Inuit to Protestantism (Whittaker 1937; Stewart 1955: 312-17; Honigmann and Honigmann 1970: 35-8; Wolforth 1971: 34-6).

The missionaries generally tended to spend more intensive time with Native people, realizing that it was necessary to get to know their customs and languages in order to persuade them to convert. The priests were lone figures who, in spite of their exotic dress and celibacy, tended to live in Native camps and to devote themselves completely to their charges. Petitot, who had both an adventurous and an anthropological disposition, chose deliberately to live with the people to get to know them better, and to become more effective. The Anglicans tended to stay closer to fur trade posts, but they related to the people through such participatory measures as intermarriage and the training of lay leadership (Grant 1984: 114-15).

There was therefore some rivalry between the denominations not only in terms of converts but in methods. Both came to use the rules of language learning, key relationships, learning rules, role-playing, rapport-building, and reciprocity in

their work. Not only did they use Native languages to communicate, but they used them to teach literacy and Christian belief, often through syllabics. Shamans were singled out as rivals for religious influence, and leaders were courted in efforts to change marriage practices, prohibit "heathen" rites, and educate children; indeed, some shamans became church leaders. The Anglicans, in particular, made use of gifts to induce cooperation and to provide periodic relief. Both denominations had an uneasy acceptance of the economy of the fur trade. Missionaries realized that the Native trapping and hunting economy needed to remain intact, both as protection from damage by European society and to support the non-Native presence, yet they also wanted to promote some values associated with greater sedentism, such as literacy, church-going, and abandonment of alcohol. This resulted in some patronage competition for influence in directing the flow of immersion as each group of visitors re-created participatory rules to their own ends. (See Grant 1984: 96-118 for an analysis of these competitions among the northern Indians.)

In retrospect many Delta people view missionaries as exemplars of long-term commitment to the people and insight into culture. They are using contemporary criteria of evaluation, of course, comparing the lone missionary of yesterday with the insulated transient worker of today. More than likely some of the early missionaries were as authoritarian as they were considerate, and this was one cause of misunderstanding and conflict (Hendry 1969: 21-3; Savishinsky 1972: 283). As with other groups of visitors, some showed more interest and relativism in making cultural comparisons than others did. The records left by the missionaries can be deceptive, since they tended to overemphasize the hazards of their tasks in order to attract financial support from home communities. Some used compassion, and others used fear, particularly of disease and death, to accomplish conversions (Krech 1981). Critics have argued that conversion was often superficial and materialistic, and that the

mission boarding schools established and run by the churches in the Delta and on the coast in the early 20th century were damaging to the cultural fabric (Stefansson 1913: 405-31; Mason 1924: 58-60; Nuligak 1966: 58-9). Others suggest that the Native people gained advantages of literacy, and that they truly enjoyed a conversion to Christian principles, albeit on their own terms (Grant 1984: 113-18; Whittaker 1937); Delta residents are themselves today divided on these complex issues. Participation by missionaries, building upon the efforts of previous visitors, used many of the rules of P-O, but, unlike the deliberate P-O by later social scientists, it did not encourage a dual stance, nor a belief in a "natural context" from which this stance operated.

Although certainly some self-reflective missionaries had doubts about the validity of their results, their participation, like that of other visitors, was not a deliberate conduit to cultural insight, in spite of the ethnographic legacies of missionaries such as Kirkby (1872), Petitot (1970, 1981), and Whittaker (1937). While some espoused an evolutionist perspective, and others believed that the "savage" had fallen from an earlier state of grace, all believed that Native culture was both innate and malleable, like the personality of a child. There was a belief in group mind and cultural universals, brought from prior education, but missionaries also tended to interpret culture more as process than did other visitors, since they were concerned with the process of cultural/spiritual change. As Kirkby (1872: 419) pointed out, the reality of living with their hosts was often as much a negative experience for the Europeans as an enlightening one, and they were witness to the daily involutions and complexities of cultural knowledge. Culture was thus more multidimensional for the work of these religious leaders, and they came to realize a need to separate the rationalism of the converted individual from the natural collective mentality of the group.

C. GROWTH OF GOVERNMENT AND INDUSTRY IN THE NORTH AND THE RE-CREATION OF CULTURE

This section will highlight the development of the administrative sector in the North, with an emphasis on the Delta communities. Since this is a large subject, full coverage of the 20th century growth of government and industry is impossible here. Instead, we will touch on the types of participation that 20th century visitors have had in the North, and on the visions of culture they have created and enjoyed. Again, although some of the people who went north during this period have become permanent residents, the focus here will be on the transient, "visiting" sector. In many ways, these visitors - and permanent residents - set the rules and expectations of participation which social scientists have encountered in the area, although there are significant differences in methods and objectives. All of them have sought some measure of acceptance into the northern milieu and way of life. Their goals, and their successes, have varied, but they have all established rules, played roles, and have had assumptions about the nature of culture which have paralleled those which have developed within the narrow stream of anthropological P-O itself.

1. The Years before 1950

The Northwest Territories reached its present boundaries in 1912, with the completion of the boundary extensions of the southern provinces. In the early years the territories were governed from Ottawa by an appointed council, and there was little local administration apart from that operated by resident traders and churchmen. The first authorized government representatives were the Royal North-West Mounted Police [later the Royal Canadian Mounted Police, or RCMP]. They came to Fort McPherson, and to Herschel Island on the adjacent Beaufort Coast, in 1903 to regulate traders and

whaler and to protect sovereignty; their duties included the enforcement of the growing game regulations, the provision of relief when necessary, and the general duties of law enforcement. They remained the primary administrators in the Delta until about 1931 when their duties were partially taken over by a civilian administrator.

The government had few policies toward Native people in the NWT in the early years. The Inuit were not officially put under federal jurisdiction until after a 1939 Supreme Court decision, although various federal departments had taken implicit responsibility for them before this, providing occasional relief, RCMP services, and funding for missions (Jenness 1964: 40; Cook 1982: 8-10). The Indians of the Mackenzie Valley were asked to sign Treaty 11 in 1921 after oil was discovered at Norman Wells; they received only one of the promised reserves, and, like the Inuit, were subject to only *ad hoc* policy and supervision. Indian agencies were extended north early in the century; the closest one to the Delta was apparently at Fort Simpson, established in 1921. I have found no indication of an agent being stationed at Aklavik, which became a regional administrative centre in 1921, until around 1953. Indian agents visited the Indian communities on a rotating basis, supervising relief payments, housing, the establishment of band governments, employment, and other aspects of band membership and perpetuation (Dyck 1983: 203). Since they were not permanent residents of most communities, they too were aided by teachers, traders, and missionaries. In the Delta, there were some efforts at coordinating the administration of Inuit and Indians. For example, Dr. L. Livingstone was appointed medical officer in Aklavik from 1938-1944, and he was assigned general "welfare" duties for both populations (E. Stefansson 1945; D. Copland 1967; McEachern 1968; PAC, RG85, vol. 1911, file NR8/1-30).

Both RCMP personnel and Indian agents were part of a pattern of northern administration which continued until the mid-1960s in many areas: the enforcement of

external regulations was left largely to the discretion of the individual officer; duties were vaguely defined and their enactment legitimized more through the individuals than through the remote government departments. We have little information about the ideas that these people had about Native culture. Like other visitors, their impressions were presumably influenced by patterns of interaction circumscribed by duty and by intermittent visits to most of the communities. Traders and churchmen probably continued to have more sustained contact with Native residents as new establishments and missions were set up during these years. Aklavik boasted both an HBC and a Northern Trading Company post, followed by independent operators. Both the Catholic and Anglican churches built missions and hospitals there by 1926; the Catholics set up a residential school in 1929, followed by the Anglicans in 1936 as they competed for influence in an era of population mixing and free trade. Once Native people started attending schools, and spending longer periods of time in or near the hospitals, there was more continuing contact and interaction between residents and outsiders, resulting in the establishment of the nucleus of the present greater Delta community. Churchmen had an opportunity to teach and perpetuate their ideas about Native culture, particularly through the schools, but the era is probably best characterized as one in which formal rules and institutions were just emerging, and the persuasion of personal influence and patronage - from Native and non-Native leaders alike - was the primary power in shaping these institutions of northern participation.

2. The Years from 1950-1970

This was a period of rapid and dramatic change in many northern areas. The fur trade was declining, and people were gathering in settlements to seek work and relief; the federal government began to look north as post-WWII prosperity returned elsewhere and the public began to sympathize with social and economic problems in the North.

There was also an increased interest in establishing sovereignty and defense networks, and, with the construction of Distant Early Warning (DEW) line stations across the North, there was both an incursion of wage work for local residents and the importation of construction workers from the south. Government and industry began to look at the resource potential of the vast regions north of 60.

Family allowances and old age pensions were introduced everywhere, supplementing many failing northern incomes, and the government began to provide schools and hospitals. Day schools were built in Tuktoyaktuk in 1947, and in Aklavik in 1950. Native health services were also taken from the churches and provided through Health and Welfare after 1945. And the government set up various economic development projects to provide alternative incomes for Native people and a gradual assimilative experience; in Aklavik these included a sawmill, a fur garment-making project, and, as we will see, a variety of projects associated with the construction of the new planned town of Inuvik (see Jenness 1964 78-81; Wolforth 1971: 60-72; Hargrave 1971: 196-97; Zariwny 1977; W.A. Black 1959; Cantley 1950; Madsen 1976).

These changes corresponded with shifts in federal administration for the NWT. In 1950 Indian Affairs was placed under Citizenship and Immigration, and Northern Affairs (including Inuit administration) was placed under Northern Affairs and Natural Resources in 1953; in 1966 both were placed in the Department of Indian Affairs and Northern Development (DIAND), the precursor of the present integrated administration which will figure largely in research sponsorship (Cook 1982: 16). An elected NWT Council gradually replaced the appointed one, giving residents a greater voice in Ottawa, until in 1966 the A. W. Carrothers Commission, or the Advisory Commission on the Development of Government in the Northwest Territories, made recommendations leading to the institution of decentralized municipal governments for northern

communities, a territorial government with a capital in Yellowknife and an 18-member elected Legislative Assembly, and an enhanced role for the Territorial Commissioner as liaison between Ottawa (primarily DIAND) and the NWT government (Carrothers 1966; Hamelin 1979: 114-19; Madsen 1976; Drury 1980).

This is a sketchy outline of the early changes, but the effect of them was to introduce more government-hired personnel for the operation of administrative, educational, and health services. Essentially, the civil service of the North developed during this period. The first Northern Service Officers (NSOs) were appointed to various northern communities in 1954 (renamed Area Administrators five years later). Their duties were to set up and coordinate the various government programs and projects, and, as their numbers increased in the 1960s, they were to encourage community development plans. Aklavik was one of the settlements where Eskimo Councils were set up to advise the administrator in employment, leadership, and other areas. These early civil servants had a dual mandate typical of government policy at the time. They were to guide and protect the people, and they were to help them to help themselves through community development (Zariwny 1973, 1977; Ferguson 1968; Lesage 1955; Hunter 1976; Brody 1975; PAC, RG85, vol. 1660, file NR 4/3-6, pt. 1). A 1962 Handbook for Northern Administrators (Lotz, ed. 1962b) advised them to promote community awareness and leadership considered necessary to look after themselves, but this was to be done within a general assimilationist framework within which the Native people could eventually be trained to play a role in future economic development, including extraction of non-renewable resources and the provision of local infrastructure services.

Because there were still few non-Native people in most communities before the late 1960s, these administrators, like their RCMP predecessors (and Indian Agent contemporaries), wielded much individual discretion about participation and influence.

They were faced with building roles for themselves, on the one hand, and with depending upon close social ties with local residents, on the other. An incipient hierarchy developed, with non-Natives at the top, but this operated within a general context of "democracy" of social relations. The non-Natives began to set the rules for the democracy, singling out key Native residents as leaders and intermediaries. Another guide from 1962 (Hlady 1962) recommended living with the people, getting to know their language, and not taking their culture for granted; but there was much flexibility in how each administrator interpreted, or created, these rules of participation.

In a few of the larger governing centres, such as Aklavik, a larger nucleus of visitors was gathering even in the 1950s. Five government departments had offices in Aklavik, including the Department of Defense (Navy). The concentration of services only increased in these kinds of communities after the 1966-67 creation of a territorial government. Municipal governments at the settlement, hamlet, village, town, and city levels were gradually set up on southern models, with advisory or operating councils (depending upon the size and organization of the community) (Morgan 1973; Hill 1976; Drury 1980: 34-5). Non-Natives still functioned in most areas as managers and advisors, supplemented by teachers, Indian agents (in places with Indian populations), health care workers, RCMP, welfare workers, and, in varying concentrations, imported construction workers and military personnel. Many of these people were transients, working for a period of time before returning south or moving to another northern settlement. And, the more transients there were, the more they became insulated among themselves, developing internal stratification and dominance. They participated less in the Native community, working through Native patrons as intermediaries, and more in their own. Relations became more authoritarian, and unnecessary social contacts with indigenous residents were generally discouraged. This pattern was to continue into the 1970s. ⁴⁰

The town of Inuvik was created during this time, and its planning and construction exemplify government policies regarding Native people and the way in which non-Native personnel should interact with them. By 1953 Ottawa, through the Advisory Committee on Northern Development, was beginning to evaluate Aklavik's future as a government service centre, and officials were sent there to document problems with flooding, sanitation, the absence of gravel for land fill, and the lack of a suitable air strip site.⁴¹ There was also concern about employment for local people, with the decline of the Delta fur trade, and it was thought that if the community were moved to a new site then the ensuing construction would provide wage work opportunities. The decision to move Aklavik was approved by Cabinet before residents were informed; as it was announced in December of 1953, Aklavik citizens heard about it on the radio (Wolforth 1971: 68-72; H. Black 1975; Aklavik Journal March 1956; PAC, RG85, vol. 302, file 1009-3-2).

Plans were made to develop a modern, planned community to replace Aklavik. A site across the Delta was chosen, again without local consultation, which was considered by residents as ecologically poor in fish and game resources, but which met the logistical requirements of the government. The new town, named Inuvik at the suggestion of an ACND member (through an Aklavik politician) (researcher p.c. July 16, 1982) was to replace Aklavik completely, and residents were given various incentives to move, including compensation for homes and an exchange of lots. While Inuvik was being constructed over the period of 1955-61, however, various problems arose with the management of the move, and Aklavik residents mounted protests over poor planning and lack of consultation; many were simply unprepared to move (PAC, RG85, vol. 302, file 1009-3-6 and vol. 1265, file 1000-125; Boek and Boek 1960; Wolforth 1971: 68-72, Aklavik Journal March 1957; H. Black 1975). Many went to

Inuvik on a permanent or a temporary basis to work on construction; some of these returned to Aklavik when problems arose with hiring Native workers, with separation from families, with alcohol, and with lack of opportunities to hunt and fish. By 1959, there were 285 Native people in Inuvik, only half of them from Aklavik (H. Black 1975: 68). The old town of Aklavik survived.

Inuvik was set up to be an administrative service centre for the Delta and nearby communities upstream and on the coast. It attracted people for employment, for schools, to health care, for government assistance, and for escape from the pressures of life down south or in the smaller Native communities. A school, hospital, air strip, a Navy base, commercial establishments, government offices, two residential hostels for Native students (run by the churches) and recreational facilities were built to accommodate up to 3000 people within ten years. The town quickly became stratified and segregated in a southern urban model. Good, serviced dwellings were built in the east end for transient government workers. The west end, which housed Native people and lower-strata non-Natives, was not serviced and had small government houses (512s); there was also a tent town on the river banks which accommodated transient Native visitors and workers, and non-Native transient construction workers completed the population profile (Honigmann and Honigmann 1970; Boek and Boek 1960; Lotz 1962a). Already Inuvik gained an image which has haunted it to the present: that, in creating an ideal community, planners made an artificial, marginal one in which there was little interaction between east and west, Native and transient non-Native, as we have suggested above. Each segment became internally stratified, and mobility was slim during the formative years of the community.

All of these changes were taking their toll within the indigenous community (including non-Native permanent residents), both in terms of social problems and breakdown and in the relationships between these residents and their administrators.

There was resentment and confusion surrounding the move and the lack of consultation, and this became focussed on outside scientists, surveyors, and other personnel who offered "expert" opinion about the changes. Opinions about the competence of these individuals, and of most government personnel, became derogatory (PAC, RG85, v. 1656, file 2/3-24, pt. 1); a government official in Aklavik in 1963 noted a cynicism about the viability of government projects, such as the fish oil reduction plant which briefly operated there (PAC, RG85, v. 1415, file 251-1-4). These attitudes were to have some influence on reactions to social researchers, most of whom were sponsored by the government at this time (researcher p.c. May 19, 1982), and on the nature of the jointly-created rules of participation linking host and visitor.

The government workers of the 1950s and 1960s brought with them sets of assumptions about culture consistent with the times. The duality which the missionaries employed in their consideration of culture became more defined, splitting views of culture into the rational/practical dimensions of individual performance, on one hand, and a collective/innate dimension on the other. Native people did not think like the EuroCanadian -- yet; this was to be accomplished with assimilation and acculturation of the individual, who could then, with guidance, leave behind the collective dimension. And the collective dimension itself could be transformed into a set of abstract images and survivals. The Minister of Northern Affairs and Natural Resources in 1955, Jean Lesage, said that the northern people could offer these symbols as "cultural enrichment" to the south in exchange for the material assistance offered them in this difficult economic period. Increasingly, culture lost its aura of mystique and complexity, and became, to these visitors, a simple display of symbols, stereotypes, and lifestyle. The people and their culture became associated with the resource economy; they could be transformed and utilized in the new economy (see for example Stewart 1955; Peters 1943; E. Stefansson 1945).

3. The 1970s and 1980s

The social history of the Delta communities, and the rest of the Canadian North, becomes very complex during this period, and we cannot begin to do justice to it. Most of the data in this section are current only to the approximate time of fieldwork, 1981, or in some cases, the early 1980s. Since it was not my intent to do community studies, I was able to obtain only a partial portrait of life in Aklavik and Inuvik. Although there is some current information available on both communities, neither has been fully studied since the early 1970s.

During this period, local government in the North has evolved considerably. The Territorial government had, by the mid-1980s, a 24-member Legislative Assembly and an 8-member elected Executive Council; the majority of Assembly members were of Native ancestry. The territorial government has taken increasing control over budget allocation and program jurisdiction in health, education, social services, wildlife and renewable resource management, and many other areas controlled by provinces. They continue to deal with Ottawa through Indian and Northern Affairs Canada (INAC), the department responsible for status and Indian and Inuit affairs and northern development, but the role of the Commissioner has been taken over by the territorial government leader, a member of the Executive Council. The territorial government has, however, no control yet over its non-renewable resources, and this, along with a limited tax base and a lack of jurisdiction over its own constitutional development, retards its progress toward provincial status and fiscal independence from the federal government (although in 1988 they were negotiating a Northern Accord with Ottawa which would have increased their control over resource development and their share of direct revenues from royalties). Throughout the 1980s there have been continuing efforts by involved parties to split the NWT into eastern and western sections, with each evolving

independently toward self-government for indigenous people and eventual provincial status.

There have also been significant initiatives in developing government on other levels, including municipal and band governments, regional governments, and Native organizations. Aklavik, for example, has a hamlet council, which is the primary community-level government and which has gradually increased its range and competence in decision-making. Inuvik is a town, with a council and with greater powers to manage its budget and to levy local property and commercial taxes than is found on the hamlet level. Both have band councils, which manage the affairs of the local status Indian bands, and which sponsor projects in conjunction with the Yellowknife-based Dene Nation, which represents status Indians in the territories.⁴² Band councils in other areas are working to become the primary representative bodies, particularly in communities where Indian people are the majority. Both Aklavik and Inuvik have offices of the Dene Nation (associated with the bands there), the Métis Association of the NWT, and the Committee for Original People's Entitlement (COPE). The Métis Association has fostered awareness locally about the history and traditions of mixed-ancestry people; it has become aligned with the Dene Nation in working toward a land claim in the western subarctic and a form of self-government which incorporates indigenous traditions and representations.⁴³ COPE succeeded in settling a land claims agreement with the federal government in 1984. It represents the Inuvialuit, or people of Inuit ancestry, in the Delta and adjacent western Arctic coast.⁴⁴ COPE, and the regional development corporations which have replaced it, have become a significant economic and political presence in the Delta, and have long been working for a form of regional government for the western Arctic which would work in tandem with any future territorial or provincial governments; the people of the western Arctic have, however,

long been divided as to whether their fortunes are best linked to the Inuit east or the resource-rich west should the territories divide.

Although the policy in the North for all levels of government is to hire more Native and long-term non-Native northern residents, in fact the numbers of non-Native visitors have increased, especially in regional administrative centres like Inuvik, which in 1983 was 65% non-Native (population 3238) (Welcome North - Inuvik Drum, Oct. 1983). Government is not the only draw, however; both Inuvik and Tuktoyaktuk have attracted many outsiders associated with industry, particularly those exploring for oil and gas reserves in the Delta and Beaufort Sea. The history of industrial development is outlined in an appendix, and we can note here only a few features which are important to understanding participation and research. In 1970 Imperial Oil discovered oil in the Delta, sparking reaction from local business and from Native people; the discovery was an impetus for the development of COPE and the Dene Nation (then the Indian Brotherhood of the NWT), who organized to protect their concerns in anticipation of rapid development. Proposals were made in the early 1970s to build a natural gas pipeline through the Delta to bring gas from Alaska and from the Delta to southern ports. These plans further stimulated local and national interest in the battle between Native people, who were submitting claims to resource-rich lands and who were opposed to the potential negative social and economic impacts of pipeline construction, and developers who would export most of the rewards. The pipeline through the Delta was not approved by federal regulatory processes; a path down the Alaskan Highway from Alaskan reserves was approved instead.

Although the decision was made primarily for national economic and political reasons, one influence was the federally-sponsored Mackenzie Valley Pipeline Inquiry, into the terms and conditions of building a pipeline, led by Mr. Justice Thomas Berger in 1975-76. We will return to the "Berger Inquiry" in future discussion. The Inquiry

featured community consultations and hearings, a style of research which has since become very popular. It also gave serious attention to Native cultural and economic concerns, and played a role in disseminating nation-wide (through media coverage) new constructions of Native culture in the North. And it called attention to research being done by government, industry, and Native organizations in impact assessment, or the attempts to predict social, cultural, environmental, and economic impacts of construction and operation of development projects. Much of this research was done hastily and superficially, and it recast expectations --and doubts -- about methods and results of social research in the North.

Since the mid-1970s, an oil pipeline was approved and built south of the Delta, from Norman Wells on the Mackenzie River to Zama, Alberta. Another major federal review of alternatives for production and transport of oil and gas, the Beaufort Sea Environmental Assessment Review, resulted in a 1984 recommendation that a small-diameter pipeline might be built through the Delta, although there were reservations about the use of tankers for sea transport (FEARO 1984). This review also called attention again to the quality of consultative and impact research, as the socio-economic impact statements made by developers were initially rejected. Since that time, proposals have been made once again for a Delta pipeline, and several major international companies, including Dome Petroleum, Gulf Canada, and Esso have undertaken explorations for oil and gas, particularly in the Beaufort Sea. Gulf announced a major oil find in the Sea in 1984, and, although at the time of writing a recession in oil prices has hampered production, there are still plans to make the Delta/Beaufort Sea a major development area. In 1989 the National Energy Board opened hearings to consider granting export licences to three companies who want to extract natural gas by pipeline from the Mackenzie Delta. Northern residents are bracing themselves once again for a major development scenario.

Since the early 1970s the federal government has sought to balance its priorities in the north; the Department of Indian and Northern Affairs has a dual, often conflicting, mandate to serve the interests of northern Native people, including settling land claims and helping them achieve self-government, and at the same time responding to pressures from industry to develop northern non-renewable resources. Although a northern policy was announced in 1972 which apparently gave priority to the former, it lacked power and definition, and in fact the development interests have often been given precedence in recent years. Many doubt the policy's intent: whether a true "balancing" of priorities is possible, or that the government can succeed in managing impacts of development to the mutual benefit of all; this latter objective has been one stimulus to social research, as we will see (Dacks 1981; Hunt 1973; Dosman 1975; Cummings 1973; Hamelin 1979).

Industrial development has brought workers, executives, and business speculators to the region from the south. Tuktoyaktuk suffered from a period of serious social impact as several companies established camps nearby. As both government and industry have increased their presence in the area, Native people and resident business interests have increased their voice as well, seeking a stake in the future of community and economic development. By the time of fieldwork this had resulted in more of a conflict than a balancing of interests, particularly in Inuvik. The nature of interaction between hosts and visitors was changing rapidly. Native people were not passive recipients of development plans; they were taking an active role in shaping northern participation in spite of divisions among themselves. Although they had rejected pipeline development in the 1970s, they were more prepared in the 1980s to take advantage of it if and when it should come.

In the early 1980s the Native people succeeded in getting one land claim settled, that of the Inuvialuit; the Dene, Métis, and Inuvialuit have entered into economic

development and business ventures both privately and through the organizations. They were entering into contracts with oil companies to provide services and to manage impacts; for example Esso Resources set up a partnership company with the Dene Nation and the Métis Association for services relating to the construction of the Norman Wells pipeline. Many people were hired by government and industry or were receiving training, although this fell short of many northern objectives and of the policy of the legislation on northern oil and gas regulations passed in 1981, Bill C-48, which acknowledged increased employment of and partnership with northern residents. For example, COPE has signed exploration agreements with Esso Resources concerning terms and conditions of exploration and development on Inuvialuit lands. The successor to the COPE, the Inuvialuit Regional Corporation, was set in 1989 to embrace natural gas development because they held subsurface rights to some of the gas-producing lands. The people have also been active participants in impact management committees such as the Beaufort Sea Community Advisory Committee and its successor, the Beaufort Mackenzie Delta Development Impact Zone Society (funded jointly by industry and the two governments) (Stockermans 1984).

Life in both Aklavik and Inuvik at the time of fieldwork in 1981 was a "balance" among the interests of the residents groups in staking the future. There was some unity, as in both areas new generations recently reaching adulthood are surpassing some of the identity problems caused by rapid cultural change in the 1950s and 1960s. These include both Native and non-Native youth, who, in growing up together, are reaching a spirit of cooperation and understanding not previously present. Aklavik retains the community spirit engendered by resistance to its planned demise, and Native and non-Native residents alike participate in the land-based economy and culture which flourishes there alongside the extractive and wage work economy. There is shared

interest in overcoming some of the problems in the area, such as industrial impacts, the need for employment, and alcohol abuse.

The same is somewhat true for Inuvik, which has begun to overcome its image as an artificial, marginal town. A core of long-term Native and non-Native residents have strived to overcome division and stereotype and to create a multi-faceted town with a variety of businesses, services, and recreational facilities. As with Aklavik, a new generation of leadership is overcoming the old patterns of patronage. The west end is now serviced, and there is less physical segregation between Native and non-Native residents; there is also increased social participation among residents of all groups through private interactions and community organizations. Although there is little local land economy, there is shared interest in building an infrastructure for the town and in overcoming social and abusive disorders. And, in both communities, the "government" is often generalized and scapegoated into a common opponent against which all population sectors can unite; it takes on the mystical and incomprehensible force which Native culture once carried. The proliferation of programs, consultations, and offices which reside in or visit the area indeed cause confusion and disgust among local people, Native and non-Native, who can only generalize its collective presence in frustration. We will return to this point when we discuss research.

But divisions are still evident. Social and cultural segregation between transients, however, such as the Canadian Forces personnel (who have left Inuvik since the time of fieldwork), and resident hosts is still there, and there is internal stratification and division within both Native and non-Native sectors. Alliances among the players have frequently shifted and have been the cause of some doubt and suspicion among residents; some have seen an industry-Native partnership as the only realistic path towards a Native stake in the economic future, whereas others have perceived it as a buy-off. Native entrepreneurs have had difficulties reconciling with Native

organizations who encourage collective rather than individual economic investment; the Native organizations were sometimes accused of being run for the profit of their directors. Business interests in Inuvik, represented strongly in 1981 on the Town Council, were also uneasy about powers and opportunities being gained by Native organizations, whereas the latter feared an alliance between commercial and government interests. Each group sought to maintain a stake in the viability of the future community.

These are just some of the issues which have kept factions operating in counterbalance to unity. Just as federal policy in these years emphasized balance, the visitors who have gone to the area to live and work have faced a balance between unity and division, participation and insularity, acceptance and refusal. Pressures mount on the newcomer to demonstrate acceptance by the community, particularly by the Native residents whom many of them serve. As in former years, the building of rapport is often facilitated by establishing key relationships in the Native sector. Increasingly, the new visitors also face pressures to demonstrate commitment. This is evidenced not only by the amount of time spent in the North, which, as in P-O, can be singled out as rule and criterion, but by attitude, rapport, and a willingness to engage in local networks of reciprocity and visiting. And the move toward local control of governing processes, evident on both government and Native organizational levels, provides an added stimulus to commitment and active participation in community institutions.

Yet the various sectors, Native and non-Native, transient and permanent, retain within themselves the seeds of insularity, and this is perhaps best seen in the realm of culture. Derek Smith (1975) has documented the evolution of what he deems a northern Native sub-culture, which unites contemporary Inuit, Indians, and Métis in a social strata of Delta society. The more the Native people have found themselves placed in the lower classes of an integrated northern society, the more they have coalesced in

some of their cultural values and attitudes. This includes an emphasis on the traditions of the "traditional" fur trade, on individualism and self-reliance, on social marginality and "frontier" behaviour, and acceptance on personal merit rather than class or background. Although cultural and economic differences among these indigenous peoples are maintained, there was also, at the time of fieldwork, recognition of a common indigenous heritage and social position. Political efforts toward recognition of land claims and self-government, which began during the much-publicized Berger era, have worked as well to revitalize a common Native heritage as both value and as ideology. The symbols of culture, such as those associated with land, harmony, and a traditional existence, took on new meaning in a kind of "opposition" ideology to combat development interests (Koster 1974; Paine 1977; Larsen 1983).

The transient sector also has an internal culture, as developed in the 1950s and 1960s. This includes some stratification by occupation and income, and some division according to social networks and length of time spent in the community. Sets of rules and expectations guide participation and visiting, and those who do not fit in often leave quickly. There was also evident, in the early 1980s, a tendency to adopt the frontierism of the traditional heritage and of the Native culture as a self-image of northern living. Many non-Natives took pride in the same kinds of values noted above, with an emphasis on exaggerated, semi-eccentric behaviour associated with marginality. Indeed, the paradox of acceptance by hosts/insularity has itself become an acceptable stance, much as a similar marginality has become a dominant image in P-O.

The dual perspectives on Native culture have continued; there are evaluations of culture as individualized and as collective. There is less overt emphasis on assimilation as the primary destiny of Native people, but individuals who succeed in non-Native institutions in a "rational," individualistic manner are still applauded. Although the rounds of consultation (to be discussed in Chapter Five as a research method) treat the

single speaker as "representative" of his background and culture, the disinterest in Native culture of the 1960s, and the reliance on collective stereotypes of the "group mind," have been mitigated. Only the long-term residents still actively perpetuate images of Native behaviour such as childishness and impulsiveness; many Native residents have fought to dispel these images, and newcomers maintain some open-mindedness in regard to these impressions. One of the most important factors, however, has been the increased complexity of societal relations in the Delta; as both non-Native and Native populations have become more diversified in their attitudes and experiences, the rigid stereotypes have become less valid. They are being replaced by new interest in the potential of culture as ideology and behaviour.

There is a renewed romanticized interest in Native culture, therefore, corresponding with the perpetuation of tradition by Native hosts themselves. As they did in earlier eras, visitors seek the "real" Inuit or Indian, associated with bush living, not the ones somehow ruined and marginalized by urban life. Again, the Berger Inquiry provided a forum for these images to come forth (see Alexander 1976; Turner 1977 for critical views of this). Industrial developers have seized upon this notion of culture for their own purposes: to demonstrate that traditional culture is no longer viable. Through the revitalization of symbol, culture has, in its collective dimension, become known as a concrete lifestyle with a visible overlay of ideology. It is not reduced to stereotypes, but the complexity is masked by this ideological relativism, which emphasizes cultural differences and symbols. And it is marketable, a set of symbols exchanged and re-created in the competition among factions and sectors for a committed stake in the future.

The more culture is regarded as malleable and marketable, the more these attitudes reveal current ideas about a more responsive, inclusive participation in northern society. Culture is not so much abstract, in the modern Delta/North, but an

act of faith and commitment to a way of life. Native people use it to reaffirm their heritage and potential. Non-Native people use it to validate their presence, seeking acceptance into the norms of northern society and defining their own "frontier" culture in terms of this intersubjective agreement. Culture takes on the contextualizing and validating functions discussed in Chapter Two, with added complexity deriving from the complexity of the intersubjective parameters. The players are trying to find the limits to what is truly "real" and valid about culture, and, in the process, they must ground it in a participation which goes beyond the individual, and his faith in the marginal and unconventional, to the relating and objectifying context from whence marginality takes its meaning.

As we proceed with the case study, we will see how social researchers have played a part in the models of participation and of culture. In the next chapter, we will stray from the theoretical and substantive issues of the case study to survey the agencies and institutions involved in the sponsorship, conduct, and regulation of social research in the North.

CHAPTER FOUR: SPONSORS OF SOCIAL RESEARCH

In this chapter, we will briefly mention some of the sponsors, regulators, and utilizers of social research in the Delta and elsewhere. The chapter serves both as a continuation of the discussion of research context in the previous chapter, and as an introduction to the major actors to be followed in succeeding chapters. These actors include the federal government, the territorial government, universities, and Native organizations. As mentioned in the previous chapter, we will not pursue industrial developers as sponsors except superficially. Much of the actual work done for industry in the Delta has been done by private consultants; not all of it is fully accessible (Symons 1980: 41; White 1981: 10). The style of investigation used for impact assessment, and the problems with its coverage of social and cultural issues, will be mentioned for comparative purposes. The emphasis in this chapter will be on sponsorship functions. These overlap, however, with regulatory and coordination functions discussed in later chapters.

A. FEDERAL AGENCIES

The focus here will be on the department of Indian and Northern Affairs Canada (INAC) and its predecessors. The department was originally the Department of Indian Affairs and Northern Development, DIAND, and then was known as the Department of Indian and Northern Affairs, DINA; both of these latter acronyms will be used in the text. The other agencies selected here are also active in the sponsorship, regulation, and coordination of research.

1. Department of Indian and Northern Affairs

One of the first offices created for federal social research was the Eskimo Research unit, created in 1952 under the Northern Administration and Lands Branch of the Department of Resources and Development. Its functions were to research economic development projects for the Inuit, including relocation and business loan programs (Arctic Circular 1952: 65). It was short-lived, for the new Department of Northern Affairs and National Resources (NANR) was established in 1953, supplanting Resources and Development. The new Department was given a mandate which has continued through the present Northern Affairs program: responsibility for "fostering, through scientific investigation and technology, knowledge of the Canadian north and of the means of dealing with conditions related to its further development" (Senate of Canada, DIAND Report to the Special Committee on Science Policy, No. 31 1969: 4369).

The inter-departmental Advisory Committee on Northern Development (ACND), chaired by Northern Affairs, was revived from dormancy about this time and took an interest in promoting this mandate through its sub-committee on Research and Development (later [1959] Scientific Research, then Science and Technology). The mandate of the sub-committee was to discuss and exchange information on northern-related research going on in the various departments concerned with northern development, and to coordinate the flow of research information on behalf of ACND (PAC, RG85, vol. 1654, file NR1/1-13). The Secretary for ACND, Graham Rowley, of NANR, identified in 1953 the need for an information centre within the new Department; the idea had already been proposed by the chairman of ACND as members realized that the acceleration of research during and after World War II demanded more governmental coordination and access.

In 1954 the Northern Research and Coordination Centre (NRCC) was set up within NANR but under the direction of the ACND Secretariat; the latter would insure that its work would be of benefit to all departments. This work included the collection

and dissemination of scientific and technical information, the coordination of research activities in the North, and the direct sponsorship of research in areas not already covered by other departments besides NANR. One of its officers was to serve as secretary to the ACND sub-committee on Research and Development (PAC, RG85, v. 1654, file NR1/1-13; v. 549, file 1003-1-4; researcher p.c. July 16, 1982; Dec. 8, 1982).

Social research was the area most neglected by other departments, and the new officers of the NRCC began to push the need for anthropology in particular. Research was sponsored by grants to institutes and individuals, by seasonal hire, and by contracts for particular projects (researcher p.c. July 19a, 1982; PAC, RG85, f. 1654, file NR1/1-13). The NRCC took responsibility for science and exploration licenses, and it established the Inuvik Research Laboratory; these will be discussed below and in later chapter). As NANR was reorganized in 1959, the NRCC became the Northern Coordination and Research Centre (NCRC). And in 1961 the Centre began a program of administering training grants to institutions, primarily universities, sponsoring northern research; these grants were to train young scientists for northern work.⁴⁵ The federal government was by the mid-1950s encouraging universities to set up northern institutes, such as the now defunct Institute for Northern Studies at the University of Saskatchewan (one of the first), to develop northern expertise, and the training grants aided this purpose.

The NCRC maintained some structural autonomy by remaining under the ACND secretariat rather than under any division of the Northern Administration Branch of NANR; its programs were coordinated by the ACND sub-committee. There were conflicts, as the Branch complained that it did not know what the Centre was doing, and the Centre said that it could not meet all of the demands put forth through the Branch (PAC, RG85, v. 1897, file 1003-1-4). Two years after NANR was disbanded, and

northern affairs were combined with Indian Affairs to produce the Department of Indian Affairs and Northern Development (DIAND) (1966), the NCRC became the Northern Science Research Group (NSRG). With the new name came a new structure, as the research centre became part of the Northern Affairs structure. Its function increasingly became one of simply advising the department on northern science and technology.

Although its independence in sponsoring research was limited, the Northern Science Research group continued to emphasize social research, particularly the adaptations of Native people to changing environments (Senate of Canada, DIAND Report to the Special Committee on Science Policy, No. 31 1969: 4384). It organized the Mackenzie Delta Research Project, the first large multi-disciplinary project with an areal focus; it was oriented to social, cultural, and economic issues. It continued also with the Research Laboratory, the training grants, and the coordination of research, but the importance of the research centre declined with the loss of the connection to the ACND, even though its officers continued to chair the ACND sub-committee on Science and Technology (researcher p.c. July 16, 1982). With another reorganization in 1973, the unit became the Northern Social Research Division (NSRD).

At the time of fieldwork, 1981-82, DINA had become a large bureaucracy in which research operations were parcelled by function, and the NSRD had lost its centrality. It was located in the Northern Affairs program, Northern Coordination and Social Development Branch. It had a Circumpolar Affairs unit, a documentation service, the training grants program, the administration of the Northern Scientific Resource Centres programs (including the Inuvik Lab), and provision of core funding to the Association of Canadian Universities for Northern Studies (see below). It was sponsoring only a few in-house research projects; most work was contracted out and

was more limited in scope than had once been the case (DIAND 1980: 15; ACND 1981: 95).

More recently, in 1983, there was yet another reorganization; the NSRD became the Office of the Northern Research and Science Advisor, under the Northern Policy and Coordination Branch. Its duties have not changed, but it was further placed in an advisory and managerial rather than a sponsorship position (Information North Spring 1983: 2). The centre absorbed the formerly independent position (in DINA) of Science Advisor, and there was in 1983 a new emphasis on coordination with industry and universities as, in the 1980s, these institutions have struggled with government for regulatory control of northern research. In addition, the centre must coordinate research within the department, and, given the potential conflict between development and Native affairs sectors, as discussed in the last chapter, this is a difficult task (Savoie 1983: 140). With loss of funding, a decrease in interest in northern research within DINA, the problems of cooperation, and the lack of an independent critical role, the unit has lost much of its potential since the 1970s (researcher p.c. July 21, 1982) By 1988, the unit was known as the Scientific and Circumpolar Affairs Directorate, but was still located in the Northern Affairs Program; it had lost the responsibility for northern laboratories but was still administering the Northern Scientific Training Program, the Circumpolar Exchange Program, a Northern Science Award, and various researches (Symons 1988: 99).

The NRCC and its successors were to serve coordinative roles both among northern departments and among the divisions of one department, Northern Affairs: research has been sponsored and utilized on all these levels (Lotz and Valentine 1963: 419). The ACND sub-committee on Science and Technology, one of the most active committees in the ACND, is one body through which this was attempted. The ACND itself lost some of its coordinative power during the 1970s for various political and

bureaucratic reasons, including the lesser prestige that its chair, DINA, has among other federal departments (researcher p.c. July 16, 1982). The science sub-committee continued to operate, supervising training grants, the research laboratories, the creation of guidelines for scientific activities in the North in 1976, and the recent creation of a Science Institute for the North, as we will see in Chapter 7 on science regulation.⁴⁶

There has also been a departmental Research Committee, within Northern Affairs, which has had a more uneven history. Since its beginnings in 1954, it was intended to link the NCRC with the rest of the Northern Affairs branches and programs, such as the Canadian Wildlife Service, the National Museum of Canada, the Arctic Division, and the Territorial division. It was to be a forum to discuss problems, set research priorities, consider grant and contract applications, review progress, and set procedures for communication and liaison among sectors (PAC, RG85, v. 549, file 1003-1-4-1954-57/NCRC; v. 1897, file 1003-1-4 (pt. 3), NRCC 1962-65). By 1962 the Committee was already running into problems; again there were difficulties between the understaffed NCRC and the requirements of the bureaucracy, and an attempt in 1963 to create a sub-committee to improve relations also failed (*ibid.*). At the time of fieldwork a Northern Program Research Committee still existed, with representation at the Director general level from the various divisions in Northern Affairs and secretarial/coordinating functions still served by the Research Division. The terms of reference were similar to what they had been twenty years before, with more emphasis here, too, on research utilization and liaison with other sponsors outside government.

As we have noted, the Research Division has also had responsibility for operating the Inuvik Scientific Resource Centre, or Laboratory; this institution is crucial for understanding methods and consequences of research in the Delta. Laboratories were also

built in Igloolik in 1975 and Iqaluit (Frobisher Bay) in 1978, and all were coordinated jointly in the early 1980s by the research unit and the ACND sub-committee on Science and Technology. The idea of constructing a laboratory in Inuvik came originally from the Canadian Wildlife Service (within Northern Affairs) in 1958; this division was already doing Delta research and needed facilities. The ACND sub-committee became interested in the idea; this was the time when Inuvik was being constructed and there was high interest in northern development. Inuvik would provide an accessible site for access to the entire Delta (boreal) and coastal (arctic) regions, and the presence of the Lab would add prestige to the new community and bring local residents into the science enterprise (Espie 1980: 3). Should the Lab be successful, it was thought, others could be built elsewhere (PAC, RG85, v. 1654, file NR1/1-13; v. 1659, file NR4/2-2, pt. 2; Warner 1975: 20-9, 88).

The Lab was operational by 1964. Its first manager perceived it to be a community resource, and the facilities were opened to community use; a Science Club was also established (Warner 1975: 41-51, 69). The first 10 years were very active scientifically, particularly in the physical sciences, reaching a peak with the oil and gas exploration, and the Mackenzie Delta Research Project, of the early 1970s. Little research was sponsored in-house, but accommodations, office and laboratory space, and logistical support were offered to users, primarily government and university-sponsored. Since 1980 profit-oriented researchers have been charged a fee for the facilities. Use of the Lab has dropped since the mid-1970s as development interests have shifted more towards the Beaufort Sea, and industry has done more of its own research; the federal department of Energy, Mines and Resources, through its Continental Polar Shelf project, built a science facility at Tuktoyaktuk also (Espie 1980:5). In the early 1980s social research itself represented only 10% of projects (ACND sub-committee on Science and Technology 1981: Appendix F-2).

Some feel that the Lab has suffered from a lack of planning and priorities; it has not fulfilled its original purposes, particularly in the involvement of local people. By the early 1980s some residents had gained employment there, including Native and non-Native people, but the Lab was no longer used by local organizations and few people in the town knew much about its operations or projects. They felt that the Lab operated independently of the community, with no local problem-solving emerging from the research and no consultation about priorities (Warner 1975: 68-9; Espie 1980: 17-18). Even though DINA has been responsible for the running of the labs, the agency has not set any priorities for research either; and there was confusion in the early 1980s over whether the Research Division or the ACND sub-committee (since 1974) was ultimately responsible for its management, including priorities and planning. In addition, the Yellowknife regional offices of DINA had control of immediate administrative support for the Lab. The issues of control and regulation will be further discussed in Chapter 7.

There have been other units and branches in the Indian and Northern Affairs superstructure over the years which have sponsored social research. As noted, other divisions made recommendations to the NCRC in the 1950s and 1960s about research that they needed for their own planning. Some engaged in direct sponsorship, such as the Industrial Division set up in the 1959 NANR reorganization. It sponsored a series of area economic surveys as part of its responsibility for northern economic development (through cooperatives, tourism, and handicrafts, for example (Cook 1982: 17). The area economic surveys, several of which covered the Delta and adjacent areas, were designed to produce information on regional economic productivity and needs, and to suggest ways to tap resources for small industry. And from 1957 to 1963 the National Museum of Canada was located under the Northern Affairs Branch. Its efforts were coordinated with those of the NCRC through the departmental Research

Committee. Most of its researches around the Delta have been archaeological rather than ethnological, although it has sponsored both kinds of investigations across the North (researchers p.c. Oct. 4, 1982; Dec. 10, 1982).⁴⁷

More recently social research sponsored in the department has been oriented to industrial development and its impacts, to land use, and to land claims. The Office of Native Claims has, since 1973, offered "loans" to Native organizations to finance land claims. The Research Branch of the Policy, Research, and Evaluation Group (Corporate Policy) was, in 1982, carrying out policy-related social investigations relating to impact assessment, the in-house accumulation of data bases, and treaties and historical research, for example (researcher p.c. Dec. 9, 1982, DIAND1980: iv); this was done both by staff and contract. In the early 1970s, the sub-committee on Social and Environmental Research, Task Force on Northern Oil Development, sponsored economic research on a similar basis, some of which was carried out in the Delta region. And the Resources and Economic Planning branch of Northern Affairs was doing some social science relating to the development of non-renewable resources in the North (DIAND 1980).

More recently, the Canadian Oil and Gas Lands Administration (COGLA), which joins Northern Affairs with the Department of Energy, Mines and Resources, set aside the Environmental Studies Revolving Funds after the administration was authorized in 1982. These funds, to be set aside by industry for research relating to the impact of oil and gas development on the physical and social environments, were to be prioritized and allocated by government/industry advisory boards to universities, consulting agencies, public agencies, and Native organizations. The Office of the Northern Research and Science Advisor was to link these investigations to other northern researches relating to oil and gas development (ACUNS Annual Conference Proceedings 1983: 6-7; Information North Fall 1982: 1; DIAND 1982: 65).⁴⁸ These other researches include the

Northern Oil and Gas Action Program, NOGAP, begun in 1984 by the federal government to provide resources for planning northern hydrocarbon development. DINA was only one of six participating federal departments, and several studies were being planned in the mid-1980s, including some in the Delta/Beaufort region out of Inuvik on regional impacts (Inuvik Drum Sept. 13, 1984: 13; Nov. 14, 1984: 3). In 1988 NOGAP lost its funding, but it had done some planning and assessment work in regional communities (Tusaayaksat June 15, 1988:1)

2. Other Federal Agencies

Other federal departments, such as Health and Welfare, the Office of the Secretary of State, and the Department of Regional Industrial Expansion have sponsored social research in the North, but we will not deal with them here. The Science Council of Canada has had some interest in the general directions of northern research, as we will see, and has sponsored some research in technology assessment in the case study region (SCC 1977; Keith et. al. 1976).

The Social Science and Humanities Research Council (SSHRC) funds some northern investigations in the social sciences. T.H.B. Symons, in a review of northern science, found that only 2% of SSHRC projects from 1970 to 1980 were northern, however (ACUNS 1980: 38). Plans for a new Strategic Grants program in Native Studies, including northern Native people, were formulated by the agency in 1981, and meetings were held across the country in 1982 to plan priorities and ensure Native participation (SSHRC May 1982, Jan. 1983; Igalaaq July 1982: 18; Northline 2(3): 3). Despite strong support from Native and academic communities, this program has never been implemented.

Canada also became involved in 1972 in the United Nations Educational, Scientific, and Cultural Organization's (UNESCO) Man and Biosphere program (MAB).

The MAB program, begun in 1970, was an international attempt to create an integrated approach to solving environmental and natural resource problems (The Canadian Committee for MAB 1973: 2). Canada selected from 14 research themes to create four sub-program areas to work on, including Science for the North (no. 4). The theme for this latter sub-program is "how to achieve maximum benefits and avoid adverse effects of social and cultural patterns, environmental quality, and wildlife productivity, in the circumstances of technological change and industrial development in the Arctic, sub-Arctic, and other isolated regions" (ibid., p. 12). A Working Group of northerners and northern scientists was set up to recommend a framework for northern science to address these issues and to approach the conduct of science; the ethical principles derived by this group will be discussed in Chapter 8.

B. THE GOVERNMENT OF THE NORTHWEST TERRITORIES

The GNWT has not, in the past, taken an active role in northern science, but this is changing. Although the writer has information current only to the early 1980s, it is apparent that their role is increasing with the recent development of the Science Institute, which will be discussed in Chapter 7. The territorial government is seeking a more active partnership with the federal government in establishing local control of northern science.

The Science Advisory Board of the NWT was established by the Legislative Assembly in 1976 to advise them on applying "science, engineering, and technology to improve life and living conditions for the people of the Northwest Territories" (SAB 1976: 3). The Board had 9 members, appointed by the Commissioner of the NWT for their experiences, interests, and contributions to the North and to northern science; at least five of them must be NWT residents. In 1981 an Aklavik Native person was on the

Board, as well as many well-known scientists (SAB 1976; researcher p.c. Dec. 1b, 1981).

The formation of the SAB was spurred in part by the activities surrounding the Mackenzie Valley Pipeline Inquiry and the oil and gas explorations of the 1970s; the NWT was anxious to create a stronger role for itself in the regulation of non-renewable and renewable resources. Most of the studies have concerned renewable resources; there had not, by the early 1980s, been any social investigations sponsored. One of the priorities of the SAB was the encouragement of indigenous science, and to this end they offered financial support for northern residents to assist scientific field parties. They also have supported the idea of locally-supervised research institutes or resource centres; we will return to this and to the replacement of the SAB with the Science Institute of the NWT in Chapter 7 (Harrison 1983: 4; SAB 1976; p.c. Dec. 1b, 1981).

In 1979 the territories established a long-awaited museum, the Prince of Wales Northern Heritage Centre in Yellowknife. The Centre has permanent and travelling exhibits, sharing with the National Museum artifacts and specimens found on Crown land; it also has a library and archives, an advisory service for local museums, and education resources for curricula development for northern schools. In 1981 there was as yet little ethnological research; most social research was archaeological, and the staff had control of the issuance of permits for archaeological research in the territories (Janes 1982: 5; Information North Autumn 1981: 4-5; p.c. Dec. 10, 1982).

During the time of the Berger Inquiry, the GNWT began to sponsor its own impact assessment studies to increase its participation in controlling resource development. In 1980 they announced a new assessment and approvals policy, whereby developers must go through a review process resulting in, if appropriate, an approval and an Action Plan agreement ensuring northern benefits. Northern involvement in the assessment process is also provided for through the establishment of Development

Impact Zones and the concomitant Advisory Groups, such as the Beaufort/Mackenzie Delta Development Impact Zone Society mentioned in the preceding chapter. These societies, funded by industry and by both territorial and federal governments, serve as funnels for information flow between local residents, on one hand, and developers and government, on the other. This enhanced communication serves advisory, assessment, and monitoring functions. The Beaufort group, for example, informed local residents about the Beaufort Sea Environmental Review Panel and provided a critique of the Panel report (Oil Drum Winter/Spring 1984: 2). Although the groups help to facilitate local adjustment to development, there have been questions about their long-term effectiveness in being allowed to make decisions beyond those of an advisory capacity (Inuvik Drum Oct. 2, 1985: 3).

In late 1984 the GNWT Ministry of Culture announced a new allotment of funds for community-based cultural research projects, such as the recording of folklore and history. At that time a new GNWT cultural policy was being created, including the appointment of a cultural advisor, the creation of heritage councils, and funding for cultural research (Native Press Nov. 30, 1984: 4; Oct. 21, 1983: 3; News/North May 17, 1985: 1). In 1985 the Ministry of Culture was replaced by a new Ministry of Culture and Communications, retaining the mandate for cultural affairs and policy (Native Press Aug. 23, 1985: 1).

C. UNIVERSITIES AND RESEARCH INSTITUTES

Most northern research has, until recently, been affiliated in some way with the academic university system. Many proposals have originated in the theoretical and data foundations of academic disciplines; most of the conduct has been guided by academic professional rules; and most of the utilization of northern research has been in teaching and in academic publications. The university system is of course responsible for

training northern specialists, and it is through this function that the federal government has chosen to rest its primary influence. We have noted that in the 1950s some Canadian universities began to develop northern institutes and committees, both because of the research interests of the staff and students and as a channel for government funds. There are now many northern research committees and institutes, most of them university-based. We will not discuss each one separately, but will concentrate on the organization which has come to represent them for purposes of coordination and funding, the Association of Canadian Universities for Northern Studies (ACUNS). We will mention only one individual institute here, the Boreal Institute, because of its on-going regional interest in western Arctic and sub-arctic affairs. We will also discuss briefly the Arctic Institute of North America, AINA. Although AINA has always operated out of universities, it has received substantial government funding and has served a liaison function between university research and public and private sponsors.

The Boreal Institute has operated out of the University of Alberta at Edmonton since 1962. Like many other university-based institutes, it administers the training grants program for the university (see section I on DINA), and it offers some long-term research contracts. One of the first projects that it sponsored was the work done on education in Inuvik by the sociologist Charles Hobart, and Hobart has continued to maintain a link between the Delta and the Institute. The Boreal Institute has also undertaken educational programs, including the Arctic Summer School held for several summers in Inuvik to instruct managerial and professional people about the natural and human environments they would encounter upon working in the North (Northline 4 (1): 13-14).

The Arctic Institute of North America was begun in 1945 as a private institute serving both Canada and the U.S. in coordinating information on polar areas among

industrial, academic, and government users. It has served as a sponsor for large and small scale projects in the natural and the social sciences; and it is provided a library, a journal, and an Arctic bibliography/information system. Based at McGill University until the 1970s, it is now located at the University of Calgary.

Before universities began to establish their own research institutes, AINA was given core funding through the Northern Research and Coordination Centre of NANR as a way of providing training grants and funds to expeditions. Since AINA had ties with the U.S in the 1960s and the early 1970s, especially with industry and the military, the Canadian government came to prefer routing funding through the universities and ceased providing core funding in 1973. AINA now receives Canadian funding from sources such as the National Research Council, the Defense Research Board, and from business, private foundations, and industry (PAC, RG85, v. 1356, file 1010-46-1, 1950-51; Solandt 1961:3; Lloyd 1961: 617). It has maintained a cooperative relationship with Indian and Northern Affairs over the years, and some funding still comes through this source as well. 49

AINA sponsored more projects in the social sciences in the early years than it has recently; for example, it provided funding to R. Slobodin for his Kutchin studies in the Delta in 1946-48. In the period from 1946 to 1952, however, only 23 of 168 projects were archaeological or ethnological (PAC, RG85, v. 1356, file 1010-46-1, 1950-51, AINA). In 1970 AINA instituted a special project in the social sciences, the Man in the North, or MIN, project. It was conceived to be a three -year program dealing with the northern human environment; its objectives included the dissemination of knowledge about the North, the enhancement of public understanding about the Arctic, and the creation of Task Forces to identify and remedy social problems through applied research projects. The sponsors held a conference on community development in Inuvik in 1970, inviting both northerners and northern specialists. One of the outcomes of

this conference was the encouragement of local organizations to study their own needs through community workshops. Several Task Forces were organized in different areas and on different topics. In the Delta, a Task Force consisting of residents, researchers, and representatives of Native organizations was formed to direct three projects in solving local problems in education. Aklavik was, in 1971-72, the site of two experimental projects/studies on community-guided education and apprentice-teaching (MIN Progress Report 1971; MIN Technical Paper 1973).

The Association of Canadian Universities for Northern Studies (ACUNS) has its origins in the 1950s with the post-war establishment of university-based northern institutes. We have already noted how the federal government encouraged universities to perpetuate northern studies and expertise at that time. R.A.J. Phillips, later Director of the Northern Administration Branch of NANR, wrote a memo in February of 1955 calling for the establishment of a centre for northern studies in Canada to link and coordinate universities with government. There was a suggestion at that time that the centre be based in the NWT, an idea which has taken 30 years to develop fully, as we will see in Chapter 7 (PAC, RG85, v. 1356, file 1010-46-1, 1950-51). In 1967, steps were taken toward developing a coordinative body when the Institute for Northern Studies at the University of Saskatchewan approached DIAND for support of a conference to bring together representatives of all of these northern committees. The First National Northern Research Conference was held at the University of Saskatchewan in that year, followed by five more held around the country over the next eight years.

These conferences provided a regular forum for government-university interaction and the discussion of joint concerns. Participants urged DIAND to continue its grants program, especially as funds for universities and for social science research were being cut in the 1970s in favour of applied research within government and industry (Hamelin and Cailleux, eds. 1970). By 1976, there were 22 universities

represented at the conferences, and it was felt that a more permanent structure was needed. Planned in 1976-77, the first meeting of the new Association, ACUNS, was held in 1978.

The primary purpose of the Association is the advancement of northern studies, research, and training, particularly in and through the universities. Among the means which may be used are: fostering university relations with residents, organizations, and governments in northern Canada, with the Government of Canada and of the Provinces, with non-governmental organizations and industry, and with universities, scientific institutions, and polar and similar organizations at home and abroad. The purposes will be achieved by gathering, preserving, and distributing to the universities information on research and other activities, assisting in the advancement of research, the publication of journals and other information sources. Particular attention will be given to the research needs of northern residents and to discussion with Native Organizations of preferred methods for carrying on field research in the north (Sixth Canadian Universities Northern Science Training Conference, Second Session 1977: 40).

Core funding was obtained through DINA; additional monies have been gathered through member universities, foundations, business and industry, and other government departments (President's Report, ACUNS Annual Conference Proceedings 1981). By 1981, ACUNS had 32 member universities, with an Executive office in Ottawa, a newsletter (Northline), and several working committees in areas such as education, international relations, and relationships with northern peoples. It was making recommendations in regard to the administration of training grants, advising DINA in science policy, and it has sponsored special projects: conferences, a guide to federal funding sources, a guide to licensing for northern research, a list of northern specialists, an ethical statement for northern research, and other enterprises; some of these will be discussed further in later chapters (Lerchs 1979; Kubiski 1980; Northline 2(4) 1982: 1; Annual Proceedings 1981; Kubiski and Associates 1983; Executive Director's Report, ACUNS Annual Annual Conference Proceedings 1983).

In general, ACUNS has sought to advocate the value of independent, basic university research in the North within a policy context where priorities of government

and research have leaned toward applied and directed studies, as noted. To this end, there have been some conflicts with DINA, which provides core funding. The Northern Affairs program has expected more specific, program-oriented tasks from ACUNS than the broad, independent advisory role that ACUNS originally envisioned (Kubiski 1980: 9). Both sides have attempted to work this relationship out, and, as we shall see in the chapter on regulation and control, it is at the heart of the issues of research policy and evaluation.

In 1981 the Research and Field Facilities Committee recommended the institution, through ACUNS, of a Northern Research Council to overview research needs and priorities and to lobby for university-based expertise. The initiative came partly as a response to decreasing levels of funding within government, particularly for basic research (ACUNS Annual Conference Proceedings 1981: 92-7). Although this idea did not reach fruition as originally proposed, it led to another proposal to begin a Canadian Northern Studies Trust, which was funded by the Donner Foundation in 1982. The Trust has awarded senior fellowships, studentships, and special awards toward northern studies, as recommended by a Management Committee of representatives of both academic and non-academic communities. This has included, most recently, Special Awards for the education of northern residents, as well as awards to Native students researching economic development issues (see Chapter 7) (Northline 5(1) 1985: 2; ACUNS Canadian Northern Studies Trust Proposal Summary 1982).

D. NATIVE ORGANIZATIONS

The 1970s era of oil and gas development in the Western Arctic and Subarctic saw the beginnings of research by Native organizations such as COPE and the Dene Nation. These groups came to realize the need for studies to back their political and economic interests in a competitive arena. The Mackenzie Valley Pipeline Inquiry provided money

from the government to interest groups such as these to prepare statements and background information for the Inquiry. During the hearings held by Justice Berger, both COPE and the Dene Nation sent fieldworkers to the communities to talk to people about the hearings and to obtain data and reaction. In addition, anthropologists and other social researchers were hired to prepare briefs and to testify on behalf of the claims of the organizations; this process will be mentioned again in the next chapter.

At about the same time, DINA began to fund research into land claims and related historical issues; this has included some social research by and on behalf of the organizations. Since the mid-1970s, the involvement by Native organizations has rapidly increased, for several reasons. The centrality of land claims and the data needed to support them have stimulated an interest in these investigations. At the same time, Native organizations have been among the strongest critics of the conduct and utilization of much northern research, and they have sought involvement in revising research methods and context.

A compilation of research summaries published by DINA for 1978-80 lists nine (of 422) projects for the years 1977-78, for example, as being sponsored by Native organizations (see Rosenberg 1978, 1979, 1980), but as Symons (1980: 43) has pointed out, this figure may not reveal the full extent of land claims research. Moira White did a survey in 1981 of northern social research and its sponsorship, and found that Native organizations sponsored about 3.3% of listed projects (1981: 9). These have included studies across Canada on land use and resource harvesting. A few examples of these researches are mentioned here, but there are many others as well. In the 1970s the Inuit Tapirisat of Canada (ITC) undertook, with DINA, a comprehensive land use and occupancy study for all Inuit of the NWT (Freeman, ed., 1976). In the east, the Makivik Corporation of Quebec has established a research department which promotes collaboration between researchers and communities in areas such as wildlife

and renewable resources (Kemp and Brooks 1983: 1). Other organizations have also funded research in areas such as resource development, language, and education: the Inuit Cultural Institute in Eskimo Point, the Cree School Board of Quebec, and the Tungavik Federation of Nunavut (Upton 1983: 1; Northline 5(1) 1985: 1,4).

The Dene and the Inuvialuit in the west have been active in social research since the time of Berger, also. Fieldworkers are still active in the organizational structures of both organizations; at the time of fieldwork COPE employed several workers in the Delta as communications links and as researchers. Research has also been done in areas like health, social services, languages, and education. This information, like the land use data, is useful in providing a check on data gathered by industry or government-sponsored investigators, and in supporting the planning of self-government and cultural preservation. COPE, for example, has sponsored health studies, and has been involved in broad-ranging linguistic research on Inuvialuit dialects and curriculum development (Tusaayaksat July 1986: 1).

Several neighbouring Dene communities, like Fort Good Hope, are engaged in similar projects to study their own language and history (see Native Press Feb. 24, 1984: 12; Nov. 30, 1984). The Dene Nation itself has become increasingly involved in land use and development impact assessment studies, partly in response to the problems they have found in studies done by outsiders. The organization has a Mapping and Research project, responsible for land use studies, and which has operated in cooperation with the Anthropology Department of the University of Alberta (Dene Nation Annual Report 1983: 17-8).

Similarly, the Dene Nation has recently begun its own Norman Wells monitoring program to ensure that the Native perspective is considered, and, again, to supercede inadequacies in a federally-sponsored monitoring project. Northerners were to be trained in monitoring development impacts, providing a core of trained local

researchers. At the beginning of the Norman Wells construction, monies were set aside by government and industry for employment training and for mitigating impacts. Some of these funds were allocated to monitoring. For example, a joint committee of the Dene, the Métis Association, and the GNWT investigated continuing employment and training needs. The project contracted interviewers through local Native groups, and, although there were problems with the management of the project, it did make some recommendations on employment issues (researcher p.c. June 26, 1986; Dene Nation Annual Report 1983: 33-36).

The Dene also proposed a general community monitoring project, which was joined by the University of British Columbia School of Community and Regional Planning in 1984-85. This project, the Dene Gondie, also features Dene researchers, and will be further discussed in Chapter 7. The Dene have suggested that their monitoring will be eventually extended to cover Beaufort Sea activities, and, with the settlement of Dene land claims, will ultimately lead to the establishment of a Dene regulatory agency (Native Press April 6, 1984: 9; Inuvik Drum March 15, 1984: 15).

In the next chapter, we will continue the case study with a closer examination of the methods of social investigation in the Delta and in the North, and we will begin to see how the above agencies have operated with regard to conduct, regulation, and utilization of research.

CHAPTER FIVE: CONDUCT OF SOCIAL RESEARCH IN THE DELTA

In this chapter, we will identify major studies done in the Delta, describe the methods used, and set them in the context of major policies followed by the sponsors of the research. We will use the rules of participant-observation identified and discussed in the preceding chapters as a means of comparing the types of research conducted. It is important to realize here that not all of the research was anthropological, and not all of the researchers intended to do P-O. We find other methods and techniques employed, such as formal and informal interviews, questionnaires, and even telephone consultations. Yet, by using P-O in the broad sense that we have effected throughout, we can utilize the rules as a model for examining and comparing methods, and we can use our framework for explicating the process and quality of "knowing" another culture.

In the next chapter, we will describe the responses to methods by the northern hosts who have interacted with the fieldworkers, followed by a discussion of regulation and utilization of research in Chapter 7. Data for these chapters come, as we have already noted, from a variety of sources: published and unpublished government documents, federal archival sources, published field accounts, interviews with Delta residents, interviews with fieldworkers and sponsors, and information observation and conversation. Before the conduct of research is detailed by time period, some of the details of field research methods will be noted here.

A. METHODOLOGY

In 1979 I undertook a short exploratory field trip to Inuvik; at that time my interests lay in uncovering the contemporary roles of women in the North. The visit was made in the summer, and it coincided with visits by several other anthropologists

and archaeologists. I became aware of the different approaches employed by the researchers, and of the responses they obtained. In addition, from listening to comments made by Delta residents, Native and non-Native, I observed that there were sets of images of "anthropologists" in the region which seemed to derive from features of their participatory approach: that they were transient, that they were nosy, that they exploited the people, and that they were also often friends. Upon return from the field I did further library investigation of the history of research in Inuvik and in the Delta, and I realized that there was a long and intensive history of social research there, partially encouraged by the presence of the Research Lab in Inuvik.

Over the following two years of graduate school I gradually formulated a proposal to return to the Delta to approach residents about their responses to the conduct of social research there. At that time, there was high interest within anthropology in re-examining the methods and assumptions of participant-observation, but few had pursued the topic from the hosts' perspectives. I felt that further insight into the method was contingent upon exploring the nature of the total interaction between guests and hosts; most information on interaction had come only through the filtered descriptions of methodology contained in ethnographies and field accounts.

In 1980 I wrote to the town of Inuvik requesting permission to do two months of fieldwork in the town; and, upon receiving permission, I obtained a scientific research license from the Government of the Northwest Territories, as required by their regulations. Funding was acquired from the Presidential Committee on Northern Studies, McMaster University. The proposal submitted to the town called for formal and informal interviews with residents concerning their response to methods undertaken in past years. I wanted to speak with both Native and non-Native residents, and both to those who had had extensive experience with researchers and to those who had not. Since I could not envision the exact level or type of response I would encounter with a

somewhat "unconventional" field topic, I was unable to set up strict criteria for sampling. Instead, I wanted to wait until I had spent some time in the community before finally "shaping" my own methodology. In addition, I wanted to be cautious about the image my own work projected. I did not want to pry into the affairs of my research predecessors; I did not want to be thought of as personally "checking up" on their methods and conduct. Instead, my work was presented as an attempt to allow northern residents a voice in critiquing methods in general and in shaping future interactions with researchers.

The first active phase of fieldwork was in January and February of 1981. This was followed by a return to Ontario, where I did preliminary analysis of results, and a second field phase in September, October, and November of 1981. Again I visited Inuvik, but I also spent five weeks in Aklavik. While in Inuvik, I resided at the Research Laboratory and was able to maintain an office there; few researchers were present in the winter months and my sojourn at that time not only opened facilities but gave my visit a unique image. As has been true for many previous researchers, however, I found that my visit was shaped by my association with the Lab. Some residents assumed that I was working for the Lab, and I had no ready avenues with which to build social ties such as I might have had if I had lived with a family. Open housing in the community was nonetheless scarce and expensive, and I had no prior contacts with whom to arrange another situation.

In Aklavik, I arranged through an Inuvik contact to stay with the Anglican minister and his wife. They had a large house and frequently received visitors; I paid them a fee for room and board. Through them and through their contacts in the church and school I was able to meet many residents in a more informal setting than was possible in Inuvik, although certainly my association with them also affected response to me and to my efforts, since I was present at many church-related activities and could

not easily attend functions sponsored by the other churches. As I did most of the visiting on my own, however, I do not believe that this greatly affected the nature of data obtained.

At the beginning of both phases of fieldwork I arranged to have an introductory message printed in the local newspaper in which I explained my purposes and expectations, and in which I invited response. In addition, I was able to obtain some radio time to the same end; these messages did not reach all members of the community but they often served to "introduce" me and my work to later respondents. Another initial step was to locate officials in the two communities whom I identified as key people in making introductions. Arrangements were made by telephone and in person to speak with some of these people, and I was soon able to begin to visit them. I drew up a list of questions which I used as the basis of conversations and interviews; these concerned the degree and type of prior experience with researchers, the respondent's ascertainment of methods used and response to methods, any images or expectations which were held about participant-observation, and any circumstances which affected either method or response. A few questions were devoted to the host's personal background, and I asked about any prior experience they had had working with or being consulted by social researchers. I asked them what they knew about feedback and utilization of research, and I asked them for suggestions as to what could be improved. Some questions related to general community context, such as the involvement of government in the community and the relationship of government to research.

Interviews varied in length and formality, according to the situation and the respondent. They would often begin with informal conversation about the host and about myself and my purposes. Some people replied directly to the questions asked, whereas others shaped the interview through their own initiatives. Interviews were usually held in the hosts' home or workplace. Where appropriate, I took along a gift of baked goods as

a token of appreciation for time spent. A few individuals and families who were extremely helpful and/or sociable were visited more than once.

I located potential respondents in both communities through the "network" system, asking each person whom they thought I should speak to. There was a high level of agreement concerning some "informants" who were widely held to be useful: often individuals with central positions in the community or who had known researchers well. From the list I compiled through networking, I selected contacts on the basis of their experiences and backgrounds. Of the approximately 65 individuals interviewed in the total time of Delta fieldwork (not counting occasional informal conversations), half were Native and half were non-Native. The sample included government officials who were sponsors or utilizers of research, individuals who had worked as researchers or research assistants, people who had not met any researchers but had heard about them, and both long and short-term residents. There was no attempt made to obtain a representative sample; rather, I sought a range and balance of perspectives as I proceeded. The sample was also affected by cooperation. Although very few individuals refused directly to speak with me, in fact I was unable to locate or successfully contact some recommended hosts.

As the interviews progressed, some questions on my list were changed where necessary, and others were added; methods and questions were adapted to the type and nature of response received as fieldwork continued and as I was able to do preliminary analysis of data. During conversations, I would take a few notes on what was being said, but most recording was done after the interview when I typed notes on responses. This allowed me to pay full attention to what the responding person was saying, and it facilitated the social "rapport" of the conversation. As noted above, I was careful not to inquire about the behaviour of particular individuals who had visited the Delta, although sometimes a mention of the names helped to "jog" the memories of hosts. Although some

individuals offered this information, most conversations were more general than "gossipy" in quality. Respondents often said little directly about the subject, but discussed visitors to the Delta and related subjects in more general and indirect terms. Part of the reason for this was undoubtedly caution on both sides, part was failed memory, and part was apathy toward the subject. I did not predict the degree to which the issue of methods and images of researchers would engender an apathetic, uninterested response. Some individuals stated that this was because of overload; they had simply been interviewed by researchers and journalists too many times. Some did not understand well what I was doing, and others were simply uninterested.

Responses to my efforts varied. Most people were openly cooperative, even when they did not understand or approve of what I was doing; I continue to be grateful for this response. On a few occasions I was accosted with the anthropological images I had come to investigate: for example, that I was nosy and had no business being there. This was done by both Native and non-Native people. In only a couple of instances was this accompanied by open hostility, however. Some found the enterprise amusing; I had come to "study the studiers" and this was, to them, the ultimate comment on the absurdity of social research. Non-Native people tended to assert that I was "really" studying the community, particularly its Native residents, and they sometimes expected "expert opinion" about the town's structure and secrets. Some Native people also stated that they believed that I was really studying them, as most prior researchers had done. Many also wanted to know who else I had spoken to and what they had said, but I maintained a policy of extreme caution about revealing sources. This may have inhibited some host response, but I felt that it was necessary for ethical reasons.

I also chose to maintain an open role as researcher/anthropologist. Some people suggested that I would obtain a more honest response if I pretended to be something else, such as a teacher or civil servant. It is likely that people might have been more open

with their comments -- and particularly their criticisms -- if I had disguised my purpose, but I decided not to do so for two basic reasons. First, by assessing their response to me I could obtain a better idea of how they reacted to other anthropologists. Second, one of the primary responses by informants was that they had felt deceived by researchers who had not revealed "true" purposes. I felt that I would only perpetuate this problem by taking another role. Similarly, I tried to be honest about what I could achieve with the study. Although I was able to carry out special requests from some people, in general I told them that I could only publicize my conclusions but that I could not guarantee any consequences of that in terms of government or research policy.

As a corroboration for my interviews and researcher role, I tried to attend many community events. In Inuvik, I helped with several activities at the Friendship Centre, attended bingos, teas, and community meetings, and joined a ladies dart league. In Aklavik, I attended church functions, hamlet council meetings, and went ice fishing, trapping, and skidooing. Although occasionally I visited bars and hotels with friends, I tried to avoid an association of drinking and fieldwork. I explored local libraries, and was given access to past minutes from the Aklavik Hamlet Council meetings. All of these activities allowed me to be more visible and to interact with more people, but they also allowed me to obtain a broader framework of observations within which to contextualize the interview responses. I learned about the social setting of both communities, although not completely, and this helped me to understand how response to researchers had been shaped by contextual factors such as social rules, government functions, local administration and leadership, factionalism, and cultural diversity.

At the end of fieldwork I thanked hosts through the newspaper and radio. In the summer of 1982 I wrote a field report which was circulated back to both communities through key individuals and institutions. Although response was not great, the report

did result in a last radio interview. Copies of the report also went to some Ottawa offices, such as DINA and the Canadian Arctic Resources Committee.

As the northern fieldwork progressed, I realized a need to know as much as possible about the wider context of research sponsorship, regulation, and use. For example, I needed to know more about "government" -- who sent in consultants and arranged hearings; what was the information used for? I needed to know what methods researchers had used in the past, and about any special problems they had. Therefore my research continued in 1982. The Public Archives in Ottawa allowed me access to records of the Berger Inquiry, to records of the moving of Aklavik and the building of Inuvik, and to the early records of agencies such as AINA and the Research Division within DIAND; data pertaining to the latter extended only to the end of the 1960s, however. In Yellowknife, Edmonton, Vancouver, Saskatoon, Calgary, and Ottawa I visited libraries where relevant information might be found (such as the Boreal Institute library, the DINA library, the GNWT library, and the AINA library) and conducted interviews. These interviews, approximately 30 in number, were with researchers who had worked in the Delta and elsewhere in the north, and with government officials who had had some key involvement with research sponsorship in the North through the Research Division and other divisions sponsoring research within DINA, Native organizations, the Science Advisory Board, the Science Institute, the National Museum, the Canadian Arctic Resources Committee, and the Man and Biosphere program. Again, interviews varied in length and substance. Most were done in person, but two were by telephone and one by letter. Questions concerned methods, feedback, response, and utilization, but they varied according to the experiences and expertise of the person interviewed. Through these sources, I was able to get a broader historical perspective on research conduct and regulation for the Delta and for the Canadian North as a whole.

B. RULES OF CONDUCT

The following discussion of studies and methods is divided by time period: 1900-1950, 1950-59, 1960-75, and from 1976 to the early 1980s. The third period is discussed at greater length, as it is the florescent period of social research in the Delta. These periods were selected because the types of research conducted in each of them show certain similarities, through sponsorship and/or academic disposition. It must be recognized that methods varied according to the motives of both sponsors and participants. The federal government, for example, often set some restrictions on the mode of interactions for non-Native visitors to northern communities. Researchers usually had their own methodological objectives, often shaped by academic rules. Industry has tended to encourage certain kinds of survey and consultative methods as well. Northern hosts have used their own perspectives to define, explain, and mold the methods encountered among researchers. Where possible, these varying perspectives/objectives will be described and distinguished. Some statements taken from field notes of interviews are paraphrased here.

I. Pre-1950

The early anthropological researches of this period were sponsored primarily through universities and museums, in Canada and in the U.S., sometimes in cooperation with government and its objectives (see Chapter 1). The Canadian government did not take a coordinated role in northern researches for administrative purposes at this time, although occasionally a department did send a civil servant north. Like the Americans, the Danes, and the Norwegians, the Canadians were concerned with the discovery and mapping of new lands and waterways, and with documenting aspects of the natural, physical, and human features of the North. Scientists were sometimes sent with exploration parties, as we noted in relation to 19th century exploration, to do this

documentation, often at the behest of museums or scientific societies. And as other countries continued to explore the North, Canada responded to sovereignty threats by various actions, including research sponsorship (Phillips 1967; Solandt 1961: 1-2; p.c. July 16, 1982). The government sometimes sent scientists on the northern patrol ships which it began to send into the Arctic early in this century to enforce sovereignty (Diubaldo 1978: 6-8; Dorion-Robitaille 1978: 14).

Some of these early scientists were forced by circumstance, including the difficulty and expense of exploratory travel, to stay for extended periods, whereas others, often civil servants, made only brief summer visits by ship. Anthropologists began to enter the North both as members of expeditions and as solitary researchers intent upon learning the ethnography of northern peoples. These modes of interaction echoed the diversity of approaches to Native people we have noted in previous chapters, although in both cases roles and objectives were still ambiguous in definition. The scientists were interested not just in learning about their hosts in order to convert them to a new way of life, but in using that knowledge in academic pursuits entirely removed from the lives of northern peoples.

The first research in the Delta and adjacent coast to be produced for explicit ethnological purposes was done by the scientist/explorer Vilhjamur Stefansson over a period from 1906 to 1918. Stefansson had some anthropological training from Harvard and an explorer's yen for the exotic. His trips were sponsored by both the U.S. and Canadian governments, the American Geographical Society, the Peabody Museum, the American Museum of Natural History, the Royal Ontario Museum, and the National Museum of Canada (Duibaldo 1978: 16-18, 36). He made three major expeditions to the western Arctic, including the famous and controversial Canadian Arctic expedition of 1913-18. His interests were primarily in the Alaskan and Copper Inuit, and he spent more time with these people than with the Mackenzie Inuit, whom he visited primarily

during his first visit in 1906-07 (although his published information about them comes from his second expedition of 1908-12).

Stefansson was to leave a lasting impression on Delta people. Like Malinowski, he was forced by the circumstances of his first expedition to live with the people, finding both survival and solace in their company. His anthropological background and interests convinced him of the value of this participation in learning language and custom, and he believed himself one of the initiators of a participant-observation type of study. His account (1913) of his first expedition in 1906-07 to the Delta contains several statements of his methods and intent. He was forced to winter with the Inuit when the ship which was to meet him in the Beaufort Sea failed to make the rendezvous.

From the point of view of the ethnologist, this was a very fortunate circumstance. Although I had always doubted that the ship would come to pick me up, I had nevertheless entrusted my entire outfit to her, for I wanted, if I lived with the Eskimo at all, to live exactly as one of them, in their houses, dressing like them, and eating only such food as they did. . . These were ideal conditions for me. Had I had my own party and my own house I should have lived near the Eskimo instead of with them. I should have seen them as an outsider, a stranger. If I had visited them now and then, I should have found them wearing their company manners and should have obtained no better insight into their lives than does the ordinary missionary or trader. Now my very poverty was my greatest advantage; I was not rich and powerful like the whaling captains or mounted policemen, so there was no reason why they should flatter me or show me deference. I had no visible means, and therefore what they did for me was without hope of reward. They took me into their houses and treated me hospitably and courteously, but exactly as if I were one of them. They gave me clothes to wear and food to eat, I helped them in their work and joined in their games, until they gradually forgot that I was not one of them, and began to live their lives before my eyes as if I were not there. This gave me a rare opportunity to know them as they are (1913: 9-10).

Thus Stefansson sought a greater degree of involvement in the 'natural context' of Inuit lives than had the missionary and the trader. The role he wanted to play was one of kinsman; to the degree that this was impossible to completely effect, he tried to establish new roles as non-Native companion/hunter and, in essence, as researcher. Stefansson did not in fact spend all of his time during this trip with the Mackenzie

Inuit. His first winter was spent with the non-Natives resident at Point Barrow, Alaska; he later spent some time with the Inuit of the Dolphin and Union Straits east of the Delta. He learned the language in order to communicate and to study linguistics and folklore. He participated in their hunting activities and in reciprocal transaction of daily life surrounding food and companionship.

Stefansson tried therefore to develop the rapport necessary to become one of the people and to learn the culture from the inside out. Since some of the people he visited to the east of the Delta were as yet unfamiliar with the ways of the "white man," they in fact could not figure out why he spoke the language if he were not essentially one of them (1913: 179). Thus he was a pioneer not only in the participant-observation model of research, but in establishing the role of the intrusive researcher or student of custom.

Stefansson is remembered in the Delta in the 1980s, but less because of his researches than because of the particulars of his role. One of the key relationships which he established was to take an Inuit wife, and there are descendents of his alive today. Apparently he was reluctant to acknowledge these relatives in the North. His granddaughter, who lived in Inuvik in 1981, was interested in knowing more about him, but he had never responded to the letters she wrote to him before he died. Although she was proud to be related to Stefansson, she expressed concern that, if he had lied about this aspect of his life in the North, he may in fact not be a credible source in some of his other documentation of northern lives (p.c. March 5, 1981).

Another research pioneer was Dr. Diamond Jenness, who, as a member of Stefansson's Canadian Arctic Expedition, did scientific fieldwork with both Alaskan and Copper Inuit neighbours of the Delta people; he is best known for his work among the latter group (Taylor and Collins 1970). He spent his first winter with an Inuit family near Point Barrow after Stefansson gave the family two hides to look after him and teach him the language (Jenness 1957: 131). Jenness then went east and spent two years

living and travelling with the Copper Inuit around the Coppermine River (Jenness 1922, 1959). He, too, had anthropological training, and was brought on the expedition as an ethnologist. His purpose was to collect specimens, learn the language and way of life, and record custom and folklore.

Although Jenness did not preach the ideology of participation to the extent that Stefansson did, he recognized the importance of living closely with the people to learn how they went about their seasonal round of activities. He was adopted as a son into a Copper Inuit family, and was taught as a son to hunt and fish (1959: 90). He spent some of his time with some of the other expedition scientists at a camp at Bernard Harbour, but also spent as much as seven months at a time completely in the company of the Inuit. He was tested by his hosts in his competence in social and hunting behaviour, joining, as did Stefansson, in the transactions of camp life (*ibid.*, pp. 101, 127-28). Jenness's involvement extended even to his being given an unexpected role, that of sorcerer-murderer, a charge which a shaman later disproved (*ibid.*, p. 85). In sum, his work, which was very ethnographically productive, is another early example of the cultivation of the role of the participant-researcher as one who lives within the same human and physical environment as his hosts.

The dwindling Delta/Beaufort Inuit population, by then reconstituted in part by Alaskan Inuit, was studied again in 1924 by Knud Rasmussen of the Danish Fifth Thule Expedition (Ostermann 1942). His visit, lasting through only a few months of travel, was less intensive than Stefansson's had been, but produced nonetheless some scattered ethnographic information edited later by H. Ostermann. Rasmussen's interests here were primarily in folklore and folklife.

The Kutchin began to receive anthropological attention in the 1930s, slightly later. The anthropologist Cornelius Osgood did most of his work in the summer of 1932 with Kutchin west of the Delta, but he had two Peel River Kutchin informants who

provided information on their people in the Mackenzie region (Osgood 1936: 3). He had previously, in 1928, spent 18 months among a group of Athapaskans southeast of the Delta at Great Bear Lake; that work was for his doctorate in anthropology. Osgood (1953) wrote an account of this early fieldwork, detailing his personal, emotional involvement with the camp of Indians and non-Natives where he lived. He, too, sought to live as one of the people in order to learn about them. While he learned to hunt, fish, and drive a dog team, he had more difficulty getting the Indians to reveal attitude and value, and thus relied on the two other non-Native men in camp as both informants and as emotional supports. He learned the language, and he began to learn the social rules of survival, as did Stefansson and Jenness, and he sought rapport with the Indians who taught him. His roles included those of student, researcher, and, as with Jenness, scapegoat; he, too, was tested for the extent of his knowledge and rapport.

The Delta Peel River Kutchin were visited by Richard Slobodin over a total of 18 months in 1938-39, and 1946-47; he has returned several times since (see Slobodin 1960, 1962, 1966). He was the first ethnographer to live among them; he lived in the community of Fort McPherson, visiting people in their homes and participating in some of their activities, such as camping and hunting. Like Osgood, he used participant-observation, and the attainment of rapport with the people, as an anthropological method for learning the details of culture. This meant that he needed to spend long periods of time with the Kutchin, and he, too, left an impression on the local people as a model of a participating, knowledgeable social scientist which was still extant in 1981 (p.c. Oct. 13, 1981; Nov. 12, 1981; Feb. 4, 1981). Another social scientist, Douglas Leechman (1954: 1) did some ethnology over a five-week period in the Yukon Kutchin community of Old Crow in 1946. While his primary purpose was to do archaeology, he used two principal informants to learn about culture history. R.

McKenna, similarly, spent a summer at another Yukon Kutchin village, Ft. Yukon, in 1933 (McKenna 1965).

Finally, we have noted that the federal government began to send survey workers to the North even before mid-century, although it was not common practice. The Department of the Interior sent J.F. Moran (1923) north in 1923 to study local conditions in the Mackenzie District, and Moore (1945) did a report for the Social Science Research Council on education in the district. Neither provided many details of method, as is true, we will see, for later civil servants doing surveys. We do know that they used "key informants," often non-Natives in the North recommended by the sponsoring department. Moore (1945:61) was given a list of these people by the Department of Mines and Resources, for example. His survey took only two months in the summer of 1944, and he visited twelve communities; thus he spent little time in each one.

Two styles of field research were established in these early years: the brief visit consulting principal informants, with less emphasis on social science than on meeting information requirements, and the longer-term, exploratory visit with the purpose of experiencing the environment as the hosts do, in order to extract knowledge from it. This latter kind of exploratory, classificatory research fit with the development of anthropological P-O elsewhere in the world at that time. These styles continue, alternating in importance according to the prevailing policy/academic objectives of the succeeding periods.

2. 1950-1959

Official Canadian interest in northern science did not really begin until after the Second World War, when federal involvements in the North began to grow. Stimulated by the war, national interest in defense rose sharply; and, as we have seen, there was

new national concern for the welfare of the Native northerners being affected by the fur trade, the missions, and new intrusive wage work developments such as the installations of DEW line stations. There was recognition of the need to learn more about the North and its inhabitants in order to solve the practical problems of living there. Meteorological stations, used as general science stations, were established by both Canadian and U.S. governments; the Defense Research Board built a northern laboratory at Fort Churchill, and other large projects such as the Polar Continental Shelf Project, began around this time as well (Rowley 1963: 7; van Steenburgh and Giroux 1966: 53-4). All of these northern projects were supposedly coordinated through the ACND Secretariat and the NCRC of the Department of Northern Affairs and Natural Resources. Research by government and industry into mineral resource potentials also began in the 1950s in relation to public and private investments in the North (Reed 1962; Lloyd 1961: 607).

Most of these large government undertakings were not oriented primarily to social science. Social research in the 1950s included the ethnological/ethnographic investigations of universities and museums, and the welfare and development-oriented research of government. We have already noted how the new NCRC took upon itself the task of sponsoring social research because this was inadequately covered by other agencies; recommendations for needed projects came from other government departments and agencies on welfare, housing, health, economic development, and education issues. The bureaucracy believed itself to be rescuing Native people from an outdated, static, and conservative existence, and to be guiding them carefully into a modern age. Where feasible, old ways would be maintained, or else the people would be relocated or given wage jobs. Research, to the extent that it was used, could provide necessary information for the cautious planning of the assimilative welfare state.

Some of the personnel to do these studies were sought from the universities, beginning a relationship between government and academe which was maintained through contracts with individual researchers and institutes. The universities wanted funding, and they wanted their studies to be relevant to Native people and to government policy alike, but they wanted independence in the planning, conduct, and reporting of research results. The government, on the other hand, sought a relationship with academics which would allow them to use trained personnel and to ask for specific advice; they also desired more control over reporting and results than the academic community thought necessary (for scholarly and ethical reasons) (PAC, RG85, v. 1356, file 1010-46-1, 1950-51). We will return to this theme throughout the next chapters.

Anthropologists had by this time established disciplinary training and methods for the field; fieldwork had become a necessary part of practice in Canada and the U.S. Participant-observation was used by investigators of the phenomena of cultural change, as well by those recording the disappearing features of traditional Indian or Inuit life. Those who worked in northern Canada often received their sponsorship through the NCRC, or the National Museum, or from AINA. They combined their own research interests with the objectives, often broadly defined, of sponsors.

A few projects sponsored in the Delta were stimulated by the policies of the decade, supplemented by academic interests; many more were sponsored elsewhere in the North. The Aklavik Journal of May 1956 reported a "wave" of scientists in Aklavik but did not name them; the writer was unable to identify many projects conducted at this time but some were associated with the move to the new town of Inuvik. As in earlier years, the government sent both trained scientists and untrained civil servants for summer surveys and community studies. This is the era in which the roles of the social researchers and the government expert (who may or may not have been the same person) were established for northern Native people, and often associated together.

This happened during the Aklavik move, as we have suggested (researcher p.c. May 11b, 1982).

At the time of the move, two projects were undertaken in Aklavik to determine the effects of the move and to suggest economic alternatives. W. Black (1959) did an economic study of Aklavik and Inuvik for the government civil service in the summer of 1959. He does not list his methods, but it appears that he used key informants and local records and statistics. Two other researchers were in Aklavik at the same time, in 1959, to discover why the people were resisting the move to suggest ways to ease the process. Walter and Jean Boek, who obtained directives and funding from the NCRC, noted the duplication of efforts between themselves and Mr. Black, from whom they received little cooperation.

The methods of the Boeks, a husband and wife with a small child, were similar to those used by anthropologists and sociologists doing community studies elsewhere in the North at this time. They used some participant-observation, joining in local activities and observing the flow of life. They interviewed some individuals, using more formal interview settings for their non-Native correspondents. Through the friendships established by themselves and their daughter, they were able to secure some rapport with the Native people over the short span of the summer, they reported. The Boeks also used whatever local records, such as RCMP statistics, as could be made available to them (PAC, RG85, v. 1656, file NR2/3-24, pt. 1; Boek and Boek 1960). The Boeks were critical of government planning, and their report was not published, but they forecasted many of the problems which later arose in the area and other researchers have benefitted from their analysis.

Most of the research at this time was limited to the summer months, but efforts were undertaken by those trained in the sciences to obtain cross-check data from alternate sources, and to achieve some rapport. Anthropologists working in small

communities elsewhere in the Arctic and Subarctic used similar methods, establishing a model of summer P-O -- a modification of long-term research structured by university schedules, sponsorship, and the variables of climate. They went out hunting with the people, socialized and feasted with them, and learned the rules of social interaction. They interviewed both Native and non-Native people, used government statistics, observed activities, and asked questions. They often tried to spend some time with Native people out on the land, both to build rapport and to learn about camp activities (Willmott 1959: 13-14; Balikci 1958; Van Steenhaven 1956: 7-8; Desgoffe 1955; Arima 1976: 48). Some avoided contact with non-Natives, but most assumed a role firmly within the range of those played by non-Natives in the community. For example, some were, or posed as, teachers (Helm 1979; researcher p.c. Feb. 17, 1983). W. E. Willmott described his summer fieldwork of 1958 at Port Harrison, Quebec:

For the first ten days I lived in the Northern Affairs 512 house, a standard Eskimo dwelling, becoming acquainted with the residents and collecting vital statistics in the R.C.M. Police detachment where Cst. and Mrs. Ross Gibson, now of Pictou, N.S., were warmly hospitable and helpful. I also spent some time in the Hudson's Bay Company store, whose manager, Mr. Ross Peyton, was a constant source of information and encouragements, and collected health statistics and observations from Nurse Ruth Horley, then in charge of the nursing station. Throughout the summer, I was grateful for occasional meals and frequent conversations with many of the white residents. After ten days of my own cooking I moved into the tent of an Eskimo family, Lucassie and Eva Naujakulluk, in order to observe more closely the daily life among those Eskimos who live in the settlement itself. For two weeks I made myself a terrible nuisance following them around, asking impossible questions whose answers I had difficulty understanding, and being constantly in the way: a real qualluna'aluk! (Willmott 1959: 13).

J. Ferguson, a sociologist, visited Tuktoyaktuk in 1956 and 1957 for NCRC, investigating the effects of the DEW line station on the community. He did some observation, but relied heavily on key informants. When he worked with E. Arima in Coppermine in 1958, he also used questionnaires, a method most anthropologists have

since rejected in the North as being too impersonal for effective communication with Native people (Arima 1976: 48; Ferguson 1961: 50).

While civil servants were doing short-term surveys, and anthropologists were developing a form of limited P-O (although there was overlap between these categories on the individual level), other observers in the Delta were achieving some participation also. Ethel Stewart (1955) was a teacher in Fort McPherson for four years while she gathered data on the history and social economy of the area for a Master's thesis. She does not mention her methods in her thesis otherwise, but undoubtedly her role gave her access to valuable information through the interactions associated with it. Other investigators unself-consciously identified themselves as researchers or as students, thereby helping to familiarize hosts with this group of people who come from the south for short periods of time and ask questions. Korner (1959: 96), who did some psychological research among the Athapaskans of the Mackenzie Valley, reported that the Indians were surprised when he tried to go to their camps and adapt to their way of life, rather than the opposite. Korner also provides an early account of a researcher's awareness of the importance of his own attitudes and personality as instruments of investigation; he argued that a scientist should know himself and his biases before imposing his interpretations on his hosts. Thus he became aware of the rules of immersion and of using oneself as an instrument:

When meeting the Indians as an experimenter, my reactions ran the gamut from exhilaration and animosity to exasperation and inertia. They let me wait hours and days without any rhyme or reason, or so it appeared to me. They were unwilling to part with information, they were often suspicious. At all times I was the one who had to adjust to them, not one of my subjects adjusted to me. All this appears to be a necessary condition of successful fieldwork. There were times when things went smoothly, my work progressed, my curiosity knew no bounds. My notations at the top of my data sheet indicate my feelings at the time of the experiment. It easily can be seen that my data, in some instances, were richer, more exhaustive, my subjects more informative than at other times.... It helped, in the evaluation of data, to know the conditions under which they were obtained (Korner 1959: 95-6).

Some anthropologists apparently also did physical anthropology and archaeology, as part of their general ethnological studies at this time, although no specific projects were recalled by informants. Two people in the Delta in 1981 mentioned that the practices of "measuring heads," etc., were offensive to Native people and helped to establish a negative image of the intrusiveness of the social researcher. Dyson (1979: 228) mentions a legend heard elsewhere in the North about an anthropologist distributing nails in exchange for measurements. Evidently both manifest and latent functions of these measurements were unclear to the people, and aroused some suspicion.

Thus most researchers stayed in the communities for short periods, seeking only brief involvements in Native activities, and relying more on observation, interview, and measurement. They usually had both academic and utilitarian objectives which guided data collection, structuring the interaction through specific roles, as was advocated in the P-O literature at that time. There was not much concern that hosts actively participate in the research, and host responses to methods were not taken seriously except in the immediate present of interaction. Although we will reserve discussion of government regulation of research for the next two chapters, it is important to mention that the government was imposing its own rules of conduct upon those researchers by this time, limiting the amount of time spent and the number of "nosy" inquiries about government programs. In addition, researchers were discouraged from activities such as hunting with the Inuit, and were even denied access to certain groups of people in order to reduce outside interference with Native life. These had an effect on the conduct of participation, although it appears that anthropologists frequently tried to circumvent them in order to remain true to method.

3. 1960-1975

A. *The Policy Background*

In the 1960s the bureaucracy of the North grew, as we have shown, and social research began to build toward its peak of activity in the early 1970s. Some of the same policy interests continued, and there were changes of emphasis. There were liberal interests within the federal government in community development, and conservative predilections for rapid development of non-renewable resources also. Both of these factions viewed assimilation of Native people as inevitable, but they differed in their recommended methods of achieving this. The liberal group wanted to see Native people take a more active stance in guiding their own economic and social development, through cooperatives, involvement in local government, and education. They advocated using social research as a tool for rational planning and northern participation, especially in the form of basic, long-term humanistic studies. They tended to be supported in their objectives by the university community of social scientists, including those associated with AINA. There was a boom in the number of students, in Canada and in the U.S., in this period, as well as an increase in available research funds. This, too, stimulated northern research.

The conservative group, which began to dominate Northern Affairs by the early 1970s, saw quick development of the North's minerals as a solution to both northern and southern problems. They advocated fast, practical problem-solving research to create a North into which southerners could move without noticing the difference (AINA 1965; Warner 1975: 46-7; Sivertz 1965: 261-62). They soon became supported by industry in their efforts to use science to expedite development, and social sciences were subordinated to natural, physical, and technical investigations.

The North was seen by almost all scientists in the early part of this period as a natural laboratory for studying how Native cultures have adapted to a harsh

environment, and how the non-Native visitor could do the same. "Research can guide us to new solutions to age old problems of intractable climate, or impenetrable terrain. The North is a land of great riches guarded by massive obstacles... and mistakes are especially costly both in material wealth and human happiness " (Laing 1967: 100-03). Thus northerners, in their habitat, became free objects of study to government, universities, industry, and later Native organizations. Scientists were trained to be investigators, and, as science was called into its strategic tasks, the training of northern scientists increased, as we saw in the preceding chapter, through the training grants and the growth of northern institutes.

We also noted that by the early 1970s the government had embarked on a course of "balancing" human and development priorities in the North. There was never, in these years, a coherent research policy by sponsors to balance science; it was arbitrarily used to support the strategies of the sponsors, both welfare and development. As science took its legitimation from policies, controversies over social science legitimation began in this era, as will be seen. As industry and Native organizations began to sponsor social science, the "balance" of science and policy criteria became crucial.

To meet these diverse needs, studies were sponsored in areas such as wage work, education, relocation, alcohol use, delinquency, aging, welfare, and population growth. There was a primary focus during this period on the extent of presumed and observed social breakdown; reports repeatedly pointed out the impediments to recreating the south in the North (PAC, RG85, v. 1897, file 1003-1-4, pt. 3, NRCC; v. 1654, file NR2/2-2, pt. 1). The participants at a conference in 1963 sponsored by AINA revealed their distress at the extent of culture loss, and recommended leaving the northern populations alone until their time came to be encompassed into economic development projects (PAC, RG85, v. 1897, file 102-64, pt. 6). Other science

projects were geared to planning this "time" in order to reduce breakdown. Delta science fit into both kinds of categories.

B. Delta Research

Delta residents and their neighbours saw the greatest concentration of researchers during these years. In 1966, the Inuvik Drum (July 12) listed 12 researchers working in town on seven projects, primarily in the social sciences. After the opening of the Research Lab in 1964, the role of the "researcher" became somewhat institutionalized in the area, as we shall see (researchers p.c. May 19, 1982; July 19b, 1982). The types of research encompassed the range of models identified above: the long-term stay, the summer P-O, and the short survey; these will be discussed below in terms of a continuum. Before entering the discussion of conduct, a survey of some of these projects is necessary.

Jim Lotz (1962a) did a sociological community planning survey of Inuvik for DIAND in 1961 and discovered the buds of the difficulties that the Boeks had predicted. Paul Puritt did research on generational change and culture change/continuity in Fort McPherson in 1961, following Slobodin's early work there, but his report was not published. This study was part of a larger project under R. Cohen and J. VanStone of the University of Toronto (funded through NCRC) in 1961 to investigate assimilation into the wage economy; this multi-faceted study was a follow-up in turn to brief visits by Cohen and VanStone to the Mackenzie Valley communities in 1960 (PAC, RG85, v. 1656, file NR2/3-3, pt. 2). Janice Hurlbert (1962) and Hiroko Sue Hara (1980) did investigations at Fort Good Hope as part of this project as well (see Slobodin 1982). A. Balikci (1963) visited the Kutchin relatives to the west at Old Crow in 1961 and 1962 to look at social breakdown among youth. D. Clairmont's (1963) study of drinking and deviance in Aklavik was used to provide a rationale for controlling the use of alcoholic beverages in the area, another legacy of the move (the liquor store was

later moved to Inuvik) (PAC, RG85, v. 1654, NR2/3-42, pt. 1). These were all summer projects.

R. Slobodin (1966) returned to the area in 1963 to do the first study of the Metis population. The sociologist Charles Hobart, using a mixture of P-O and survey methods, began his work in the Delta in 1964. He has returned many times since, doing work on residential school adaptations, infant mortality and morbidity, work rotation for industrial employment, and industrial training programs (researcher p.c. Dec. 11, 1981; see for example Hobart 1968, 1970, 1975, 1976, 1981). J. Fried (1964) conducted a brief survey of Delta communities, as did R. Cohen (1962).

More research was done for the Area Economic Surveys of the Industrial Division of NANR. P. Usher (1965) and G. Abramson (1963), working together in the early 1960s, surveyed the coastal areas, including Tuktoyaktuk, Cape Parry, and Banks Island; they made occasional visits to the Delta. Usher was to continue his researches on Banks Island in the late 1960s and early 1970s (Usher 1971a; see Chapter 7). Don Bissett (1967) included the Delta in his survey of the Lower Mackenzie. These surveys were also summer projects, and combined interviewing with observation and some participation. These researchers were not all anthropologists. Some, such as Usher and Lotz, were geographers, and Hobart was doing sociological research. They are included here because the nature of their human investigations, and their methods, were similar and collectively set a model for research at the time. In addition, local residents were often not aware of disciplinary boundaries and responded to researchers by method and approach rather than discipline.

The major Delta project of the late 1960s was the Mackenzie Delta Research Project organized by NCRC and its successor, the Northern Science Research Group. This was the first major interdisciplinary, large scale project produced by the unit. The MDRP was intended to resolve problems of coordination, on the research level, and

planning, on the policy level. Its purpose was to "isolate and analyze social and economic conditions which impede the extent to which the [northern residents] are making effective adjustment to changes brought about by government and commercial expansion in the north" (MDRP 1965: 3). The Delta was an area earmarked for development, and Ottawa wanted to know more about the preconditions and potential mitigations of development impacts on Native people, and thus science, as suggested above, became a tool for rational planning.

The history of the project will be further discussed in the next chapter. The MDRP incorporated both preliminary and final studies in different disciplinary and topical areas. The topics were chosen within government, and personnel located -- primarily from the universities. Most of it was social research on the problems of the Native and, to a lesser degree, non-Native populations, and most of it was conducted in the summer. For example, J. Wolforth (1971) did an economic history and survey of the Delta, and J. Lubart (1969) analyzed the cultural psychology of the Inuit in relation to their response to change (see also Ervin 1968; Mailhot 1967; Parsons 1970). A.J. Kerr, the director of the project, made periodic summer visits as well. Two of the investigators (D. Smith 1975; Eades 1971) stayed in the Delta over a year, doing long-term P-O. C. Aasen and W. Wright did a year-long cultural geography study which was apparently never published as part of the MDRP series.

Other researchers also came to the Delta and environs in the late 1960s and the early 1970s. A doctoral student worked in Ft. McPherson for a year studying the fur trapping economy (Krech 1974), to be followed somewhat later by another such student, G. Miller, who was in the same community doing the ethnohistory of the early hunting and trapping culture (p.c. Oct. 6, 1982). John and Irma Honigmann also spent a longer period of time, 7 months, in Inuvik in 1967, observing adaptations by Native people to the new urban lifestyle (1970). Although they were not formally associated

with the simultaneous MDRP, the Honigmans acted as advisors to it; Irma Honigmann had an NCRC contract to study childhood adaptations (researcher p.c. July 19a, 1982). G. Lewis (1972) and H. Black (1975) did summer thesis research projects in the early 1970s in Inuvik and Aklavik. AINA's Man in the the North project sponsored two community-based education investigations in the Delta in 1972 which occasioned visits from coordinating and evaluating personnel (see MIN Technical Report 1973).

There was also linguistic research in the Kutchin communities by M. Durbin and J. Ritter in the 1960s and 1970s. Neighbouring Hare and Kutchin groups were still being investigated; J. Savishinsky (1974) went to the Fort Good Hope Hare, and Ann Welsh (1970) did P-O among the Old Crow Kutchin in 1968-69, followed by survey impact assessors associated with plans for pipeline development (Bissett and Meldrum 1973; Stager 1974). The Science Council of Canada sponsored a study of "technology assessment" in the Delta and Beaufort Sea region in the summer of 1974 which involved interviews with northerners (Keith et. al. 1976). Karl Francis (1973) visited the Delta area as part of a geography study on travel in 1971-73; he did some interviewing but also used Native residents as interviewers. Federal listings for licensed research mention other projects for this period for which I could find no record.

C. Conduct - the Continuum

Most of the data available on conduct and on response come from this time period. The various types of social research can be seen as ranging on a continuum, from those with very little participation, with a reliance on interview and observation, to those in which the researcher immersed himself in both participation and observation. The type of research chosen depended upon the disciplinary background of the researcher, the conditions set by sponsors, the size and nature of the host community, and the personality of the investigator. During this period, too, northern people began to

actively respond to researchers and to set some conditions on their success, so that this, too, shaped the nature of the investigation.

In the discussion to follow, we will look at some of these variables by describing a continuum of types. The description of the continuum, which will focus primarily, but not exclusively, on the Delta research, is taken from accounts by researchers themselves, oral or written. Similarly, the objectives of sponsors, particularly the federal government, must be accounted for. In a second subsection, we will discuss separately another method of investigation which arose during this time, the consultative hearing. This method originated primarily with the Berger Inquiry, and has become a tool of social research.

i The surveyor or consultant who enters a community for a short period of time, who speaks primarily with key individuals recommended to him by his sponsors or the local administration, and who makes brief observations, represents a kind of research with minimal participatory involvement (see Bissett 1967; Cohen 1962; Lotz 1962; Stager 1974; Wolforth 1971; Gemini North 1973). Most visits have been made in the summer, encouraging the image of researchers as "summer birds," although more recently consultants have come into the Delta at all times of the year. Often no attempt is made to establish rapport other than the minimum necessary for the interview process, and interviews are conducted only with English-speaking informants, as a rule. Identification is with sponsors, usually government or industry, and the role played is the same as would be played in the south by a government employee or consultant. This mode of research had been operational since early in the century, but it became more prevalent with the design and management of the new town of Inuvik, and with the growth of government infrastructure and industrial development. Most of the research conducted for industry and government concerning the proposal for a natural gas pipeline in the early 1970s, and its assessment, was of this type. Industry often hired

private firms/individuals to do the research, with some involvement by university-based institutes. As we will see, this is also the kind of research which has come to dominate the most recent social inquiry, particularly by industry and government.

Many of the reports from consulting/survey research did not list methods in detail; this was true, for instance, with the firms preparing documents for the Berger Inquiry. Objectives were clearly defined and often narrow; the consultant entered with a specific set of questions in mind. The tendency to circumscribe inquiry has increased from the beginning to the end of this time period, partly because the reasons for research were more clearly delimited, and partly because the type of researcher changed from university-based investigators to members of private firms with precise research scenarios and limited time frames (researcher p.c. Dec. 7, 1982). There has often also been a reliance on existing data, particularly statistics, collected by government and industry -- sometimes with little critical insight into the derivation of statistics.

Because methods were not described in detail, it is in fact difficult to make a balanced assessment of these kinds of northern forays. Visits have usually been brief, and key informants used for most interviews. R. Cohen, in an early example (1962: 2), did a survey of 12 communities, including those in the Delta, in 1960, spending from 3-14 days in each one. He described his techniques as combining interviews and observations. His interactions with non-Native people tended to be more formal than those with Native people, and he took the role of a writer or teacher working for the federal government. He noted that this role could be threatening to Native people, who were already beginning to distrust the government, and he suggested that a more passive and involved P-O stance might have been less threatening. More recently, a firm doing research on recreation at the time of fieldwork in 1981, for example, came in the fall for a few days and, like the writer, used a network approach to locate informants.

Interviews were brief and conducted primarily by telephone, however, with little personal information exchanged to contextualize the data obtained.

ii A second group of researchers consisted of the successors of the unique limited-time P-O model developed in the 1950s. These investigators usually stayed in one or two communities and took a middle stance between establishing effective long-term rapport with the Native people and maintaining an essentially non-Native role as teacher, student, or researcher.

Those who were anthropologists usually tried to do some participant-observation. This entailed both observation and interview, supplemented with attendance at public events such as dances, feasts, meetings, or bingo games, and with joining activities such as hunting or fishing. The Honigmans describe their participation in Inuvik:

Social participation took us into public facilities like The Bay (as the Hudson's Bay Company now calls itself); bakery; liquor store, court; cafe; coffee shop, where workmen take coffee breaks and kid the attractive, native counter girls and where juveniles after school play pinball machines; government-operated laundromat that contains public showers (but we never met another soul in the cavernous shower rooms); public library; and post office, a place sure to be crowded at the end of the day provided the plane from Edmonton is on time. . . .Bingo games at the rectory cost us more money than they returned in winnings, but they offered a weekly opportunity to see a big cross-section of the native adult community. . . .On later occasions we went to the gym for dances, to see shows put on by touring companies, to watch basketball games, and for a Girl Guide bake sale. On summer evenings we brushed away mosquitoes while watching the men's or women's softball teams playing. . . .From time to time we dropped into the hotel's dim cocktail lounge and noisier tavern, the latter alive with inebriated, exuberant fellowship that excluded no one. Twice we experienced the pleasure of a canoe trip on the river and through quiet, twisting sloughs . . .(Honigmann and Honigmann 1970: 5-6).

Similarly, another researcher and her assistant in Inuvik in 1967 did likewise:

We participated in drinking sessions at the hotel, attended every possible sports event in town, had tea in the afternoon and dinner in the evening with a certain number of families we had become friends with; we went picnicking with the girl guides and their leaders, attended as many meetings of the numerous voluntary associations as we could, attended many religious services every

Sunday and became friendly with the different religious leaders and so on and on (researcher p.c. Nov. 23, 1982).

A few tried to learn the language, but this was not feasible in a short period of time. They tried to learn the rules of deportment which would facilitate both rapport and the understanding of culture and social relationships, including introductions and acknowledgements.

I put a notice on the radio that I was there, and I wrote letters to some households [in Inuvik]. Most people were cooperative (researcher p.c. July 22, 1982).

I used the radio to thank people for special things, such as a hunting trip. I think they liked that (researcher p.c. May 10, 1982).

Non-Native officials and records were consulted for information on social statistics. Non-anthropologists relied more on these records and interviews than on participation and informal conversation, according to their training.

Other kinds of reciprocity involved the roles played. In chapters 1 and 2 we discussed briefly the importance of roles played by, and assigned to, anthropologists world-wide, as reported in accounts of field experiences. To review, roles were defined as sets of cultural expectations regarding behaviour appropriate to given activities or events, which may be to varying degrees crystallized around a social structural position. Roles should not necessarily be taken to denote the social positions or statuses themselves, then, but they refer to the expectations surrounding these positions. For the researcher, the role is ultimately an achieved, never an ascribed part of the social setting, but it is nonetheless important to the establishment of the place of the visitor in the natural context. The associated expectations include some about communication: what communication can be gotten from the person; what can she be told; what will he do with the information?

Roles can thus be crucial to information-gathering and the creation of knowledge about culture. We saw that early fieldworkers tended to see roles as structurally

inflexible, and they admonished would-be fieldworkers to pay close attention to the kinds of roles played and the results. It was recognized that evaluation of role-playing must be done carefully by both host and guest. An initial role may be assigned to visitors; these have commonly included teacher, administrator, student, writer, or spy. Anthropologists have often been assigned outsider roles derived from colonialist experience, even though their activities may not strictly conform to these role-type. They may be initially suspected of being spies, of having a dangerous ambiguity. Or, their failure to fit into adult roles may lead to a more harmless result, the assignation of the position of a child. Once it is realized that the visitor is so ignorant as to be harmless in his intentions, at least in the immediate setting, the anthropologist be reassigned new roles. He may find a kind of "learner" role, child or student, useful in receiving instruction and information; this also allows the researcher to be more easily inserted, or socialized, into the local kinship system -- in itself a source of valuable insight.

Role settings in the field have become more complex over time. As the numbers of journalists, anthropologists, and other investigators have increased, familiarity with the researcher-type of role has increased for many residents, often beyond the visitor's initial expectations. Habits of writing, of asking questions, even of role-playing itself can be understood by the creation of this special role. It is in some ways an elaboration on the ambiguity of the stranger, since the duality of position is recognized and linked with the role.

With changes in methodological focus, along with the evolution of a researcher role in many areas, role playing has become more flexible as well as complex. As we will discuss below in regard to Delta fieldwork, the role of the "anthropologist" can be restrictive even while it is flexible, in two respects: the expectation that the researcher must play the same role in all settings, and the continual expectations that

he must take a dual stance. In order to overcome these restrictions the researcher must use the duality to increase flexibility instead: emphasizing the closeness of friendship in some cases, for instance, or the distance of the stranger in other settings. This flexibility, along with the above-mentioned ambiguity of role, allows a range of behaviours which can be useful in "testing" appropriate behaviours and responses. The role of the "friend" also aids the outsider in overcoming the limitations of being a stranger. The more the anthropologist has become a "friend" to his/her hosts, the more he has access to backstage information, but the more strongly he is cautioned to adhere to a set of internal rules and observations -- thus requiring a reciprocal distance. Interactionist theories, such as ethnomethodology, have been used to explore the potential of role flexibility, within participant-observation, in the use and circulation of knowledge.

The situation in the Delta (and elsewhere in the North) has been similar in the fundamentals of role-playing to other international field settings, and even in the types of roles utilized by researchers: student, writer, teacher, spy (researchers p.c. July 19b, 1982; Dec. 11, 1981; Oct. 2, 1982). In the preceding chapter we have discussed some of these types in relation to the evolution of method, culminating in the 1970s in the widespread acknowledgement of the anthropologist as a "type" of person using particular interaction methods. Early researchers reported that the Native people were not sure who and what they were. Stefansson (1913: 179) has stated, for instance, that the Eskimos could not understand why he was not really one of them, since he could speak the language. Other researchers, similarly, have reported this kind of confusion about the mission of the ethnographer, but in more contemporary times (researchers p.c. Feb. 17, 1983; July 19b, 1982; Osgood 1953; R. Nelson 1973; Helm 1979). As people became more familiar with roles brought from the outside, then the association of the behaviour of the investigator with roles like that of administrator, or, in particular,

teacher, was common not only in the Delta but in other parts of the western Arctic and Subarctic. John Honigmann (1970) has compared his research among the Kaska (Athapaskan Indians) in the 1940s to his research in Frobisher Bay (Inuit) twenty years later. In the first instance, given his age, lifestyle, and interests, he felt he was identified with non-Native trappers rather than with transients, whereas in Frobisher Bay, because of changes in the same factors, he could not escape association with transient roles. His personal inclinations had changed from youth to middle age, affecting his willingness to participate in activities such as drinking parties. And, the two communities were very different in size and composition, which also affected the potential roles and the labelling process.

When I first started doing fieldwork in Alaska in the 1950s people did not know what either an anthropologist or a schoolteacher was, but they were tolerant and helpful. The people had little experience with outsiders so the role of teacher was less threatening. Later they came to understand that researchers wrote books or took information away to get rich, with some possible benefits in the long term. The young people who had been away to school, in particular, seemed to know more about what I was doing (researcher p.c. Feb. 17, 1983).

In late August, 1964, I was flown into Wainwright. I was alone and knew no one in the village, but was identified in part by Dr. Frederick Milan, who had done ethnographic work there previously. The first problem was settling into the house provided by the Arctic Research Laboratory and, of course, getting acquainted with the villagers. As one would expect, their initial reaction was one of reserve, which gradually gave way to "acceptance" over the weeks and months of my residence. Newcomers and outsiders are regarded with reserve no matter what their business may be in the village, but certain factors can increase or diminish this reaction. School teachers, traders, and missionaries have been visiting North Alaskan villages for many years and have come to be regarded as something "normal." But the resident scientist appears much less frequently, and his motives are more difficult to fathom. Anyone who is continually asking questions appears to be greeted with somewhat more reticence than the "normal" visitor or temporary resident. This is especially true if the questions concern subjects which are considered unimportant or trivial, or are a part of the culture which has disappeared or gone underground (R. Nelson 1969: 392).

Even when a researcher, like Honigmann, was placed, or placed himself, in a familiar role, people often still found his behaviour ambiguous: why did he ask so many questions; why did he keep notes (researcher p.c. May 11b, 1982)? Even the roles

themselves were loosely formed, since the first administrators, for example, were considered very unpredictable. Given the combination of role and performance, then, the investigators were still suspected of being strangers, of perhaps having a foot in another world not fully understood.

Researchers in the late 1950s through the early 1970s in the North, then, as elsewhere, often found that existing role types, even when they were themselves imperfectly understood, were useful in giving them some predictable information access within a flexible setting. Another who worked briefly in small communities in the early 1960s found, however, that in a short time even a familiar and flexible role did not allow sufficient growth of rapport, especially if the role -- in this case of civil servant -- restricted social participation with Native people:

I had some house-to-house questionnaire surveys. But in the little towns I could not break down their reserve in a short time, especially with Indians. So I used local assistants when I could (researcher p.c. Dec. 7, 1982).

Students and teachers were better role models, since these people were, after all, those who learned and traded in knowledge; as hosts came to understand these models they began to form prototypes for comprehending and shaping investigative roles. The development of acceptable roles has been part of the development of methodology, related to reciprocity, learning of language and culture, rapport-building, and other rules. It was during this period that the role of the researcher fully evolved from factors such as the immigration of "experts" who did local investigations and made policy decisions (such as the surveyors of the Aklavik move); the proliferation of researchers of different kinds and dispositions; the association of social scientists with journalists, some of the latter of whom manipulated anthropological roles (researcher p.c. May 9, 1982); the growth of the government sector and the increasing familiarity by residents with its many functions; and, finally, the increasing self-assignment by investigators

of the role of "researcher," "sociologist," or "anthropologist." In Inuvik, the role of the researcher became associated with the Research Lab in the 1960s, and was still there at the time of fieldwork as many people continued to interpret all research as having been "hired" by the Lab. The writer frequently encountered this assumption in her own fieldwork. A specific pattern of expectation had come to be associated with the role, as a structural position, and it was used to varying extents (Ervin 1967; Lubart 1969; Parsons 1970; Hobart 1975; Honigmann and Honigmann 1970; H. Black 1975; Lewis 1972; Abramson 1963; Mailhot 1967; researchers p.c. Feb. 17, 1983, Dec. 11, 1981; July 22, 1982; PAC, RG85, v. 1276, file 251-1-4 Area Economic Surveys 1961-63). All roles played by outsiders have been affected to some degree by these restrictions, interestingly. People commented upon how old-time priests were able to play many roles in relation to the people, but as other individuals, such as medical care workers, have come to assume these functions, the role has narrowed.

And as the role became more restrictive, and its associated implications of utilization and quality of knowledge were more widely understood, researchers were reluctant to admit to it openly. As we will see in the next chapter, a certain negative imagery also came to surround the role of participant investigator and this has added to the restrictive expectations. While the researchers did not deny that they were social investigators, and associated with government (usually), this information was often not freely offered.

The community had known researchers before and had been exposed to house-to-house interviewing, and we sensed its resistance. Humorous comments about it being an early season for anthropologists alerted us to be wary (Honigmann and Honigmann 1970: 5).

I said that I was interested in learning about Native people. I also denied sometimes that I worked with the government. I could have been more honest (researcher p.c. May 10, 1982).

I did not hide the fact that I was a sociologist, but I did not flaunt it either. I just said that I was a researcher and people tried to associate me with one group or another (researcher p.c. July 22, 1982).

We had to be careful not to be associated with one part or other of the town [of Inuvik]...it would have been a natural and easy thing to associate with the non-Native part of town, and we felt friendly pressure from that direction (researcher p.c. Nov. 23, 1982).

I tried to set myself apart from the other whites; they came to realize [in a small Inuit community on coast] that I was not a government employee. I was "one who writes" but I tried to make my work appear concrete, and relevant (researcher p.c. July 19b, 1982).

In small communities where individuals can normally be allowed to play multiple roles in relation to groupings of neighbours and friends, the researcher who is expected to play the same role in all settings loses the freedom to adapt. This placed boundaries on information flow. The alternative has been to either deny the label or to once again broaden the expectations and flexibility associated with role, as noted earlier in the section. Researchers spoke of trying to play multiple roles, including that of "friend," to broaden their access to people and information and to achieve the multiplexity of the "natural context"; this was easier in the long-term P-O discussed below. Some chose to establish friendships with some informants, and, as in the writer's own fieldwork, these relationships were important sources of information. Friendships were in some cases based on a kind of reciprocal testing and role definition, however, as the parameters of the research and of the personal relations were tested; fieldworkers showed much concern about the nature of these relationships when interviewed, and some said that the interactions by themselves or by other fieldworkers were problematic in terms of the reciprocity of information. One of the main difficulties with "reciprocal" relationships in this period was the use of bars and taverns for fieldwork. Even in 1981 both Native and non-Native residents complained that they had been exploited by researchers who would buy alcohol for their friends and then encourage them to talk freely; this system was part of the testing of the limits of

friendship. One government-sponsored researcher in the Delta in the early 1960s got into trouble with the non-Native sector after participating in drinking parties with Native youths, ending in arrests and charges (PAC, RG85, v. 1656, file NR2/3-3, pt. 2):

If I could change one thing about the fieldwork I did in the early 1960s (in a small Indian community), I would no longer drink with the Indians and help them with their brew parties; I feel uneasy about that (researcher p.c. Feb. 17, 1983).

In those days everyone was in the bar. We often had parties afterwards, and drank home brew, and sometimes there was fighting with guns and knives (researcher p.c. Oct. 6b, 1982).

Some anthropologists, particular graduate students, would do anything to get data. One anthropologist in [Delta community] was run out of town for drinking and cavorting with girls. The people watch everything we do (researcher p.c. Oct. 6a, 1982).

One researcher [in a Delta community] was tactless in style; he drank with the women and got them to tell secrets although the people didn't seem too upset about it. I had a female [Inuit] friend whom I tried to protect and I was afraid rumours would spread misinterpreting our relationship. . . . Once I got drunk with a [Native] man I worked with and when he was drunk he told me how much he disliked anthropologists and threatened me...but later he said I was one of his best friends. . . . I drank with them because it helped to get to know them, even if I wouldn't do it in other circumstances. . . . Once the Native people stuck up for me when the RCMP asked me why I was in the Native part of town partying (researcher p.c. May 10, 1982).

In some of these cases, use of local assistants helped overcome barriers and to employ local rules of reciprocity; this of course is a central value of using key relationships, and those using short-term P-O had to rely extensively on both on quick, intense friendships and on key informants or assistants, often paid.

We were friends with a Metis young man [in Inuvik], around 20, who lived in the unserved end of town and who knew everybody. We had made him a map of that area locating every household so we sat down with him and went over the map Things he was not sure of he took upon himself to check with other people. I don't think we paid him for that work, in fact I don't think we paid anybody for information (researcher p.c. Nov. 23, 1982).

At \$2.00 per hour plus \$1.00 "northern allowance" we had no trouble engaging our first informants, two men, on an Indian and other an Indian Metis. But the ease with which we found them proved deceptive, for we could not as easily locate other informants who would fit desired specifications (Honigmann and Honigmann 1970: 6).

iii Longer-term, "classic" fieldwork allows one more time and flexibility in establishing participatory roles, and Delta fieldworkers who did this kind of research found that they could surpass some of the limitations of the researcher role and re-establish the latitude of more general "learning" and "teaching" roles. Indeed, this kind of fieldwork, allowing a period of gradual observation and learning, accorded best with Native learning styles and thus found more acceptance among Native residents. These studies were primarily in small communities with predominately Native populations. Several Delta researchers did this, living in settlements and in the bush with Native people for up to a year or more (see Smith 1975; Eades 1971; Krech 1974).

The researchers in this category worked primarily with Native people, identifying both publicly and privately with them as much as possible. Like the summer participants, they used a variety of cross-checking data sources and participated in community and household activities in settlement and bush. The achievement of rapport and friendship was critical, as this facilitated the reporting of Native life from the inside point of view. Roles were central to the process.

On my first trip to live with the Inuit they did not know what to make of me. I did not play a very structured researcher role. I introduced myself as someone who wanted to live with them and learn to live like an Inuk. They took that seriously and literally treated me like a child at first. I had to sit with the women in the whaleboats, and occasionally I was left behind with the women. I tried to establish an anthropological role, asking questions, but they had trouble accepting that. . . . Most Inuit were embarrassed to have whites in their homes, so I stayed away from the white sector and they did not care too much about me and my work (researcher p.c. May 9, 1982).

I did not introduce myself as an anthropologist [in a small Delta community]. I said that I was a teacher, and that I wanted to learn the language and to see how Native people made a living (researcher p.c. Oct. 6b, 1982).

Most people did not know what anthropologists were, except that they asked silly questions and tried to learn the language and made mistakes. The Dene often called me a teacher, and thought that I was eccentric to live among them. The whites called me an "Indian-lover" (researcher p.c. May 11a, 1982).

The role of student learning Native language and culture was often the most effective, as it made it possible to establish interaction patterns familiar to both parties -- even though it also led to some of the negative images of incompetence sometimes associated with anthropologists, as we will see. Regardless of the role chosen, these social scientists were aware of the advantages of having some elasticity built into it. This could be done more easily in smaller communities or in the bush, where the immediacies of interaction with a small group of people allowed both flexibility of behaviour and acceptance on a personal rather than a categorical level (researchers p.c. July 19b, 1982; Oct. 6, 1982; May 12, 1982; May 19, 1982; see Brody 1975: 71-71 for a discussion of this in his own northern fieldwork).

The Dene appreciated me for personal reasons; for some reason I fit right in. They would always talk to me (researcher p.c. May 11a, 1982).

There is no need to build elaborate explanations of what you do. If you are natural, tactful, you will be accepted, as I was. Be honest. I tried to be easy-going with them, and spend as much time with them in the bush and on hunting trips as I could (researcher p.c. Oct. 6b, 1982).

Reciprocity was perhaps even more important for those participants wanting long-term relationships than it was for short-term visitors; the role of friendship took a deeper, more generalized meaning. They were less likely to pay their teachers, although there was still a reliance on key individuals (Krech 1974; Nelson 1973: 7-10). Reciprocity must be more continuous, active, and subtle. These researchers joined in exchanges of food, goods, and services, as well as in the more casual reciprocity endemic to the process of establishing and testing personal relationships (D. Smith 1975: xiii). The greater the expectations which are built, the more the long-

term researcher is pressured to return and to keep up with relationships established in the field.

I still host northern people when they come south. They helped me, and I still feel I owe something to them. One anthropologist paid people just a small amount for filling out a long questionnaire. This exploited them, especially when they realized that he had money that they did not. I tried to pay well per hour for those who worked for me. I also did some work for the band office (researcher p.c. Oct. 6a, 1982).

I paid people for gas and food when they took me on hunting trips or to camps, and they appreciated that. I established several strong friendships; I tried not to use them as entrees into things, but sometimes that happened. One man asked why I had not asked his permission. I said that it was because he was drunk all the time, and he liked my honesty. He became my partner, checking the data for me; he was also an elder, a leader, and that helped. Most people never return. When I did, people were pleased (researcher p.c. Oct. 6b, 1982).

Most of the researchers learned the language well enough to communicate on everyday matters, again facilitating rapport because the Native people could more easily accept and identify with a non-Native person who had this competence (researcher p.c. May 19, 1982; see Brody 1975: 4-14). The visitors learned to manipulate the contexts in which they were working, essentially, so that participation was an active process of reaffirming social relationships and playing both passive and active roles in learning. R. Nelson (1969, 1973), who has worked with both Alaskan Kutchin and Inuit, has practiced a kind of passive participation of watching and imitating hunting activities which accords with Native styles of teaching and learning. He nonetheless took an active part in helping with the hunt, and in playing the role of a new hunter.

The complexities of long-term fieldwork have demanded a reflexive stance, whereby the researcher becomes aware of the centrality of his own interpretations, and the difference between inside and outside points of view. The researchers interviewed for this study expressed this concern repeatedly; I also kept journals during my northern fieldwork where I could articulate doubts about the benefits and harms of research for both myself and my hosts (researchers p.c. Oct. 7, 1982; May 12, 1982;

July 19b, 1982; Nelson 1973: 10). Regardless of the amount of rapport they have achieved, they must maintain, anthropologically, an objective stance, and they never become fully integrated members of the society they study. Everyone knows that they will leave soon, and take the information away to be used on the outside.

I have great love and respect for the people. My work would not have been possible without this friendship. . . . But we all have left some negative impacts (researcher p.c. Oct. 6b, 1982).

Most people were polite and accepted me, but they watched everything I did, including my mistakes. We should be honest with the people. People feel betrayed, with no return, no feedback. We should not make promises we do not keep (researcher p.c. Oct. 6a, 1982).

There is a lot of self-testing in fieldwork. It is easy to get so involved that you lose all sense of purpose. My advisor told me to be self-reflexive, to hold dialogue with myself. . . . We see the Inuit as friends, but they may see us as useless. There is always a basic conflict between being a friend and being a stranger (researcher p.c. May 9, 1982).

I wonder if I should have been less reticent, more involved. It is difficult to take a professional role, asking questions, when you are dealing with people's lives. . . . Even [a respected anthropologist in the Delta] was accused of writing books and getting rich. . . . When I did fieldwork in the late 1960s people were just beginning to be concerned with ethics and privacy (researcher p.c. May 10, 1982).

Some of the researchers who conducted long-term fieldwork in the 1960s and 1970s began to work for Native organizations as researchers and advisors in the years of the Berger hearings, and have continued to work as consultants since -- although in some cases they have been replaced by Native workers in the 1980s. These researcher/advisors have pointed out that the Native people sought social scientists who had a familiarity and rapport born of intensive involvement and crafted trust (Helm 1979; researchers p.c. July 19b, 1982; Dec. 11, 1981; May 19, 1982; Brody 1981: xiii). They have also noted that working on the "inside," employed by the people, has given them added legitimacy. This validation has served to enhance acceptance, but it has not always alleviated the conflict between inside and outside perspectives and

representations, as we will see in the next two chapters. These issues will be put in focus in succeeding discussions on host response to field methods and the regulation of research.

D. The Mackenzie Valley Pipeline Inquiry - the Hearings Process

In Chapter 3 we mentioned the Mackenzie Valley Pipeline Inquiry of 1974-76, headed by Justice Thomas Berger (also referred to as the Berger Inquiry). In interpreting his mandate on defining the terms and conditions of development, Berger required participants to make all relevant documentation public. He also provided funding for Native, local, and interest organizations, such as COPE, the Dene Nation, and the Canadian Arctic Resources Committee (CARC), to prepare submissions to the Inquiry (Gamble 1978: 949). In addition, both the industrial proponents and the federal and territorial governments had initiated various impact assessments, and reviews of assessments, in the process of preparing, submitting, and evaluating development proposals. The years just before and during the Inquiry represented the climax of social research in the Delta as the various participants prepared their cases for approval/disapproval of the proposed gas pipeline. We have already noted how much of this research involved little original field research, apart from abbreviated community visits. Government statistical compilations were circulated among the researchers and formed the basis for the focus on economic, rather than social or cultural, issues in the reports. The Native organizations did most of the field work in preparation for their submissions; several advisor/researchers were hired to help prepare testimony and documentation, as we have seen (Wenzel 1978; Caloren 1978; p.c. July 19b, 1982; Dec. 10, 1981). Most Inuit households in the Delta were visited to produce land use maps, and people were apprised of the coming hearings and the substance of the

proposals (p.c. Nov. 12, 1981; Nov. 17, 1981). This, we have noted, was the beginning of social research by Native organizations in the region.

In this section we will discuss the hearings themselves as a form of research. Formal hearings were held in several large centres, North and South, to present expert witness testimony by both proponents and intervenors. This testimony was subject to cross-examination, and, as B.L. Campbell (1982) has effectively argued in a recent thesis on the testimony of expert witnesses in biology, the scientific arguments were often subsumed to the policy ones. The principal proponent, Canadian Arctic Gas, presented a panel of witnesses who solidly backed their development policy, whereas critics attacked their position on both political and scientific grounds. The social context of controversy affected the way science was publicly argued.

The other set of hearings, the informal community ones, were held in 35 settlements and involved approximately one thousand people (Gamble 1978: 949). While the Berger Inquiry did not originate the practice of holding community hearings and consultation in matters of social and economic policy, the practice has since become more widespread. The federal Environmental Assessment and Review Process (EARP), which has sponsored hearings at Norman Wells, and, more recently, the Beaufort Sea, began in 1973 and has used a similar model of public review. Berger's incorporation of informal hearings and flexible proceedings, and the positive response achieved by these measures, encouraged other agencies to proceed similarly in northern assessments since that time.

The Berger Inquiry is regarded as a watershed in the history of inquiries in Canada. Since that time, many people have assumed that a scientific assessment is or should be part of the inquiry process, and that an inquiry is the necessary first step before many major development can take place. . . . Since the Berger Inquiry, many people assume that scientific assessment is made possible through inquiries, for they alone permit the incorporation of the full range of information. . . . It has also been assumed, though often implicitly, that inquiries mesh into existing procedures of investigation and decision-making. Inquiries, it is said, may often cause delay, but the public contribution made

possible through them makes costs worthwhile, and it is assumed that the research demanded by inquiries from the proponents of new projects will be adequate to meet the demands of the interested public. These assumptions are problematic. How much an inquiry can or will function as a surrogate research team remains to be seen, and the nature and quality of its research remains to be tested. The Berger Inquiry may have caught the mood, aspirations, and special characteristics of northern native populations, but other inquiries investigating the same or different issues have not been so successful. The quality of the research carried out by the Berger Inquiry and its methodology have also come under attack, mainly in the daily press (Salter and Slaco 1981: 22).

The hearings process has been the focus of both praise and criticism of the Inquiry, since it has indeed become a form of "surrogate research." The act of consulting people through informal hearings has become a way of letting them participate in decision-making by providing a forum for public opinion. By holding a public meeting or hearing, an official can, ideally, inform people of government or industry plans, and find out what their reactions are; he can also use the opportunity, in the name of social research, to collect some kinds of information from the residents. If we examine the model of the Berger hearings in relation to the "rules" of participant-observation, we can see how this new mode of inquiry has brought about a structural shift in the mode of participation.

Berger treated the informal hearings as an "emergent" form. He let people shape -- or at least believe they were shaping -- the interaction according both to what they wanted to say, and how they wanted it said. People were encouraged to speak openly and freely. Most of the Native Delta residents who commented on the Berger hearings had positive reactions, believing that Justice Berger had sincerely listened to them and that the situation had been well-handled. They stressed that they were able, collectively, to produce a body of knowledge invaluable to local identity and external understanding. Two Inuit ladies said that they were "proud to have met that lawyer" (p.c. Nov. 5, 1981).

Berger's apparent success in rapport-building has led to the formulation of a model for the hearing-as-investigation, incorporating parts of the P-O model. The

roles that the investigators play are those of their official positions, with some concessions to local norms. The hearing/consultation is perceived by many administrators as a reciprocal arrangement whereby local people are allowed to contribute to decision-making, if only in a minimal way. They are told that they are advisors and experts, and that their knowledge is valuable to the social and political process. Although the forum is ideally open to all people, in fact a few spokespeople are informally selected to make remarks, either by local residents or by the officials convening the hearing; some self-selection is part of this. Thus, as foci of local expertise, they remain in the classic role of key informant.

One of the functions of having key informants, in this setting as in P-O, is to enhance the process of rapport-building. The officials seek some acceptance and rapport with the people to build cooperation, dressing in informal style and often letting the setting be "framed" by those attending. The writer had an opportunity during fieldwork in 1981 to watch the process in action while attending some of the hearings of the Beaufort Sea Environmental Assessment Review Panel. Submissions were both formal and spontaneous, and many residents expressed irritation at the seemingly endless repetition of hearings, telling the Panel that they had already told the government how they felt. Panel members responded uneasily and guardedly, trying to balance their task with the necessity of keeping a sense of rapport and cooperation. They, in turn, repeatedly stressed the importance of viewing the hearings as a cooperative venture in decision-making. Although people were allowed to say what they wanted to, the rapport present was often in fact only a veneer of reciprocity of purpose.

Apart from some flexibility of format, the hearings have generally been conducted according to outside, rather than local, rules. Some individuals expressed concern that they did not really understand the presentations of the developers; they did not understand the language or the technology so that they felt unable to comment.

Most of the Native people here don't understand what is said in meetings unless it is interpreted. I did not understand the big words, and I nearly fell asleep. I thought some of the comments people had were funny; we want to know what benefits the pipeline would bring us in our communities (p.c. Nov. 5, 1981).

There is a general recognition of the ideal construct of "time as knowledge;" people realize that a one-day hearing is no substitute for long-term intimate research. But, by structuring the hearings so as to make the resident the researcher, the insider the commentator, this rule can supposedly be circumvented. Instead of letting the social scientist do the research, and then be consulted for his ensuing expertise by the policy establishment, the administrator goes directly to the people with the expectation that their knowledge can be directly tapped, and that the perspective of science can be added as polish. The resident as "informant" or teacher is allowed to select relevant information and response to present in hearing, thus apparently "objectifying" and legitimating data for policy purposes. But the data are then subsequently reviewed and re-created by officials for decision-making, thereby removing the control of information flow from residents.

In looking at the comments and criticisms about the Inquiry results made by outsiders and by local people, two things become clear. Both refer more to the consequences of the Inquiry and the gamut of similar hearings that followed it than to the process of the Inquiry itself. One is that there is a difference between the ideal functions of the process and its actual, manifest, operations. The second is that the official or researcher conducting the hearings does not participate in the "natural" social context, but that he expects the people to participate in a context or format of his own design. These points are related. People felt that, in the conduct of the Berger Inquiry and of subsequent hearings, the results have not lived up to expectations about what the format can accomplish in terms of real communication and mutual growth of knowledge and influence. There has been no real rapport; the situation has not been one in which all

people feel comfortable expressing their views. People become more involved with roles than with relationships, and reciprocity is contrived and inflexible. It is hard for the northern resident to judge purpose and motivation, and it is not known whether decision-makers are listening.

It should be noted again that some of these reactions have developed in the post-Berger era as potential did not achieve promise. Some residents and observers have been more critical of the model/image of culture and participation created by the hearings than of the procedures themselves, feeling that, with the aid of a biased press, the hearings conveyed a romanticized view of Native culture which is a false portrayal of contemporary reality and which Berger "bought" at the detriment of local industry (Lotz 1977; D. Turner 1977; Dyson 1979: 276); we will return to this point. Many criticisms are largely derived from the fact that people have become aware that the "ideal" model has not been as successful in its consequences as had been hoped. The more hearings that are held, the more people become cognizant that perhaps the hearings are not as important to the political process as suggested; maybe they are public relations exercises, and maybe they benefit the researcher more than the researched. A Metis woman said she felt that Berger had "just lined his pockets" with the Inquiry exercise and that people no longer take the hearings seriously: "they just get drunk and say stupid things" (p.c. March 6, 1981).

Similarly, some have argued that the hearings are not effective as social science. This is because they cannot meet the ideal of serving as a substitute for carefully gleaned insight. People generally can comment only on a limited range of technical topics, it is argued, and may not be sufficiently informed of the development plans at hand to be able to contribute effectively. More fundamentally, the kinds of things which are discussed in the hearings, primarily qualitative opinion, cannot provide the same legitimating function as a broader range of qualitative and quantitative complementarity of data

(Salisbury 1977: 14; Lanari and Castonguay 1976; Stabler 1977: 58). Berger was criticized in particular for not backing his rhetoric about Native culture and economy with facts. One cannot confuse emic statements of local fears about what will happen with scientific assessment of the likelihood of those fears being realized, it is argued. In sum, the alternative vision of research provided by the hearings has not been accepted by the social science establishment itself as an adequate substitute for basic research, in and of itself. The problem noted by Campbell (1982) of the mixing of policy and scientific criteria of validation has resulted in the failure of the exercise to be taken seriously in either arena.

This relates to the second point made above about the control of the context of the hearings. Delta people believed, at the time of fieldwork, that they were being forced to participate, without adequate resources, in a southern-based exercise; the "natural context" within which the hearings operate is imperfectly understood and its rules are not their own, as we have stated. The classic P-O rules are inverted: the hosts are invited to participate in a ritual based in an outside context. The ideology of P-O remains, to some extent, intact. Hearings are justified by an argument that, by calling local people "experts," they are respecting the natural context of the North and engaging in reciprocal exchange of opinions. Indeed, it can be said, as many Northerners realize, that in fact they are not participating in a policy context at all. Both the individual at the hearing, and the official who convenes it, are part of a much larger national and international domain which is really setting the "rules." The problem is that the boundaries of context, and the requirements of accountability to it, are as yet uncertain, and the hearings process reveals a slice of a larger debate about these issues; this will be a focus for discussion of regulation and control in the next chapters.

4. 1976-1985

As we have stated above, the amount of social research in the North, including the Delta, has decreased in the past ten years. The conflict between independent and strategic studies has continued in both academic and in government/sponsoring establishments, with a perceived gap between basic and applied work which has only very recently begun to diminish. As we shall see in the next chapters, efforts to create a science policy have also continued within the policy frameworks of "balanced" priorities. Government priorities have come increasingly to compete with one another in a context in which coordination and communication have become even more unwieldy. Each actor has exerted his own priorities through sponsorship, as molded by economic and political objectives. Research has continued in general areas of employment, health, education, family structure, and alcohol use. Struggles by northern people for self-government, for control of education and health, and for land claims have all affected modern research. Impact assessment, and studies of land and resource use, have become more common, as identified above. Cultural preservation in the setting of ethnic relations has spurred oral history, language, and folklore collections. As Native groups have proclaimed nationhood, there has been interest in using culture and renewable resources as foundations for these nations, casting new perspectives on the nature of culture.

In the Delta, there was a definite decrease in independent, basic social science during this period. Some investigators made return visits, and a few new projects were begun. V. Paraschak (researcher p.c. Dec. 4, 1981) was in Inuvik in 1980 doing research on Native recreation, for example. There has been medical research on the social aspects of health, such as that by O. Schaefer and colleagues in 1977 on nutrition (Schaefer et. al. 1980; p.c. Feb. 10, 1981). The researches conducted by COPE in languages, health, and land use have already been mentioned; there have also

been continuing archaeological investigations in the Delta sponsored by the National Museum and the Prince of Wales Northern Heritage Centre.

There have been very few projects involving long-term participant-observation in the social sciences. The studies done by V. Paraschak (Inuvik) on Native recreation and by G. Miller (in Fort McPherson, as mentioned in the previous section) both utilized these methods. Both individuals participated in the Native community, living within it, and relying heavily upon observation as well as interview and participation in community and social activities. Both researchers identified with the Native sector, and both found that P-O has become difficult in a context where response to research by Native and non-Native residents has become guarded and often negative. These issues will be further discussed in Chapter 6; the researchers found that they, like other long-term researchers, were able to achieve acceptance and rapport on the basis of personality and intent rather than on the basis of role. The assumption of roles has in fact become one of the most problematic aspects of P-O as hosts have questioned some of the consequences and images associated with "social researcher" or anthropologist roles. In addition, the researchers found that they encountered a great variety of expectations within both Native and non-Native sectors, and that consequently they had difficulty retaining a neutral, and dual, stance -- a classic problem in participant-observation (researchers p.c. Dec. 4, 1981; Oct. 6a, 1982). The writer experienced similar reactions; because of the diversified nature of the populations in towns such as Inuvik there were constant pressures to align with a definite social or cultural group. This would make it easier to "place" the researcher in pre-existing role sets and to predict behaviour and outcome.

Most recent social research in the Delta has used one or more of three basic methods: hearings, short-term surveys (often by consultants), and the use of local interviewers. The use of hearings was discussed in the previous sub-section. Both

industry and government have made frequent use of hearings, both to disseminate and to glean information. The use of survey consultants has likewise grown. People who specialize in either research methods or science expertise are called in to address specific planning or monitoring programs, hired by industry, local governments, the territorial/federal governments, and interest organizations. As we have already suggested, the increase in assessment and monitoring as part of the economic development process has correlated with the boom in consultancy, since consultants were able to meet the needs of short-term directive research. By employing the researcher for delimited tasks, people apparently hope to gain more control over the effectiveness of social science, although there is also suspicion that people hired on a short-term basis will be accountable only to themselves. In addition, the use of consultants tends to be poorly coordinated, and the work duplicative (Ritchie 1978: 69). Because of these constraints, consultants have tended to rely heavily on re-circulation of quantitative (usually economic) indices collected by government or industry.

Consultants may combine formal survey methods (often taking only a few days) with informal interviews, observation, and public meetings, extending the method into the second type noted in the continuum above -- but without the participative ideal. For example, the socio-economic surveys of Beaufort Sea drilling carried out from 1976-79 by the territorial government used all of these methods (see Collins 1976; Foster 1980). A sociologist doing work for industry on work rotation and job training also used interviews to determine responses of northern workers to job programs (researcher p.c. Dec. 11, 1981; Hobart 1976). In 1982-83 the federal government commissioned a survey of some of the communities upriver of the Delta affected by the Norman Wells project to monitor socio-economic impacts. The survey, directed by a southern university, included annual surveys of four communities on population,

labour force, and business development (Savoie and Meldrum 1984: 6). Students did much of the summer field work, collecting quantitative information primarily through use of government statistics and interviews. Native people later criticized the study because of its bias toward beneficial effects, its quantitative emphases, and the use of outsiders to conduct the study. This resulted in the stalling of the study in 1984, and the withdrawal of two of the four communities. By 1986, the project had nonetheless issued 26 reports on cooperating communities (see for example DIAND Norman Wells Socio-Economic Impact Monitoring Project Reports 1982-86; Green and Bone 1984; Stewart and Bone 1986; Native Press July 27, 1984: 3; April 8, 1984: 9; March 9, 1984: 6; Inuvik Drum May 24, 1984: 12).

As mentioned in the previous chapter, other surveys were also underway in the Norman Wells region in the early 1980s, including the Joint Needs Assessment study and at least two other employment surveys. These also apparently used summer survey methods, combining a heavy emphasis on quantitative data with qualitative insights. They also made use of the third method, the employment of local researchers.

Because of the problems identified above, there has been an increased call for indigenous researchers. We have already discussed, in Chapter 4, the increase of research conducted by Native organizations, and we will further discuss the use of internal researchers in the next three chapters. The use of Native residents as investigators is replacing the older P-O methods of the anthropologist in these contemporary surveys. Hobart (1976), for example, used Native interviewers in one study of work rotation patterns. COPE has hired both outside consultants and local people to do interviews; local people had, at the time of fieldwork, been recently involved in the Delta in education-oriented surveys also.

The Dene Nation program for monitoring Norman Wells was also mentioned in the last chapter. This project, planned in part as a reaction against the federal monitoring

study noted above, does not directly involve Delta communities, but is being conducted by their neighbours upriver and will perhaps serve as a model for future extended efforts by the organization. The study was developed in collaboration with university-based researchers from UBC, but most information-gathering, which began in 1985, was being done by community residents (Rees 1986; Northline 5(2) 1985: 3).

The case for Native researchers has been argued in the literature since the 1970s (see Whiteside 1974; Francis 1973) as a means of addressing issues of perspective and accountability, as we will see in later discussion. Not only do these studies combine the differing contributions of an outside, academic community and those of community representatives, but, in so doing, they incorporate some of the ideals of P-O with a series of specialized methods. The objectives of rapport, reciprocity, and immersion can be met by using insiders, and the duality of perspectives can be achieved without ensuing marginality, hopefully. The roles used by local researchers may still be subject to definition, however, since the social researcher role and image has heretofore been defined in association with outsiders.

The kinds of research being conducted in the past decade, and their rules, reveal that the frame of participant-observation has been narrowed and redirected. Researchers rarely seek to "go among the people" for extended periods with an emergent methodology. Rather, methods and objectives are set out in advance, and the mode of interaction with the people is circumscribed and specialized. Investigators rely on key informants, even in the setting of community hearings, so that most people are not aware of the interactions or their consequences. Here we confront an issue typifying all of social research in this area, however. Most of the researchers, whether doing P-O or more limited survey work, have sought data and opinions representative of the population being researched. Again, the ideal of total participation and inside knowledge is being sought. By identifying Native people as "experts" and providing the illusion that

they are in control of the research process, it has been thought that the ideals of dual perspectives and "objectivity" have been retained, even though the praxis is not there to support them. There is still a reliance on a few key informants, as in classic P-O, but without the intensive building of knowledge through communication which serves as a check on the secondary filtering of knowledge. The Native style of learning and the reciprocity of long-term relationships are aborted as the quick dialogue of the specialized hearing or consultation replaces observation and qualitative testing.

As the methods of research have become more specialized, the model of participation, then, has continued without the reality of involvement, as argued in regard to hearings as research process. The context of participation is no longer the "natural context" of northern society, however, and this is where the principal difference lies, and the continuity is broken. Even when the researcher seeks to "know what the people think," he realizes that northern and/or Native cultures can no longer be assumed to be isolated. Both sides to the interaction are, as argued above, part of a larger context of a national political economy and culture. The specialized methods of consultancy and hearing can be justified on this basis. The context which must be entered and understood is a shared one, and these short-term methods can be thought to be effective because of shared presuppositions about the nature of what is to be understood and the data necessary to understanding. The difference between Native and non-Native cultures is no longer assumed to be pre-determined, but must be negotiated, bounded, and validated within this larger shared context. The rules of conduct are emergent from this broader social context rather than from the narrower cultural, or natural, one, and are intended to short-circuit the P-O dialogue of duality, reciprocity, and comprehension. As we will see in the next chapters, new forms of locally-based accountability in communication are intended to remedy this.

CHAPTER SIX: RESPONSE

In this chapter we will discuss the responses, by northerners, to the methods discussed in the previous chapter. As we have noted above, reaction to anthropologists across the North has often been negative in the past 20 years. This seems to be more a function of an association of anthropology with the colonial experience, as we have suggested, than with personal experiences, but northerners have used each to explain the other. One of the reasons why this study was undertaken was to find out why anthropologists in particular were the target of frequent accusations, and this chapter relates the responses of Delta residents to this and related questions at the time of fieldwork.

Data for the chapter come from interviews and discussions with Native and non-Native hosts, and from interviews with researchers about responses to their work. In addition, interviews with sponsors, and documents produced by researchers and sponsors, have also been used to some extent. Most of the data focus on the Delta experience, but some comparative information from across the North has been brought in; for instance, the Science Institute conducted a study in 1986-87 with all the major northern Native organizations about their research needs which included comments about the research process in the North (Lange 1987). The conclusions are not substantiated by a statistically representative sample; instead, as has been explained in chapter Five, responses by northern hosts fell into qualitative patterns of interpretation. Since a range of opinion was sought, both from those directly involved in research methods and from those unaware of researcher presence, the data do yield insights into the way that people have conceptualized and patterned their exposure to

social research. An attempt will be made in the discussion to distinguish key variables in response, including degree of direct exposure to methods, a Native or a non-Native background, amount of education, and degree of transience. It should also be noted that the quotations give below are paraphrases taken from detailed notes of interview responses.

The first section specifically responds to the discussion of the previous chapter on conduct: what do hosts think of the various fieldwork rules (as outlined in Chapter One)? The second section is devoted specifically to role-playing, including the dual stance and the question of expertise. The third section attempts to explain comments made on the quality of knowledge obtained by researchers. This is a discussion of research symbolism rather than a direct commentary on the actual content of studies and reports, since few hosts had read or were willing to read them. This section also refers to images, both those held by hosts of researchers and those held by social scientists about themselves. The statements made by researchers about how they perceive themselves to have been received by hosts have been incorporated also throughout the chapter; references and paraphrased information from researchers have been preceded by the term (researcher). One of the primary responses by hosts relates to feedback, communication, and utilization of research results, and this will be treated in the last section; it is especially critical to understanding all aspects of host response to the rules of participation. Utilization by sponsors as a form of research regulation will be discussed in the following chapter.

A central theme emerges throughout the chapter. Hosts have been instrumental in shaping the participatory nature of research; in the two succeeding chapters we will examine likewise the roles of sponsors, public and academic. The pattern of changing modes of investigation summarized in Chapter Five cannot be explained solely from the perspective of social science epistemology. As we have seen from the first two

chapters, methods have everywhere been molded by the dialogue between the anthropologist and his/her teacher: through role, reciprocity, and the mutual manipulation of knowledge and circumstance. This has happened also in the Delta. Northern residents have held strong, if varied, expectations about how researchers must behave to obtain an adequate and valid knowledge about the North, and these expectations have tended to shift over time in conjunction with cultural and political evolution. For example, hosts are demanding more specialized assessments, and more technical expertise from their social investigators, than ever before, and these kinds of expectations have helped to shape models of participation.

In chapter Five, in our discussion of the evolution of participatory methods in the Delta, we discovered that P-O as method was first used by researchers like Stefansson and Jenness early in the century. It became the dominant model of method only in the period of 1960-75, when many researchers used P-O as a model of conduct even when they did not follow all of its rules. In practice, the method must be adapted to suit the setting: the size and composition of the community, the length of stay, the requirements of the sponsoring contract, the type of problem investigated, and the dispositions of the participants. The northern Delta hosts formulated impressions and responses to P-O primarily in this period also; these impressions cannot be separated from their responses to the burgeoning of government bureaucracy and other outside intrusions. As residents, Native and non-Native, created a social milieu which incorporated a north-south flow of personnel, ideas, and attitudes, they began to place responses to anthropologists and other social scientists within the framework of this milieu. As we will see, reactions to the anthropologist came to represent reactions to a series of people coming to the North to find things: jobs, cultural symbols, self-searching, excitement, and knowledge. We have already seen how visitors came as

seekers and participants, and P-O was both an overt and a tacit model for many hosts and guests -- however phrased and understood.

Over the years of research, hosts came to better understand the rules and rationales of research method; as we will see in this and the next chapters, they also came to better understand the outside context of research planning and utilization. Some individuals, particularly those interviewed frequently, began to develop a critical perspective on the appropriateness of conduct. This appropriateness reflected personal sensitivity, which was deemed important, but it also reflected a sense of whether the methods were correct in producing valid, and validated, knowledge. Others continued (at the time of fieldwork) to evaluate research methods by cultural rules, etiquette, and personal behaviour -- is the researcher a nice person behaving in inoffensive fashion? But the majority of northerners, particularly Native residents, simply were unaware of, or apathetic to, northern social science as anything but a symbol or image. We will suggest in this and in the next chapter that non-Native residents have undoubtedly played a crucial role in informing Native people of the nature of methods and the intent of the researchers, as well as of the criteria by which to assess these.

A. RULES OF CONDUCT

Native responses to fieldwork questions showed an appreciation of the following rules of participant observation as described in Chapter One: immersion into daily lives [G], the necessity of establishing rapport [F], the selection of key informants [E], employing reciprocity in money, favours, skills, and goods [I], learning the language [J], time as knowledge [M], role-playing [H], and the necessity of a dual stance [K]. Some realized that fieldwork was an initiation into professionalism, but others saw much of field behaviour as idiosyncratic. The first four rules [A-D], of bounding the unit of study and developing method, were less understood as social science epistemology,

but, as we shall see in discussing responses to the quality of knowledge, some people tended to have an implicit recognition of rule [D], that the researcher is using himself as a research instrument. They realized that the individual researcher is in many ways accountable for the quality of knowledge and product, regardless of social context.

One of the more basic guidelines to the researcher's methodological performance was the rule of time as knowledge. The most frequent negative comment by Native hosts referred succinctly to length of stay, a visible clue by which a range of expectations could be evaluated. Most researchers have not remained more than a few months, and there was a tendency to underestimate the stay for researchers who were not liked. Although people recognized, in some instances, that research styles were changing, they still felt that the short-term consultant could not gain adequate rapport or insight, and thus the quality of the product was de-valued.

Although relatively few of the Native people interviewed recalled individual researchers by name, those whom they did recall, and approve, were on the other hand those who came and stayed for long periods or who kept returning. This was viewed as a commitment to the people, an acceptance of the rules of involvement, and as adequate reciprocity. Time is an investment not only in knowledge of culture, but also in knowledge of the people who present that culture. Only by adequately knowing the people can the scientist investigate the "sociology of knowledge" in knowing whether a particular informant is "representative" of a view accepted by other people. One must stay long enough to get to know the right people, the right teachers, or else the resulting understanding will be flawed. In my own fieldwork, there were frequent hints that I might not be able to assess people properly in a short time.

A researcher can only get to know the people if he goes out on the land at least a year. You need to spend time. Unless a white spends at least a year, he can't write a book. But I [Native person] could write a book in six months. Anthropologists who come for only a short time write crap books (p.c. March 11, 1981).

That fellow [non-Native] that runs things [at one community institution] doesn't mix with the people much, but he's been here a long time. His partner is too big for his britches. He's only been here three or four years (p.c. Feb. 16, 1981).

I [non-Native civil servant] have been here for three years, and only now feel comfortable talking with Native people (p.c. Feb. 2, 1981).

The oil companies have a guy who has been here only a year telling newcomers what Native people are like (p.c. Nov. 17, 1981).

The Native people feel that anthropologists are too quick to judge. There is resentment against those who only stay a few weeks and write a book, although they don't mind if tourists comment. An anthropologist should be able to get rid of old habits of looking at the world (p.c. Feb. 27, 1981).

Most of the people who come here to write books don't stay long enough to know what they are talking about, sometimes only a year or so. They come and they leave: we don't need them (p.c. Oct. 6, 1981).

I've known some whites who have done really rotten things to Native people, but they are trusted because they have been here a long time (p.c. Sept. 21, 1981).

In addition, a short stay is inadequate reciprocity. One cannot "pay one's dues" in social exchange in a brief period (p.c. Dec. 10, 1981). This involves not just payment in material goods, but involvement in friendships, in exchanges of skills and knowledge, and in the whole process of socialization. Researchers reported that they felt better accepted if people realized that they had "paid their own way" and were not dependent upon outside agencies or host hospitality. Specific complaints were related by both Native and non-Native hosts about researchers who had not invested properly in what they got in return; this need not be measured in the amount of "goods" or time, specifically, but in the quality of the interaction -- although, as Lange's (1987) informants noted also, there is an increasing trend in the North to formalize the terms of reciprocity. It is possible that warnings about deception and inadequacy here are perpetuated among the Native people by non-Natives, including social researchers themselves. We will return to this process.

One guy would buy oranges at the Bay and pretend that he could not afford more so that people would ask him to dinner; he was really from a rich family (researcher p.c. Sept. 21, 1981).

One guy came into our community about 7 or 8 years ago, from the States, I think, and started inviting people over to drink, acting like a jolly good friend, yet only left to make a fast buck. He didn't go to the Council or anything, just wandered in - people told him what he wanted (p.c. Feb. 4, 1981).

I've heard stories about those who have been here before me and ruined it all. One fellow paid only \$2 to people to fill out these long questionnaires; that was hardly enough to buy milk (researcher p.c. Oct. 6, 1982).

As we noted in our general discussion of fieldwork practice, hosts often made a distinction among researchers between those who are perceived to have some power on the outside and those who do not have much influence; we will return to this below. This was true in the Delta as well. All participants, including the researcher, are believed to manipulate the field circumstances through reciprocity. One of the objectives of reciprocity, however, is the creation of the appearance of symbolic equality, for without it communication falters along with the entire dual stance of the investigator as interpreter. The violation of the rules of reciprocity, by which manipulation by power becomes overt, then, leaves a negative image of method.

Other expectations also emerged from responses. A few people mentioned the importance of learning a Native language, although this comment related more frequently to feedback than to method. One man, in speaking of his elder mother, said that she and other elders were afraid to speak in English, for, not knowing it well, they did not want to be embarrassed or misunderstood (p.c. Nov. 17, 1981). Older people may avoid researchers for this reason. At least one researcher from the 1960-75 period, one who also stayed more than a year, was praised for trying to learn a Native language, not just because it would increase his understanding, but because it demonstrated a commitment from him to the rules of social interaction, including reciprocity (Nov. 8, 1981).

He liked people; he was good to us. He came around to visit and learned the language fast. We learned from him, too (p.c. Feb. 18, 1981).

You've got to learn the language, or at least use interpreters, to talk to the old people (p.c. Nov.17, 1981).

Language learning means also that the people have added opportunity to socialize their guest. Similarly, an older Inuit man criticized a popular writer who had written fictional books about the north for not writing much truth in spite of knowing the language; this was an abuse of privilege and insight (p.c. Feb. 24, 1981).

Several people pointed also to the tendency for researchers, especially those resident for a limited time, to call on a core of key people. Both Native and non-Native hosts commented on this. Some accepted it, as there is some agreement about who the best sources of information are, and, as Native hosts noted, it relieves everyone else from the "burden" of being singled out and interviewed (p.c. Nov. 17, 1981; Sept. 23, 1981; Sept. 25, 1981; Jan. 27, 1981). We will return to this in discussing the quality of knowledge. The credibility of the informant is at least as important to hosts as to guests, and knowing the right person is, as has been suggested, a criterion of success. In my own experience, people frequently requested the names of those who had previously been interviewed; they hoped they could then correct or corroborate the version of the answers I had been given to my questions.

The other rules noted above, rapport and learning the rules of culture through involvement, were generally expressed by Native and non-Native hosts as approval of those few investigators who lived with or like the people. Those who lived on the land, learning much-valued bush culture, and who accepted people and customs at their preferred value, were praised as they were for reciprocity and language learning.

He was single then, and that made it easier to fit in, and he lived right out in the bush. Another one who came, too, was the same; both were easy going and relaxed around people, and visited people in their homes. People knew they were anthropologists, but it didn't have the stigma it does now (p.c. Nov. 9, 1981).

I remember two students who came. They both stayed about six months, and I was interviewed a couple of times. They went in the bush with us, or stayed around town just getting to know people, not asking direct questions (p.c. Nov. 3, 1981).

Two Native women who had known a couple of anthropologists expressed it as, "They lived with us, doing what we do, and we all helped each other, unlike today, when no one helps each other" (p.c. Oct. 21, 1981; see also Feb. 24, 1981; Feb. 18, 1981; Oct. 27, 1981; Nov. 4, 1981). Thus this kind of interpersonal participation is idealized in terms of traditional norms of community stasis. The high value placed upon cooperation, acceptance, trust, and sharing represents the images of a cherished traditional life best achieved in bush settings. It represents, symbolically, the total participation in the whole of the small society, much as P-O does. Thus these methods are elevated as models for outsiders who want to come to know the people, even as models for Native people themselves seeking revitalization through tradition.

Delta people, like social scientists, recognize that this is not easily achieved in the urbanizing community where roles like that of the "expert" have become specialized. They urge visitors nonetheless to be relaxed, accepting, sincere, and to live like the people even when they cannot live with them. For instance, a researcher who brings his family and lives within the community, following the same general routines as anyone else, or who is single and becomes partially adopted into a local family, is accepted as a part of the community. No one expects researchers to fully participate, but to show some interest and commitment. I was often given advice on rapport in either overt or veiled form, and from both Native and non-Native residents; the latter were openly instructing me on how to "get along with the Native people."

Just act yourself, not higher or lower than other people (p.c. Feb. 6, 1981).

We get along best with those that take the time to get to know us before they ask questions (p.c. Nov. 17, 1981).

It takes a long time to gain their [Native people] trust; it is easier if you go out on the land with them. They are more open, friendlier there. Don't thank them, just go on and do your work (p.c. Sept. 25, 1981).

I like for people to have patience, to communicate with me even though I am only a professor of land, of snow and weather and sea (p.c. Feb. 16, 1981).

We like the ones who trust us, who are open to what we say (p.c. Feb. 24a, 1981).

You have to prove yourself, and just listen and learn things in the beginning. Don't apologize for who you are (p.c. Feb. 2, 1981).

Native people were often more subtle than non-Native hosts in their instructions, couching advice in comments about other people, but they were nonetheless cautious in actually arranging a live-in situation. Living with a Native or northern family is still sometimes possible, although pressures of money, lack of space, and uncertainty make this difficult. Until a suitable period of mutual acquaintance and testing had passed, they apparently felt more comfortable in having the researcher stay in researchers' quarters (see also p.c. Feb. 25, 1981; Dec. 4, 1981; Oct. 6, 1981; Feb. 24, 1981; Oct. 13, 1981; Feb. 20, 1981).

Attitude is, again, part of the key: does the outsider show a willingness to live like the people, to be sensitive to them, to listen to them, and to accept?

We like it when the scientist learns the language, tries to live in the bush, learns the right way, and has the right temperament and patience (p.c. Feb. 20, 1981).

All of these are qualifiers for validity, as we will see. "You must come live like us, and then you will see life as we see it," said a young Native woman doing some investigation of her own (p.c. Oct. 29, 1981). Again, there must be the appearance of equality, if only symbolically. "Trust," said one Native man, "is most important. Even if you have not been here long, if we trust you, that is okay" (p.c. Nov. 17, 1981). Thus following the rules of the culture, getting involved in either daily reciprocity (living with them) or in local activities (living like them) can lead to a validation of results which can overcome the persuasive criterion of time as knowledge. Another Native man said:

Even though it takes time to get to know people, we tend to size up the person on a first impression, and we know whether we like his attitude (p.c. Feb. 16, 1981).

B. ROLES AS RESPONSE: THE DUAL STANCE

In the preceding chapter we discussed the importance of role as method in the Delta. Some attention was paid there to the importance of the reactions of hosts in shaping roles, including the assignment of roles and the association of restrictive expectations and negative imagery with the role of the "anthropologist" or researcher. Responses of hosts have played a large part, then, in the formation of the researcher role, and in the latitude accorded individual researchers. Because of the expectations associated with role, evaluation of role-playing by hosts has been cautious. We have seen how some roles, such as that of student or teacher, have been better accepted in relation to P-O than have roles such as administrator or spy.

The most prevalent response to role by many Native people at the time of fieldwork was the desire to avoid the obvious confinements of role as interference in interpersonal relations. Researchers were in part most resented when they acted as they thought they were expected to in terms of task definitions rather than "from the heart." Role-playing was thought to imply artifice; rapport should instead be built by personality and character, with no pretense that the outsider was not what he seemed to be. Friendship was taken as a state of trust, not as an assumption.

You must be the same with everyone, no punches pulled. I have seen anthropologists pull a deference act to Native people; they are aware when an act is being put on (researcher p.c. Oct. 6a, 1982).

I don't like people to pretend that they know more than me. They come here and get big brains, forgetting we are their teachers. We take them out of kindergarten, out of the Lab, and show them around (p.c. Feb.16, 1981).

People here are very self-conscious, nervous about being singled out and asked to play a role, that of expert. They don't want people coming in and playing expert; you can pick out that type of person (p.c. Feb. 9, 1981).

We [Inuit] are afraid of those who fit successfully in both worlds; it used to be more clear cut who people were, less role playing (p.c. Jan. 28, 1981).

I remember he came to visit us in the evening, really friendly, and liked people for who they were (p.c. Feb. 18, 1981).

People are put off by white role-playing. White people wear uniforms. It is better if Native people do their own research. They don't have to pretend to be what they are not (p.c. Oct. 29, 1981).

The best research situation would be for you to do research in secret, and just have ordinary conversations. Then they [Native people] would not be put off by your role (p.c. Oct. 7, 1981).

It doesn't matter just what profession you are in. People judge the truth of what you say by how you got along with the people. I have helped out lots of researchers and never gotten anything in return (p.c. Feb. 5b, 1981).

One of the most intense negative reactions, implied in the discussion above, is stimulated by betrayal of trust; we noted this in Chapter Five and in the discussions above. We will further discuss the issue of role when we discuss truth and the quality of knowledge in the next section. This can happen through harmful manipulations of reciprocity, as above, or it can happen through the reporting of false or untrue information. This latter problem has arisen more with journalists and fiction writers, since more people have read their writing than they have anthropological reports and publications; we will return to this below. But an attempt to associate oneself with an identifiable role which cannot be readily justified by P-O models -- such as association with government bureaucracy -- violates the rules of living with or like the people.

Rule [K] concerns the dual insider/outsider stance, and all of the above applies to this rule as well. Researchers are expected by Native people to have some outside training and expertise which they are applying to the local context. Otherwise they would not be there, learning and collecting information; otherwise they would not be expected to be interpreters, to make use of the information, or to be connected with the

symbols of power. One articulate Native woman, who had spent years in southern Canada, commented:

We can tell things to an outsider that we cannot tell each other; we are too close to each other, and an outsider can give us some insight about things we do not see about ourselves (p.c. Jan. 28, 1981).

Another Native woman also said:

I used to resent outsiders doing research, but now that I've tried some of it myself I can see that sometimes outsiders can help people see themselves (p.c. March 10, 1981).

The outside stance can sometimes lend an external legitimacy to internal belief, then. It can also abrogate that belief if the researcher is not sensitive to local interpretations of role as method: the outsider must bring his/her expertise but must use it in a "correct" fashion. Native people are wary of an outsider who plays, openly, the role of the expert without meeting the criteria outlined above. We will return to this rule in the next sub-section, but it is worth stating here that the Native hosts were keenly aware that an investigator, in playing his roles outside the North, must not transfer them to the northern community without adaptation into a state of apparent role-less-ness. Only this role-less-ness, which signals a liminal ability to shed old expectations and assume new ones within the Native community, can lend a balanced validity to the process of expertise. Only by knowing how the people "really live" can the researcher gain control over his comprehension of social reality.

Whereas Native people were willing to resolve the insider-outsider contradiction by rejecting the restrictions of overt role-playing, non-Native residents were more likely to be openly critical of the contradiction and to see the resolution in a rigid adherence to role. These individuals were more likely to ask the writer, "What do you see about us that we do not see?" They expected the outsider to contribute a perspective that they did not have themselves. Consultants were hired, in their view, for specific

tasks requiring the expertise of the trained social scientist, such as the ability to quickly assess and evaluate a social or economic situation. There was a general respect for education and expertise; many non-Native northerners, especially those who have come north to work for the governments, have extensive post-secondary education themselves.

The consultant, or the panel member, is not expected to have a good understanding of an insider's knowledge; as we saw in Chapter 5, the panel might use substitutes, such as public testimonies at hearings, to achieve this. A researcher who spends a longer time in the community, is, however, expected by non-Native residents to attain a better understanding. In the formative period of 1960-75 the non-Native person also developed a response to the investigator, often negative, which focussed on the participating anthropologist. There was fear that the anthropologist would get to know the people too well and would facilitate agitation and criticism against the government. The anthropologist is outside the immediate control of most agencies, and is not therefore to be trusted in his/her conclusions. Rapport may lead to advocacy, to criticism, and to public censure. Anthropologists were themselves sometimes censured for getting too close to the people, or too inquisitive about government affairs. Several researchers reported having encountered this reaction, especially when they began to ask direct questions of local officials about the effects of programs on local residents.

The administrators saw anthropologists as snooping, manipulative. Anthropologists don't keep a proper distance from the Native people; they don't fit neatly enough into the northern caste system (researcher p.c. July 19b, 1982).

The white people thought I was mad for living in the Native community. They were threatened; they thought I might get information which would expose their mistakes (researcher p.c. May 11, 1982).

I had trouble with the whites, too. The HBC manager had heard of anthropologists and knew that they asked questions and wrote books. The RCMP officer gave me trouble, and tried to tell me what I should learn, to define my role as anthropologist (researcher p.c. May 9, 1981).

The young whites felt threatened and upset by what I was doing. The Native people either cooperated with me or ignored me (researcher p.c. Oct. 6a, 1982).

At the time of fieldwork in 1981, non-Native people frequently expressed admiration for researchers who got to know the people, and they supported in theory the model of total participation. There were still criticisms, nonetheless, of this kind of insider knowledge, generally expressed as a critique of the result rather than of the method per se. There was a lingering ambivalence, and a sense of threat, about the dual stance. Several civil servants said that they found anthropological reports to be too "subjective." They lacked the objectivity of a trained scientific stance, were not quantitative enough, and were therefore not practically useful for administrators looking for quick objective facts. These individuals felt that, in order to make anthropological results useful, they would personally have to re-interpret the reports themselves with a less biased view, and that this was rarely worth the trouble: the result might only be, as suggested above, that the civil servant would arrive at the knowledge he already had.

I don't use social science reports. I get them, but only occasionally do I look at them. And when I do I find that they don't tell me anything I do not already know. Research should be objective, action-oriented. Some of the studies rely too much on the subjective judgements of the people interviewed (p.c. Feb. 9, 1981).

I usually avoid social scientists when they come here. Some of the studies are okay, if they are not done by instant experts. The government ignores the studies anyway, just like it does the lessons of experience (p.c. Oct. 7, 1981).

We don't need more studies to tell us the problems. We have had some teachers who did studies, but sometimes they treated students like pieces of data...We need objective studies, but also ones where people stay long enough to get to know the people well. Some long-time residents know the people and their values better than a researcher would and even we don't know how they think (p.c. Feb. 25, 1981).

There's not much difference between social scientists and other government tourists. Social science too loose, not rigorous enough. Need more hypothesis testing, not just the odd statistic here and there (p.c. Feb. 5, 1981).

I call the social sciences semi-sciences. They are too soft; they rely too much on culture rather than on fact (p.c. Jan. 23, 1981).

I'd like to know what you find out about the Native people. Sometimes the outsider can give a more objective opinion, but really research should be done by the people themselves. It takes a long time for a newcomer to learn the ropes (p.c. Sept. 25, 1981).

I don't read most of the research that crosses my desk in social services. Most research is subjective, and a formality. I can predict what people will say when interviewed about particular topics. Anthropological reports are too nebulous, too general, based on limited visits (p.c. Feb. 17, 1981).

By following this secondary interpretation to its logical conclusions, one can discover a hidden, or not-so-hidden, message. The perceptive non-Native bureaucrat, performing his duties, can learn what he needs to know from experience, while retaining an outside perspective. On the other hand, there is recognition that the participating investigator may achieve a rapport and insight on a more fundamental level. Hence the ambivalent envy/disdain toward the social science researcher who may prejudice his dual stance by overinvolvement in role-lessness. The more the administrator, or other non-Native person in a position of authority, perceives the researcher to be abiding by the rules of role, the more faith she has in the results and the more secure she is that the conclusions of the researcher will not be antithetical to administrative progress.

You have to stay a while with the people. You can't come to conclusions in just a short period of time. After three years I am just getting comfortable with the people. You have to listen and observe (p.c. Feb. 2, 1981).

You must be careful not to ask questions about sensitive things, to offend people. They don't like being put under a magnifying glass. Be careful what you do, but be yourself (p.c. Feb. 6, 1981).

I stay away from anthropologists in smaller communities. I do not want to be associated with them. I am more practical in orientation. Anthropologists treat the north like a crystal ball, but they never find anything (p.c. Feb. 19, 1981).

The above ambivalence relates also to a professed desire to protect Native people from prying outsiders; the researcher who gets too close to the people, and who tries to know too much, is prying where he does not belong, and negating his/her proper position in the community -- again a resort to role (researcher p.c. May 11, 1982; Dec. 7, 1982).

In one community the priest was telling the Indians to reject the researcher, that the anthropologist would betray them as an agent of imperialism (researcher Feb. 9, 1982 [paraphrase from lecture]).

One researcher who was going across the Arctic to survey communities was told by the administrator in one community to "keep his nose clean." The fellow assumed that the anthropologist was going to study the Native people, and told him to be careful but to let the administration know about the "savage" practices in the Native village so that they would better know how to control the people (researcher p.c. May 19, 1982).

It is difficult to analyze reactions here to determine whether this "protection" was not often an attempt to protect the non-Native person's own position, as discussed above, especially since the non-Native person's position has historically been one of "role model" and "socializer." Hugh Brody (1975: 71-73) has written about such criticisms encountered in his fieldwork in Pond Inlet in the 1970s.

Particularly in the case of social anthropologists, social scientists tend to stay comparatively long periods in one community, and they set out to discover details of social life that are usually not apparent to the settlement Whites themselves. The arriving social scientist has no clear relationship to any of the established institutions, and therefore he cannot be expected to take any predictable position in the social life of the community. His location in the informal hierarchy is as uncertain as his dependence upon the established agencies. Two difficulties thus arise automatically. First, resident Whites become anxious about which circle of friends and colleagues (which clique) is going to adopt this newcomer...Secondly, and far more importantly, resident Whites know that social scientists regard full and easy relations with Eskimos as the sine qua non of their work and are therefore unlikely to pay much, if any, attention to prevailing conventions about the limits of inter-ethnic contact...Obviously, anthropological field methods oppose frontiers between Whites and Eskimos and cannot be limited by how agency Whites see their roles as socializers of the Eskimos (Brody 1975: 71-2; emphasis in original).

The protectionist stance has a history of evidence in the Delta which continued through the time of fieldwork. A young anthropologist in a Delta community in the early 1960s,

for example, was censured by the non-Native population for becoming friendly with young Native men and women. The anthropologist in question drank and partied with the youth, and the non-Native people felt that he was taking advantage of them, as well as setting bad role models for deportment (PAC, RG85, vol, 1656, file NR2/3-3). Other examples have also appeared in the records. Said one officer in 1957, "The Eskimo must be protected from overstudy by unqualified people...we must help them evaluate unfamiliar demands" (PAC, RG85, vol. 492, file 530-136-1 pt. 12). This, too, has formed a rationale for regulation, as we will see in the next two chapters.

Critique has also centered on other aspects of role, rule, and reciprocity. The non-Native people have consistently rejected investigators who have taken private information and have misused it, not giving anything back. They have interpreted friendship as a role rather than as an attempt to overcome role; they have felt that for a researcher to take the role of friend, only to desert it to revert to the role of "government employee" and to use data accordingly was a betrayal of the original role. Similarly, although they realized the importance of rapport and acceptance, often seeking it in the Native community themselves, non-Native hosts were less concerned with personal accountability through rapport than with a set of standards about what is good for Native people. Thus several of these hosts interviewed during fieldwork assumed that the writer was there to study Native people, regardless of protestations otherwise, and interview responses were often phrased as warnings not to abuse the faith of the Native people. This protectionist role is slowly changing as Native people achieve more power in the northern community and redefine the rules of participation in relation to rules of culture, objectivity, relativity, and validity. To the extent that P-O rules are echoes of more general ideals of trust and reciprocity, then the dual stance must evolve beyond marginality to dialogue.

C. THE QUALITY OF KNOWLEDGE

Throughout the preceding sections we have seen that an evaluation of the quality of knowledge is implicit in host response to the methods and results of research. This quality is expressed as legitimacy, power, representation, investment, accountability, and objectivity. In this section, these concepts will be explored as aspects of the assessment of cultural knowledge and its creation.

It must be stated that few responses about the quality of knowledge were based directly on an acquaintance with social science writings about the Delta. Most of the civil servants interviewed said that they rarely read reports that crossed their desks, as stated in the previous section. In addition, there were problems of accessibility for many documents, as well as a pervasive apathy about results. A few long-term non-Native residents, and some Native hosts, had read reports, primarily if they had acquaintance with the writers. Although I experimented with asking interview respondents to read excerpts, there was a reluctance to do so, since this was a time-consuming process that few people were interested in. There was also a danger that such excerpts would be misunderstood out of context. Many reports dating back to the 1960s were available to the public through the Research Lab library; a small selection could also be found in the public library. Occasionally a better-known work, such as the volume produced by the Honigmanns (1970), was used in the schools; and some individuals -- again most likely those who knew the writers -- had copies of northern social science and fiction volumes in their homes.

Less than a fourth of the respondents, then, had read social science reports themselves, then. Newspaper and magazine articles and popular books were read more frequently. Several people commented that Native people in general were less likely to read, having had little encouragement to do so, although a few did read extensively. In

general, most public impressions, Native and non-Native, about the quality of published writings about the Delta or North have come from the reading of popular works. Even if an individual has not read these volumes him or herself, he or she may have formed opinions from hearsay. What is of particular interest here are the criteria by which the quality of knowledge is assessed. These tend to be used to evaluate all outsiders who come North and write about it, although to differing degrees. These criteria often relate to method, so that if an individual does not read but is familiar with how the books/articles are researched and written, ideas are still formed about the ensuing quality of knowledge.

Tourists, journalists, and anthropologists are three categories of visitors. All are assessed by similar general guidelines: length of time spent in the North, attitude, degree of rapport, honesty, friendship, reciprocity. Tourists are tolerated, and are often a source of interest and amusement. The Inuvik newspaper has frequently carried reports written by or about visitors from distant places. The written impressions of these tourists are read with interest, especially if they mention local people, but they are not, it seems, taken very seriously unless it is felt that publication elsewhere of misleading reports will negatively impact outsiders' images of the North. Tourists are not expected to stay long, or to know much, and as long as their writings do not pretend to false expertise they are not given much credence by either Native or non-Native residents (p.c. Nov. 17, 1981; Oct. 27, 1981; Oct. 8, 1981; Oct. 16, 1981).

The popular writer/journalist (such as Duncan Pryde and Farley Mowat) and the anthropologist face higher expectations, respectively. The more intensive and long-lasting the involvement with the people, and the more the visitor is thought to bring prior expertise to the Delta, the greater the expectations about what will be written. Although people are sometimes unsure about just how they would expect a writer and a social scientist to differ, they do know that a social scientist was likely to go North

because of what he knows and can contribute, as well as because he wants to learn. The researcher is associated with institutions of government and higher learning. A scientist could bring more benefits, or more harm, than could a journalist or fiction writer. The higher the expectations, the greater the criticisms when hosts felt that the researcher was not living up to them.

Even when methods were imperfectly understood, hosts felt that the critical differences between the two types of writers were related to method. A researcher who fulfilled the conditions of rapport and investment, for example, would be expected to write an accurate account, an interesting one, but also an account with greater evidence of access to the "reality" of the Native North than would be produced by a popular writer. Even though some writers have spent extensive periods in the North, and are thought to have known people well, it is realized that a non-scientist would be less likely to employ self-conscious and systematic method toward cultural comprehension. The more understanding the host has of social science process, the more methods and results are separated in terms of validity. For Native people who yet have limited experience with researchers, the separation is often not made. In addition, one must consider some cultural differences in the way quality of knowledge is evaluated; the Native person may have a view of objective reality somewhat different from that of the non-Native. As will be discussed below, this should be considered in interpreting responses, but cautiously, as it is difficult to sort out traditional themes in a modern context.

One criterion which is closely tied to method concerns invasion of privacy and the asking of direct, personal questions by anthropologists. As R. Janes (1982) states in his overview of northern archaeology, archaeologists and anthropologists are grouped together by northerners as people who exploit privacy for personal gain: "Whether or not specific incidents created this impression is not as important as the belief that they did" (Janes 1982:7). Several criticisms underlie the statements made by hosts about

privacy. Direct questions are not only considered aggressive by Native people, but they circumvent the normal process of social relations (Nelson 1969: 392; p.c. Oct. 29, 1981; Oct. 13, 1981; Oct. 27, 1981; Feb. 9, 1981; Feb. 16, 1981). A few Native people responded to me by refusing to answer any questions that they considered too aggressive or misdirected, and non-Native people mentioned this as part of the necessary protection of Native informants.

Anthropologists are not well-received around here; they put people under a magnifying glass (p.c. Feb. 6, 1981).

Anthropologists are more disliked than other researchers because they are the most intimate. People don't appreciate the science value of being placed under a microscope (p.c. Feb. 10, 1981).

Not all social scientists have asked personal questions early in their relationships, but some reportedly have done so. What is more important is the lack of respect implied by asking questions about personal knowledge and accountability, without a reciprocal investment in the relationship. This need not be monetary, but it should follow the dictates of reciprocity and apparent equality described above in the first section.

Loss of respect is loss of control. Certainly people are concerned about the publication of private information about themselves without their consent. They are also concerned that a direct, inquisitive mode of investigation, based on limited interpersonal knowledge, deprives them of normal channels to manipulate and control impressions, again part of reciprocity. The researcher has no inherent right to ask those questions, and to do so implies lack of courtesy and respect as well as potential loss of integrity and information. Researchers who are not familiar with local standards of credibility, and who do not know the community well enough yet to compare version for truth value, are subject to manipulation.

One of the common responses, then, was an attempt to regain control: the "testing" of researchers. Informants have described various means to test an outsider as

to his/her comprehension of the Native social situation, his understanding of the rules of etiquette, and his general attitude. Several Native men admitted to the game of fooling the anthropologist, which is, according to fieldwork literature, a world-wide amusement.

Information which was known to be wrong, even ridiculous, was given to these visitors.

I remember an older man and his daughter coming through Reindeer Station when I was younger and asking embarrassing questions about our sex life...We began to make sport of these people, having their guides teach them words of greeting that were really obscene, and telling them lies (p.c. Feb. 9, 1981).

Some of those people were so stupid. They looked so stupid; they could not do anything on the land. I used to tease them about what they didn't know; I would do things like try to cut bannock with an axe. They did not like that. I would take them out and pretend to be lost (p.c. Feb. 16, 1981).

If we [Native people] don't know or like a researcher, we will bullshit them (p.c. Nov. 17, 1981).

Researchers also admitted in interview that they had experienced such tests -- or thought they had.

Once the man I was with pretended to be lost when we were out on the land. This had a lot of influence on me. In fieldwork you feel so vulnerable, having to say and do things that you would rather not (researcher p.c. May 10, 1982).

I found that they like to tease anthropologists particularly, because they see them as gullible. They told one young woman a few years ago that they all wanted to be garbage collectors (researcher p.c. May 11b, 1981).

On one of my first field trips the Inuit didn't know what to make of me. They treated me like a child, and when I tried to interview them, they could not accept it; they told me what they thought I needed to know (researcher p.c. May 9, 1981).

Not only are lies told to get rid of the anthropologist, in the bar or in the camp, but tricks are played to dismay them: a seal flipper resembling a human hand sticks out of a pot; a disgusting bit of food is offered as a delicacy; physical tests of skill and endurance on the land are set up (Dyson 1979: 58-9; Oct. 13, 1981; Feb. 16, 1981; Feb. 4, 1981; Oct. 4, 1981).

The invasion of privacy, and the attempt to deflect it or to attain control over the communication process in response, are integral to validation of knowledge through

interpersonal relationships. Responses obtained under circumstances lacking in depth and comprehension, where perhaps the researcher does not know enough about the informant to evaluate the nature of the answers to his questions, are suspect to the host as well as to the social scientist. If the investigator is gullible, if the host prevaricates, or if the researcher takes private information out of context on short acquaintance, then the validity of results is questionable. Loss of this privacy is a loss of control over impressions and insights which must be avoided or regained through the manipulations of field reciprocity.

In the first section of this chapter some of the rules of deportment in the field were described from hosts' perspectives, such as the importance of rapport, of attitude, of time spent with people. We noted that although the amount of time a researcher has spent with his hosts is a very visible marker, time itself is not fulfillment. Time implies the development of trust, rapport, and friendship; returning to the field is as good or better than a single long stay because it denotes a desire to renew one's investments. And time can be bypassed by attitude, by learning a language, or by effective communication (p.c. Jan. 28, 1981; Sept. 29, 1981; Oct. 21, 1981; Nov. 17, 1981; Nov. 5, 1981; Feb. 24, 1981; Feb. 25, 1981; March 3, 1981).

We judge the truth of the reports we get back by how long the person was here, but also how he got along. It doesn't matter what profession he is in. I have worked for the Lab and have had bad experiences with know-it-alls who don't know what they are writing about (p.c. Feb. 5, 1981).

An investment in a relationship is needed to validate the knowledge derived from it, as argued, and this investment is created by certain host criteria. The understanding which comes from living with the people is validated because the criterion of commitment has been met. Thus investment and commitment are key phrases, as is accountability. Knowledge is given legitimacy through people, through relationships, especially to the

Native person who appraises reality in these personal terms. This process is important to the non-Native "frontier" as well, since the outsider must prove him/herself in the code of the evolving social milieu.

One of the remarks made most frequently by hosts concerned the nature of informants themselves. We have argued this point throughout this and the preceding chapters: key informants are used in P-O, in surveys, and in hearings. People want to know, "Whom did you talk to? Who told you that?" A Native woman says of a man she knows who is telling stories on the radio, "He has never known what he is talking about" (p.c. Oct. 3, 1981). Another woman says, "Don't believe what you hear at meetings. People go out and get drunk and just say stupid things. It's embarrassing" (p.c. March 6, 1981). "Some people set themselves to represent us to the government when they don't really know more" (p.c. Oct. 21, 1981). "Some people use language better, and remember the past better" (p.c. Oct. 27, 1981). Native people said of each other what they said of researchers and writers: some know more than others; some have legitimacy as teachers and some do not; some are honest, and some pretend to know what they do not (p.c. March 11, 1981; Nov. 17, 1981; Jan. 28, 1981).

Not everyone knows the truth. The Native organizations sometimes hire fieldworkers who are not honest. They mislead people when they talk to them (p.c. Feb. 24, 1981).

This book [by a Native northerner] is full of lies and dirty. It is not true what he said about people, and he should not have named names (p.c. Feb. 25, 1981).

Native people acting as consultants can be just as bad as whites. They think they know it all. They don't always help the people (p.c. March 10, 1981).

Some hosts, Native and non-Native, suggested that Native interviewers would have greater legitimacy than would outside interviewers, who can be used as scapegoats for breaches of truth (p.c. Dec. 10, 1981; Dec. 7, 1981).³³

I would like to do my own research on culture loss; I am one of the people; I would understand better (p.c. Feb. 4, 1981).

I want to write a book about my childhood, but maybe this is better done after I'm gone, because I've already offended too many people. More research should be done locally, by the people (p.c. March 10, 1981).

But it was also clear that they, too, would be viewed by the same values of commitment and validity, even if to different degrees (p.c. Sept. 30, 1981; Jan. 28, 1981; Feb. 2, 1981; Feb. 12, 1981). A Native man criticized the fieldworkers from a Native organization for not being representative, and for talking only to women; he questioned the balance of the results (p.c. Feb. 24, 1981).

Honesty is another related criterion, then: honest methods yield honest results. We have already mentioned the Native rejection of pretence. Not only should one be honest about what one knows, but about how one knows it, and who one learns it from. Several people mentioned a fear that anthropologists were working for the government and not admitting it. The "government" was used as a metaphor for all of the administration which was imposed upon the people and which they regarded as fundamentally dishonest in methods and intentions. The "government" was a facade of pretense to people who never knew when "it" meant what it said (p.c. Oct. 30, 1981; Nov. 12, 1981; Nov. 3, 1981). One Indian woman said:

If you had said that you were working for the government I would not talk to you. The government says they don't have any money but they they send more researchers here to keep asking the same questions. It's not really the anthropologists we mind, but the government (p.c. Nov. 12, 1981).

Again, the importance of trust and open communication about methods was highlighted. A Native man agreed, asking if I was working for the government:

What does it all come down to? Studying Native people again for the government? (p.c. Oct. 30, 1981).

Another Native man from the same community expressed scepticism about researchers from Ottawa:

Only those in Ottawa can afford to see the long-term, objective view. We're too concerned with everyday living to care what people say about us. Only outsiders

have curiosity about us. We care about each other, but do not ask questions like the government does (p.c. Nov. 3, 1981).

Honesty is intimate, just as is much knowledge. Another "dishonest" behaviour, attributed to popular writers particularly, is very fundamental to this intimacy. Just as people are accountable for what they say; just as knowledge resides with the individual and this accountability; so one person should never speak for another. One should not take anyone else's private information and make an object of it, distort it, or betray it. This is of course related to the issue of privacy, and to that of interpersonal investment, and it is again something that the "government" frequently does. It means that one is breaching a commitment, and depriving an individual of control over what he knows and says. As a northern journalist said to the writer, "People don't like having what they say edited, and taken out of proportion" (p.c. Sept. 23, 1981).

A couple of popular writers were criticized by several Native residents for publicizing information taken from confidence, particularly concerning sexual activities. Not only was the information labelled dishonest, it was also labelled "false." It was taken from one situation and placed incorrectly in another, according to Native hosts.

This writer was dishonest. He knew the language and the people well, and he married a half-Indian girl as well. She got tired of his lack of honesty (p.c. Feb. 24, 1981).

Books should be checked for truth before they are printed. They should write about what they learn, not what they think (p.c. Feb. 25, 1981).

He learned to speak the language, and he fit in well, but I'm not sure how much of what he wrote is lies and how much is fiction (p.c. Feb. 20, 1981).

People are reluctant to give reports. Like the CBC, who knows if what is said on the radio is the truth of what we said (p.c. Oct. 30, 1981).

Several anthropologists interviewed mentioned the sensitivity of taking participation too far into the private sphere, particularly in regard to sexually intimate involvements.

This can easily jeopardize research, since it can lead to allegations of misuse and misunderstanding of commitments for open communication (researchers p.c. Oct. 6, 1982; May 10, 1982; Dec. 4, 1981).

All of these factors affect the computation of legitimacy. The perspectives of Native people have been emphasized, because they are most often the primary subjects of research, and their values, at the time of fieldwork, expressed a different view of the genesis of anthropological understanding than that generally held by non-Natives. Objectivity for them is part of the intersubjective experience, and, because of that, it is paramount that methods of knowledge acquisition be visible. It is when the host does not know what the researcher is doing, cannot interpret the researcher's behaviour, or feels he is being deceived that he claims that anthropological knowledge must be invalid, even if the host has not read the reports. Without visibility, it is impossible to assign and evaluate the criteria of legitimacy. The work of a Native organization in research may be more legitimate not just because it is Native but because it is known; if the fieldworkers or the project are not known, they can encounter some of the same problems that outsiders do.

Evaluations of method and result are related to the hosts' vision of the constitution of reality. Non-Native hosts tended to interpret reality, on the whole, with a positivist bent: there was only one set of objective truths to be discovered about the North. A good objective observer should see this more readily than others, especially if he follows the rules of method and role, but reality is accessible to all -- hence a good journalist, also following rules of intimacy and honesty, can approach that reality even though he may lack the "objective" science training. Thus, in criticizing an investigator, they need only say, "I could have seen that just as well as he did." We have already discussed the bias of the civil servant toward "objective" reporting.

It is difficult to label the Native perspective on truth as monistic or dualistic since these oppositions come in essence from a Western view of ontology and epistemology. There is an objective reality to be observed by the hunter, the individual adapting to the natural world. This reality is not necessarily divided into the natural, physical, and social realms accepted by science. Hugh Brody says of the Athapaskan Beaver Indians with whom he did fieldwork that there is a preoccupation with literal truth, and an intolerance of mistakes in judgment (1981: 175). Some of the Native people who spoke with the writer revealed themselves to be uncomfortable with accepting ambiguous questions and answers on experiences, preferring to comment on those aspects of experience which could be described in a literal fashion. But people can experience this reality in different ways for different activities -- ways that can have meaning for the individual. The hunter and the spiritual leader have access to a world, a reality, which is not often seen but can be communicated, understood, in certain conditions, and which has as much empirical validity as what can be "seen." But this validity is recognized from the perspective of the person who knows it. People are reluctant to impose truth, or judgment, on others, and similarly, they are reluctant to accept the judgement of others, especially if there is not a "knowable" way that experience can be understood, or shared. Recent insight into the use of Native knowledge about the environment has suggested that reality is not seen as hierarchical and determinate. Variation in observed behaviours or phenomena is not used by the Native observer to confirm or disprove hypotheses, but is accepted as natural and plausible. Each observer's assessments have validity in relation to his/her methods, not in relation to independent guidelines (see Freeman 1983; Gunn et. al. 1988; McDonald 1988; Berkes 1988).

This view of reality and observation reveals why the methods of the researcher need to be visible and understood, and why it is important to identify sources of

information. When the hosts criticized the way outsiders portrayed northern realities, they emphasized the power of interpretation and meaning. Who gave them that knowledge; who were their teachers; what kinds of personal relationships yielded or denied validity?

To say that the researcher deals with intimate material, then, means that he works with personal experience, asking hosts about their histories, their opinions, and their specialized knowledge. The more involved the anthropologist gets in the activities of the community, the more the symbolic properties of knowledge come under scrutiny, because knowledge is considered to be personal meaning and experience, especially to the Native people of the Delta. Jamake Highwater (1981) has attempted to interpret a generic Indian perspective on reality and image. His book is handicapped by a failure to distinguish adequately between historic and modern traditions, but Highwater makes some general assertions which affirm the perspective apparently held by Delta Native people: "We must learn to use our minds to discover meaning rather than truth, and we must come to recognize that a variety of meanings and interpretations is what ultimately makes life truthful" (Highwater 1981: 206).

As argued throughout, the Native person expects the anthropologist to know more about how to interpret social affairs than does the ordinary person; expectations are higher than those held toward tourists or popular writers, generally. It is recognized that people may reveal more of themselves to an outsider, and that the outsider may be able to see in the insider what the insider himself does not see. But it is not always understood why the researcher has this ability, especially if the host does not understand the research context. He may therefore pay attention to methods as validity, observing the ways in which the researcher interacts. This assessment may be hearsay, as it is important to remind the reader that many northerners have little direct experience with social scientists, and many do not care whether they do or not. But the image of the

anthropologist conveys popular meanings concerning methods, and methods determine legitimacy.

In the first two chapters we devoted some attention to images in anthropological participant-observation. These included images held of fieldworkers by hosts, and images held by fieldworkers of themselves. We found that, as in field roles, there are patterns of similarity found cross-culturally. This is to be expected, since if similar roles are played, often in like situations (such as colonialism), then similar descriptors of role performance will be generated. Images must be evaluated in relation to the social, cultural, economic, and political facets of context.

Images can relate to status or social position, such as the appearance of wealth or power. These are images recognized by hosts as symbolically important. Ethnic or racial classification may be a potent descriptor here also, referring not only to visible features but to the society to which the fieldworker is thought to really belong. Images of marginality can therefore be status images, if it is thought that the researcher has alliances with both sponsors and hosts, or that he never fully belongs to either. But marginality can also appear in personal images. These images, also described by hosts, refer to the performance of role and status expectations. Does the fieldworker act in expected ways; does he do it well or badly? These images have implications for character and behaviour: incompetent/competent, foolish, stingy, nosy, aggressive, or transient.

Finally, self-images are those given by the researcher to himself/herself as part of field experience. Given the intensity of P-O in an unknown culture, and the heavy expectations placed epistemologically and ontologically on the individual observer, these images can also be emotionally intense. Many field accounts have included commentary on self-image, particularly the changes in images that occur in the progressing stages of P-O. These images are shaped by the responses of hosts and others to the investigator's

actions and attitudes. A feeling of marginality, of not belonging, may precede or form a part of P-O. The failure to successfully perform unexpected or new tasks may lead to a self-image of stupidity or of helplessness. One of the most common images, in an era of increased demands for accountability, is associated with guilt and self-questioning: am I justified in being here, and how?

All of these types of images represent the reciprocal, intersubjective process of fieldwork, as they are part of the interaction and response between hosts and visitors. They relate one set of expectations with another, expectation with performance, culture with culture. We will elaborate the consequences of this relational view for our P-O framework in the last chapter. In the remainder of this section, we will provide examples of the above types of images from the Delta experience.

Like all images, those obtained in my 1981 fieldwork were filtered through experience, either through the second-hand reporting of the fieldworker, or literally through the perceptions of the hosts. Images were sensitive topics of inquiry, although they were sometimes offered in conversation about role or other topics. I found it awkward and invasive to inquire about the individual performances of other fieldworkers, but some insight was obtained through host response to the investigator and to the general topic of the fieldwork. Images were obviously personal and emotionally-charged descriptors, varying with the observer/informant's security about the situation described. They included status, personal, and self-images.

Several status images appeared, related primarily to the association of social scientists with the outside world. Although the fact that most (although not all) of the social scientists have been non-Native was noted, this in itself was not a primary image. Native people thought that, at their worst, the researchers might represent some of the most objectionable -- and imagistic -- qualities of the non-Native presentation: aggressive, talkative, superior assessors of the "primitive." Researchers were part of

the system of intruders seeking to take over the North, to tell the people how they should live their lives.

For the most part, the images suggested host response to this North/South system. Researchers were considered transient; several times they were compared to summer birds who come only in fair weather (see Honigmann and Honigmann 1970: 5; O'Malley 1976: 96; Dyson 1979: 64). In the early years, when the role of the researcher was just being formed, most scientists, including social ones, were treated as basically harmless, like flocking and noisy birds. They were simply an appendage to northern households, an extra room added to the igloo.

Anthropologists used to be a joke - an anthropologist in every family (researcher p.c. May 11b, 1982).

There were no negative images in the Delta until at least the late 1950s and early 1960s; people didn't see any harm in them (researcher p.c. July 19a, 1982).

When I first went North there were no negative images; there was no hostility. University people were thought of as slightly crazy, whether they were bug hunters or anthropologists (researcher p.c. Oct.6b, 1982).

Most people have an image of the anthropologist as just engaging in conversation, not doing anything constructive (p.c. Feb. 9, 1981).

Researchers used to come up here in their Eskimo fur outfits, and would find the Inuit in little cloth coats and rubber boots (p.c. Feb. 12, 1981).

As people have come to recognize the association of researchers with the southern-based power structure, they began to accord some potential power to them. Anthropologists became both marginal and liminal, a possible threat. They could take information and use it to adversely affect policy in ways that might not be in northern interests. They were associated with government, and with a lack of understanding between cultures.

Images reflecting this sense of danger were derived, and the term "anthropologist" became symbolic of the outside expert or researcher who occupied the border of intrusive knowledge. This semi-fictional person could be a troublesome

rabble-rouser, complaining to government; this was a view, as we have seen, often held by non-Native residents afraid for their own jobs (p.c. July 19b, 1982; Smith 1975: 125). Or the anthropologists would make promises they could not keep, and they would therefore fail in their tested alliance with northern people. Trouble could thus derive from marginality, as it represented uncertainty in social responsibility: would the researcher work for the people, or for the government? They could be false, in pretending to be one thing and being another, in betraying friendship, or in claiming knowledge they did not possess. They could exploit the people, getting rich over their data spoils (p.c. Oct. 30, 1981; Dec. 7, 1981).

You mean you are an anthropologist? I feel like I have been stabbed in the back (Comment from July 1979 interview in Inuvik).

They had a journalist pose as a social scientist and promise to write about child training methods, and then write something different. No wonder people are hostile (researcher p.c. May 9, 1982).

They could be weasels, never being where they seemed to be, always below the surface, pretending to poverty to get a supper full of private information (p.c. Feb. 22, 1981; Sept. 21, 1981; Jan. 28, 1981).

Most recently, as northern people have realized that few social scientists have made an impact on policy, positive or negative in relation to their interests, the image of irrelevance has become widespread; we will further discuss this in the next section. The anthropologist has become more neutralized, less potent in his/her imagery. This is of course associated with apathy and poor communication. Some people do not really know who the anthropologist is; some do not care. This has always been so, but the non-image seems to have become more dominant. One long-term non-Native resident claimed:

People often know which people were anthropologists, but there is not really a negative image except that people are beginning to question what they are getting out of it (p.c. Nov. 9, 1981).

Anthropologists do not do "real" work, said a Native man:

Anthropologists don't do real work; digging up things is odd, but not work (p.c. Feb. 16, 1981).

Anthropologists are associated with historians and archaeologists as being primarily interested in the past, not in the present:

Anthropologists stay in the Lab, work with bones, and use big words (p.c. Feb. 25, 1981).

Aren't you doing history? (p.c. March 10, 1981).

Aren't you doing a history of the families here? (p.c. Feb. 20, 1981).

People will think you are either doing a school project or studying old bones (p.c. Oct. 13, 1981).

I find archaeology interesting, like National Geographic (p.c. Jan. 28, 1981).

Aren't you afraid being around all those skulls and bones? (p.c. March 5, 1981).

When they run out of bones and historic trivia, they come to study each other, perhaps. Because they stay such a short time, their learning is superficial and shallow. The anthropologist and his studies are representative of the impotence of efforts to straddle the fence between the North and the South.

A few personal images were offered, and, as elsewhere, they tended to be related to the functions of status. They referred, however, to performance of these functions, to the kinds of things researchers did and to how they did them. It should be noted that not all images were negative; many individual researchers were considered by hosts to have been friends; they were described as warm, understanding, interested people. They performed role-less-ness well, in that they were thought to have behaved true to character. Other images represented appreciation of good efforts at rapport, reciprocity, and learning the rules that failed, at least at first.

Personal images often seemed to reflect emotional reactions toward the presumed personal motivations of the researcher. Three Native men reacted strongly to my own

fieldwork with hostility; two of them had worked for the Research Lab and expressed resentment of the "know-it-alls" (p.c. Feb. 5b, 1981) and the "swelled heads" (p.c. Feb. 16, 1981) who were on a "power trip" and were using their knowledge and power to show superiority. The third man reacted strongly to an inadvertent remark made by the writer at a social occasion:

You are a phony. People like you think that just because you have an education, you know more than me. You think I don't know who I am?

On the other hand, the "power" could be reversed by making allegations about the falsity of knowledge. The anthropologist does not know what to believe; he is gullible (researcher p.c. May 11b, 1982; p.c. Feb. 9, 1981).

There is an image of the anthropologist as note-taker, believing everything he has heard (p.c. Feb. 5a, 1981).

We have spoken of this above: who does not like to fool the anthropologist, who is supposed to know so much? He may like to deceive, but he can be deceived.

Among the most prevalent images are those relating to the specific activities of the anthropologist, but also to his status in the outside system. The social scientist who asks about one's personal life is too aggressive and "nosy." He or she asks too many questions, and may violate privacy in finding out what he should not. The anthropologist is one who will measure heads, peer into windows, and always watch from the sidelines (researcher p.c. Oct. 27, 1981; May 10, 1982; July 19b, 1982).

I knew you must be an anthropologist when I heard you asking questions (p.c. March 5, 1981).

How can I tell the people I am with that I am dancing with an anthropologist? (from encounter at a dance in Inuvik in Feb. 1981)

They used to talk about two women who went around peering in windows (researcher p.c. May 10, 1982).

All of these images depict the relationships among forms of knowledge and their bearers. The dangerous anthropologist and the irrelevant anthropologist are products of

this process. The anthropologist as stranger may come to represent the boundary between what the outsider knows, and what the insider knows. This liminality may be a source of power, or a source of uselessness. The researcher is not fully committed to either social context, and he relates imperfectly to both worlds. His performance is lacking relative to local values of response and competence. The role taken may itself be inadequately related to his character and potential. As the visitor absorbs the responses to himself, he forms a series of self-images in relation to what he expected of the situation, of others, of himself: his personalized culture.

Few self-images were offered by the social scientists who were interviewed; most spoke in generalities which could in themselves be revealing. One individual mentioned his foolishness in not always appearing competent in local culture.

There is a lot of self-testing in fieldwork: can I survive? I felt paranoid sometimes, like they were laughing at me. I could not speak the language well; I wondered what they thought of me (researcher p.c. May 9, 1982).

There was worry about competence in field methods, and over rapport and interpersonal relations.

Sometimes I felt pessimistic, sometimes vulnerable, shy - mostly paranoid. I sometimes thought I should be more aggressive (researcher p.c. May 10, 1982).

Others inferred similar feelings without giving specific examples. I went through similar angst in 1981: did this person "forget" the appointment deliberately? Did I see them laughing as I went out the door? All of these observations went into a field journal.

Several individuals reminisced over the roles they had played in their northern field experiences, recalling their frustration at never falling into clearly defined sets of expectations:

I used to envy doctors, nurses, teachers, who had defined roles; they want to know just what you are doing and it is so hard to describe (researcher p.c. Feb. 17, 1983).

It is hard to separate role from image; we play so many different roles, so that we are unique and different: how do we appear to them? (researcher p.c. May 9, 1982).

I felt torn between watching and participating. It was hard to be always watching. Sometimes I was glad when I could just be myself (researcher p.c. Dec. 4, 1981).

To play an acceptable role, with pre-set parameters, was to reduce the frustration of role definition, to speed rapport, and to feel that one's work was valued, even needed. I, too, felt this acutely at times when hosts cast doubt on the fuzziness and marginality of the anthropologist.

Again, others referred to this process, noting that even when the role (and the image) of the anthropologist became commonplace in the Delta, there were still ambiguities associated with it, as we have argued above. In addition, the tendency of Native hosts to suggest that one abandon one's apparent role could be upsetting, as it may leave the researcher even more confused about the images he is expected to project to attain rapport. Most researchers really played several roles, according to immediate context, and, although this is typical of social relations in a small community, it is confusing to the outsider who does not yet know how to allocate them.

The self-image which appeared most often, by suggestion as well as by statement, was that of personal frustration, even guilt, at one's marginal position. Social scientists reported that they felt that they were victims of government, who set them up to do "important" work and then ignored it. And, increasingly, they were beginning to feel that they were scapegoats of northern people, victims of the frustration people felt over their continual visitation by outsiders. The more current the image of the anthropologist as either harmful or irrelevant, the more that image was used to

make statements about the broader social, economic, and political context of North/South relations (researcher p.c. Oct. 4, 1982; Dec. 7, 1981).

We get the blame for the mistakes of other scientists. People thought they were making money off the problems of the people. They accused us of killing caribou, saying that they would not study white people. We have tried to inform people, giving talks, taking them to where we are working. We had to establish a relationship with the community, and to keep it up (researcher p.c. Dec. 7, 1981).

I feel I have no right to research without permission. Some researchers have been overbearing, and have assumed this right, and they have ruined it for the rest of us. I feel the responsibility to correct the mistakes now. They (the hosts) think we think they are less intelligent, and they are sensitive to it and are closing their communities (researcher p.c. Dec. 4, 1981).

I have seen other anthropologists deceive communities, sleep their way through, claim to be poor, not paying much. The Native people are unforgiving. It is hard to live past those mistakes. I felt that I was watched; people were especially demanding because I was Indian also (researcher p.c. Oct. 6a, 1982).

There was frustration about whether to be an advocate, to answer the political call of accountability, and over how to be an effective one. Accountability and responsiveness appeared as a theme, often an impersonal one, in interviews with social scientists, and some implied that they felt their work had not been understood by either sponsors or hosts. The image of the anthropologist needed to be improved, they said, and updated. The individual should not take the blame for the system, and should not be caught in political disputes. The answer was not the neutrality that anthropologists once sought, however, as these northern social scientists have come to realize the impossibility of this task, but rather the accountability we have stressed. Yet, it may be asked why there have to be two "sides" at all in the dialectic; cannot the anthropologist come to represent the universal nature of culture?

To do this, the researcher must come to represent the ontology of culture. This is not a resolution for duality, but a cause of it, as the field rules of the participant confuse epistemology and ontology in the re-creation of culture through the describer, as we

argued in Chapter 2. To avoid this dilemma, one need not assume anything about universality or relativity in terms of validity. One must only recognize the relational nature of the fields of culture. In the conclusion, we will return to the issue of images and relativity to argue the correlation between the relationism of knowledge and the generation of images which describe these relations. The foolish, the naive, irrelevant, and deceptive investigator is only the ideological representative of the juncture of the paths of knowing.

D. UTILIZATION OF RESEARCH RESULTS

Perhaps the most incisive comments by hosts who were familiar with social science research concerned the outcome of research. The same was true of comments made to the investigator in the Science Institute survey of research needs by northern Native organizations, where northerners stressed the need for feedback, the importance of setting priorities relevant to the people, and the importance of continuing application of research results to northern problems (Lange 1987). The northern people wanted to know whether any harm or benefit would or should come to them as a result of any researcher's investigation. Outcome is for them in fact tied to method, as it relates to promises made and roles taken during fieldwork, and to their participation in evaluating the quality and truth of findings. The processes of feedback, of informing the hosts of these findings, and of making the investigations available to those who can use them constructively, is considered part of the research methodological cycle and hosts felt that they had as much stake in these processes as in the actual participant-observation itself.

In a sense, Delta hosts have used feedback and utilization as the most pervasive, practical criterion of assessing the value and validity of knowledge. This, more than any other part of the process, can be understood in its potential if not always in its actuality. That is, hosts demand that indeed they should comprehend the product, and that it can be

used for some understandable purpose. A review of the Inuvik Research Lab in 1979 concluded that, at that time, only 7% of investigations conducted there were relevant to solving locally identified concerns such as health, community development, education, housing, and renewable resources; this represented, apparently, a decrease from earlier years (Espie 1980: 17-18). Here is the investment: what are we contributing to, and how can we be a part of the continuing research dialogue? Most informants, Native and non-Native, made reference to feedback, utilization, or both. Sometimes the issue is addressed in direct emotional and practical terms -- who will make money from this? Will it lead to policy changes which will affect us negatively? Sometimes the issue is expressed as communication -- have we effectively communicated the truth about our lives through the researcher?

Feedback to participants can occur at any stage of the research process. When it is missing, especially at the conclusion of the study, hosts often feel cheated of a stake in communication as participants. Before they can make use of the study, or judge its legitimacy, they must not only be told of the results, but be allowed to comprehend them. Even though hosts may not expect to see the researcher again, they expect to see the results, although ideally both should appear together and the community should be part of the procedures of feedback.

Failure to return a report to a community is, to many northern hosts, a straightforward error. The report either appears or it does not, and it may appear only to selected persons. A specialized consultancy report, or even a government report, may simply not have wide circulation. This is a structural problem which will be discussed in the next chapter on research regulation. But accessibility in general is less straightforward, and it vitally concerns participatory research. What is the value of a study which is based on good communication if the results cannot themselves be communicated?

There are many reasons for this failure. Reports may in fact be sent only to a few key agencies; they may be written in a language the hosts do not understand. Hosts may not be receptive to the media by which results are communicated; community radio or video may be preferred over newspapers, news shows, or even community meetings. People may feel alienated by the infrastructure of research, such as government or industry associations. And, they may simply not be interested, and no means of feedback will be very successful.

Many non-Native people, including researchers themselves, felt that local people, especially Native persons, were increasingly becoming apathetic towards research results and process. They, and the Native hosts, gave poor communication, in its various forms, as a primary reason for apathy. In addition, they made practical suggestions about how it could be improved. They suggested that non-literary forums would be best. The researchers should publicize their work on radio, television, film, and videotape, rather than in books, reports, newspapers, and magazines. Not only would most Native people not read the latter, but they would not understand the language.

They [Native people] need feedback, but not in scientific language. Use the radio; people listen to that. There are too many meetings around here; people won't come. If you make it personal, people will listen because they like to hear about themselves (p.c. Feb. 27, 1981).

The feedback must be immediate; otherwise people will have forgotten about it. It should be in basic language, not loaded with scientific terms. People will read a report or go to a meeting if they feel it is about them, but they probably will not go to the library to dig out reports (p.c. Oct. 7, 1981).

You should use TV, since it is immediate. People will talk about it if they are interested, and they will come to a meeting, although that is not the best form of feedback (p.c. Feb. 25, 1981).

People like the immediate feedback of video and TV. They liked seeing themselves on film when they got used to it (p.c. Sept. 30, 1981).

Some Native people confirmed this opinion, pointing out that the use of English for the older people, and the use of specialized jargon, for all ages, was a hindrance.

We [two Native women] don't understand much when we go to hearings by the oil companies. We don't understand the language; and the words have to be interpreted (p.c. Nov.5, 1981).

Most people don't say enough about what they do, and they use big words. You never really know what they are up to unless you know people working with them (p.c. March 5, 1981).

People would probably be interested if you handed the results to them personally. It has to be something about their region that is immediately interesting to them (p.c. Sept. 22, 1981).

People won't come to a meeting unless it is something that is going to affect them right away, such as a wildlife law change. Sometimes scientists will come and present results, but people don't come unless they think it will affect them immediately. We [Native people] don't read newspapers much, or TV documentaries (p.c. Feb. 22, 1981).

I don't read much; most people don't. Public meetings are better. Most things written about the North are for southerners anyway, not northerners (p.c. Sept. 23b, 1981).

The more immediate the feedback, and the more direct and simple the approach, the better. Again, video, drama, and TV were suggested as means to speak directly to an audience. Public meetings were not effective, because, first, not everyone would attend, and second, of those who did, only a few would feel comfortable speaking and asking questions publicly. A return to the community, and visiting with informants, would be ideal from the Native perspective; the comments made to Lange (Science Institute 1987) were similar as people stressed that letters and phone calls were not as adequate as personal explanations (Lange 1987: 28, 49). The importance of communication was discussed above in relation to the quality of knowledge and in relation to the rules of reciprocity and commitment. A committed researcher should return and personalize his information to his hosts. Lack of feedback can negate the rapport built during participatory research itself. Failure to communicate successfully in reporting can also be seen as part of the growing specialization of research roles associated with alienation from the majority of community members:

I used to know who all the researchers were. Now no one tells me. I hardly know who is at the Lab or what they are doing (p.c. March 5, 1981).

Researchers should come back, and acknowledge who they got information from. We are their roots (p.c. Feb. 16, 1981).

They should come and talk to those of us who actually gave them information, not just higher-ups. We never hear what is being done with that information (p.c. Feb. 25, 1981).

Apathy, for Native hosts, resulted not just because of lack of attempts at feedback, but because of the lack of personal communication and stimulation entailed in unsuccessful attempts. The circulation of research results within the communities of sponsorship (academic, public policy) is an affront to their definitions of reciprocity; it does not lend legitimacy to their personal involvements as informants and as hosts, as we have argued above. As a result, one Native woman with research experience noted that people do not feel that investigators are honest with the people, as they do not feel that many non-Native people are honest (p.c. March 11, 1981); lack of honesty deprecates future attempts at communication. Another Native woman said,

I do not mind if people use my knowledge, as long as they do it fairly and honestly and there are results from it. I don't like research which is done only to sell a person's point of view, such as that development should take place (p.c. Feb. 16, 1981).

The practical counterpart to feedback is the actual use of results to some purpose. In the next chapter, we will assess the degree to which government has used social research that it has sponsored in the Delta. The problem with assessing utility is the setting of standards: is the material to be useful to sponsors or hosts, and to what purpose? If hosts do not actually sponsor the research, what are the expectations they can place on the way in which the investigation can be used? This important question bears on the entire question of method as responsibility and will be further addressed in the chapter on regulation. Some Delta hosts were aware of the implications of this question, suggesting that local people should control the planning of research, the

setting of priorities, the hiring of personnel, and possibly the methodology (p.c. March 19, 1981; Feb. 5b, 1981). Lack of utilization is becoming, for many northern communities, a strong rationale for increasing their own regulatory control (see also Lange 1987).

Hosts stressed the need for a visible accredited use for research. The word "visible" is important here. If research is used somewhere else, and no one knows if and how it is used for policy, then fears arise just as they do when field methods are not visible. There were a few comments expressing concern that territorial or federal policy-makers could be using information to make decisions adversely affecting people in the Delta. This concern has existed all across the North; Delta people who saw the men surveying the town of Aklavik learned to fear that these early investigators were harbingers of misfortune. As will be noted below, this relates partly to a lack of understanding of how research is sponsored. Said one Native woman with experience in the research business:

People are afraid of what is being done with that information, because they do not know (p.c. Feb. 16, 1981).

People have long feared the visible consequences of research in the natural and physical sciences, such as wildlife research. There are apprehensions that studies will lead to restrictions on hunting, for example. A piece of correspondence from 1962, addressed to the Game Management Officer in Aklavik, reveals that a Delta trappers was afraid that, as a result of an Area Economic Survey, he would experience cuts in trapping areas. The trapper was reluctant to tell the researcher this, but he was reluctant to cooperate as well (PAC, RG85, vol. 1276, file 251-1-1, pt. 4). Comments at the time of fieldwork echoed these same hesitancies (p.c. Feb. 6, 1981; p.c. Feb. 25, 1981). With social sciences, people feared the invisible results, perhaps less immediate and

more vital. When researchers have promised that people would see results, it has not always been clear that these would be positive or beneficial (p.c. Dec. 11, 1981).

There must be immediate results, not just long term - the only immediate result that [Native] people get is the suspicion that research will result in policy changes, in spite of the good will or intentions of the researcher (p.c. Feb. 25, 1981)

Few researchers have met their promises, however, and attitudes are changing. Fears are giving way to disappointment that the studies, and the people providing information and opinions, are being ignored. Not only are the consequences not visible, but they are not powerful, either. A young anthropologist who was working in the Delta in 1961 found that the people thought he had enormous power in Ottawa to implement their recommendations (PAC, RG85, vol, 1656, file NR2/3-3, pt. 2). This impression existed to a much lesser extent in 1981, if only because people realized, as argued, that few results had come from most anthropological efforts in terms of policy.

People are getting mistrustful now; they are not seeing enough results (p.c. Nov. 9, 1981).

I guess anthropologists have no power; we [Native people] don't see results (p.c. Sept. 23, 1981).

This is becoming common knowledge even for those with little direct experience with research, and those who understand the research context are especially adamant about it. No new policies for social services, or education, or economic development appear which can be linked to recent investigations, for example. The Science Institute study in 1986 included several comments by Native groups that they particularly resented thesis writers, as students are regarded as having no power to effect change (Lange 1987: 23,29, 49, 53). We noted this image of powerlessness above in relation to images, and it is prevalent among civil servants; we have already stated that many of the non-Native people interviewed in 1981 expressed doubts about the usefulness of social science.

It should also be noted that the lack of apparent policy results has, in part, given way to speculation about other results and rewards, especially among the people who do not have this growing expertise in science. Some people commented that the investigators were certainly getting vast amounts of money for their reports, were getting high-paying jobs, or were selling artifacts (Janes 1982). One Native man said:

There is an image of anthropologists getting large amounts amounts of money from the government, and they come here anxious to spend it (p.c. Feb. 9, 1981).

They come and take the information away; we never see it again and they get jobs and get rich (p.c. Feb. 5b, 1981).

Researchers are like white businessmen, who make a buck and then leave (p.c. Jan. 28, 1981).

There was a fellow who came from the States a few years ago, to [my community] to study language or something, and he acted really friendly, and then went away to make a mint (p.c. Feb. 4, 1981).

They told me about a researcher who came before me. Most liked him and his work, but others said he had gone off to write a book and get rich (researcher p.c. May 10, 1982).

Some researchers who worked in the 1960s reported this (p.c. Feb. 17, 1981; May 10, 1982). Although this particular view seemed to be less openly prevalent at the time of fieldwork, since people had learned more about academic enterprise, there was still insistence from some people that return be immediate and tangible, and that teachers should be paid by their students -- to get a share of the rewards. Informants should be paid a fee based on current market value of labour input (p.c. Feb. 16, 1981; Sept. 21, 1981; researcher p.c. Oct. 6, 1982). Local researchers should be hired, it was felt, and informants should share in the proceeds: "You owe us something" (p.c. Feb. 4, 1981). Therefore although comments about "getting rich" often came from those who knew little about academic markets, on the contrary it was found that the more experience the respondent had with the research context, the more they tended to phrase return in practical terms: skills, jobs, payment. This is exemplified in Lange's

(1987) conversations with leaders of Native organizations, who were exacting in their terms of reference for the research process. These individuals knew that although the individual researchers were not getting rich from sale of data per se, they were part of a "business" of sponsorship and utilization which could involve the hosts as paid participants.

As they became more familiar with the research context, people also began to ask questions about process they still only imperfectly understood. The most frequent remark relating to context was that research topics and questions were frequently duplicated; this comment came from most people who have encountered social science in its various forms of methodology from interviews to hearings. Native people questioned the process:

He [an earlier researcher] should return to see how things have changed, instead of still sending new people in. But it's really the government behind it. They keep asking the same questions, and we are tired of it, because we [Native people] do not know where it all goes (p.c. Nov. 12, 1981).

Most people don't attend meetings, because they keep asking the same questions. Most people said all they wanted to at the Berger hearings, and they don't understand why more meetings are held and the same questions asked (p.c. Oct. 27a, 1981).

They keep asking the wrong questions, and they ask us over and over again (p.c. Oct. 21, 1981).

People get upset at being asked the same questions, when they don't really understand the cycle behind the research (p.c. Sept. 23, 1981).

I don't mind answering questions if I know or like the person, but still there are too many of the same questions and it is a kind of abuse of people (p.c. Oct. 27b, 1981).

The few individuals who have been often used as key informants feel overused and frustrated, they say. In spite of their experience, they are still confused about where the communication breakdown is occurring, and why the agencies involved cannot be better coordinated. The duplication of effort by the various sponsoring agencies noted in the preceding chapters will be further discussed as a regulatory issue. Without the

resources to do their own research on the consequences of science, however, hosts have no way of knowing why duplication occurs, or why it may be necessary. It is seen only as a failure of the intermediary/interpretive function.

There was still, at the time of fieldwork, an impression that the anthropologist was himself/herself a key to that larger unexplained context, and could serve as intermediary or interpreter. These were held to be potential researcher functions. The researcher should be able to convey their responses to the appropriate people, even if he was powerless himself to act upon them. Failure to fulfill the intermediary role can thence be reinterpreted not only as a coordinative but as a personal and political issue. Researchers can be criticized for duplication and its consequences in lack of visible results, and thus can have their intermediary, dualistic stance invalidated.

Social scientists themselves have become very aware of the issues of coordination, duplication, and utilization, and they expressed their frustrations at their own powerlessness to ensure that utilization and coordination took place.

Not all researchers have been sensitive and respectful. One project failed to take material back to the community and to discuss results. Local people were involved in the project only as cooks and helpers; when they don't understand what is going on, they don't support and help you (researcher p.c. Dec. 10, 1982).

People feel betrayed by those who give them no feedback, and then they don't support those of us that follow (researcher p.c. Oct. 6a, 1982).

They told me many people had come over the years that they had never heard from again. They asked me, "What can you do for us?" (researcher p.c. May 19, 1982).

There are many northerners who do understand the research context, including those with university and/or civil service backgrounds. These people serve locally to inform other residents of how to interpret research methods and results, although this function, too, has its political consequences in local rapport when non-Native people attempt to mediate relations between researchers and Native hosts. This pool of

expertise is raising the general level of understanding, as are efforts by investigators to provide their own explanations. They have helped people to realize that there are academic rewards in intellectual pursuit; while this may be criticized as self-interested, it is generally not perceived as threatening, but as part of the investigative role.

Some Native people with extensive administrative and science experience are independently gaining insights also into the processes of science discovery in relation to those of utilization and sponsorship, and this helps them link utilization with the science process itself. One Native man compared the Research Lab to a kindergarten, where each person must learn to step out into the community; a woman said:

I guess each one has to come to learn for himself what the North is like; they can't take the word of the others (p.c. Oct. 13, 1981).

The renewed comprehension of how the puzzle of methodology fits together with the larger context of science and regulation has led, then, to a more practical political view of social science. The more insights a person gains, the more they are able to separate the personal and methodological issues involved in research. The importance of direct, personalized feedback, of participation in reciprocal accountability of research, and the use of the investigator as interpreter is suggestive of a demand for personal accountability. We will return repeatedly to this theme, as it has emerged in preceding chapters.

Many Native hosts have long held people personally responsible for professional participatory methods, and have validated knowledge using personalistic criteria. Those people with broader conceptualizations of research enterprise have tended to depersonalize the process, and to separate the person from the science -- but not from his/her political context and responsibilities. In both cases the image of the anthropologist represents institutional betrayal, fears that decisions are being made

about the North based more on "un-knowing" than on knowing; and the image represents the ideological juncture between cultures as communication. Understanding these perspectives is vital in evaluating method and result, and in setting agendas for research accountability and reliability. In the next chapter, we will look at some of the mechanisms of control over research, and we will compare government, academic and host perspectives on regulation.

CHAPTER SEVEN: REGULATION

This chapter will conclude the case study. We have seen that social research in the Delta must be analyzed and comprehended within a broader context of the Canadian North and polity. Some of the facets of research regulation by sponsors, academics, and hosts will be outlined in this chapter: the utilization of research, the development of licensing, and institutional frameworks for regulation. None of these will be explored in detail because of space limitations; only the major initiatives will be included. This will be followed by a discussion comparing perspectives on regulation held by government sponsors, by academics, and by Native northerners. Some of the central concepts used throughout the thesis will be used for this comparison: access to research resources, prioritization of research, participative roles, validity, and accountability. The investment of each group of actors in the research process via regulation will be summarized, and the chapter ends with a discussion of community-based research in northern communities.

Each actor in the research process has a stake in process and outcome. Regulation of science means controlling funding, negotiating access to communities and hosts, deciding who participates in the research, and controlling feedback and utilization. Most importantly, it can involve decisions about the ethics of process and the legitimacy of knowledge. We have seen that, within the participant-observation model of anthropology, there is dispute about the nature of knowledge, subjective and objective, and its interpretive genesis. A set of rules has developed to produce and safeguard a method which will approximate reliable knowledge. We have discussed some of the conceptual problems with these rules in the preceding chapters. Anthropology as a

social science highlights the definitional problems of legitimacy; it not only uses culture as a paradigm, but also recognizes and even creates it. We have argued that the rules of participant observation convey the involvement of the researcher as a border figure, bridging "inside" cultural knowledge with "outside" objective perspectives. Both monistic and dualistic approaches to knowledge rely on the marginal individual to convey ontology through epistemology. As the instrument through which validity can be assessed, the observer's own rationality becomes the locus of ontology, and dualism is used to reconcile the epistemological/ontological separation.

In the participatory tradition of the Delta, similar processes of knowing and image-creation have been reflected in the roles of a succession of participating visitors from early explorer scientists to contemporary bureaucrats. "Culture" was reified into a collective representation which was increasingly seen as marketable as image and as ideological symbol, and which could be separated from individual performance. As culture and its creation came to signify the dualism of "us" and "them" [North and south, or Native and non-Native], the researcher became a symbol of the confrontation of cultures.

We will return to this synthesis in the conclusion. Regulation of research has in fact become a public dispute about methods, outcome, and legitimacy. It is the forum of control for all actors, including hosts, researchers, and sponsors; by controlling process, method and epistemology are affected, and they in turn mark the ontology of culture and the legitimacy of its creation and re-creation, as we have argued in previous chapters. The concepts of objectivity, relativity, and culture that we have tried to define are functions of the genesis and use of social research in "knowing how to know" within specific political contexts such as that found in the Delta. Regulation is the political process which informs validity.

A. FRAMEWORKS FOR REGULATION

1. Utilization

In the last chapter we discussed the use of research -- or, more often, the failure to use research -- as a prominent aspect of host response to methods of investigation. In this section we will examine evidence to determine to what extent northern social science in general, and Delta science in particular, has been used to inform policy-making decisions. Utilization is both the final and the ultimate regulation of research. By not using information, or even by suppressing the use of data, the sponsor or policy-maker is obviously regulating the flow of that information.

Several surveys done within the Canadian Native context of utilization support the opinions of Delta hosts about the failed promises of social research. In the early 1970s surveys by Koenig (1975: 46-49) and Whiteside (1974) concluded that northern and Native people did not feel that most university-based research was relevant to their needs. The early 1970s was a period in which relevance was widely explored in both the popular and academic press; Native leaders spoke out about the ineffectiveness of research, and academics suggested that unless future research was regulated on new terms of relevance, there would be serious problems in future research negotiations (Francis 1973; Bucksar 1969a,b; Rowley 1975; Lotz 1969). But the problem has continued. Moira White, in a brief study conducted for the Association of Canadian Universities for Northern Studies (ACUNS) on northern research priorities, interviewed Native leaders and research officials. She identified lack of relevance and failure of accountability as major problems noted by her informants (1981: 13-14). A progress report on workshops held by the Social Sciences and Humanities Research Council (SSHRC) to plan a Strategic Grants Program of Research on Native Issues (1983: 5-6) in the early 1980s stated that Native leaders and educators were still asserting that research was often biased toward academic rather

than community interests. A report prepared in 1987 under the auspices of the Science Institute of the NWT, based on consultation with Native organizations, contained many pleas by those organizations for redirection of research toward locally identified needs (Lange 1987).

Part of the regulatory process is thus making investigation relevant to interested actors; relevance can be defined as the creation of data bases, as problem-solving, or as substantiation for political policy. Northern communities may have a different perspective on relevance and utilization than sponsors, as we have seen; we will further explore this in the next section. In this subsection we will look at utilization from the perspective of the federal government, particularly INAC and its predecessors (NANR, DIAND, DINA). The uses made by industry or academia will not be included here, partly because of lack of information and partly because a full investigation of this would be well beyond the current scope. It was the connection of research with public policy which most fascinated hosts and researchers, Native and non-Native, so an attempt has been made to obtain some data about public use of information. Data come from government documents, from correspondence available in the Public Archives of Canada, and from interviews with social scientists and government officials. It is not possible to document every study conducted in the Delta itself, since, as suggested above, many have "disappeared" from public view, but it is possible to identify some general patterns of use and response by the federal government concerning northern social research (including but not strictly limited to Delta research).

Some data are available on the use of research from 1954 to the early 1970s. When the Department of Northern Affairs and Natural Resources was created in 1954, with its new Northern Research and Coordination Centre (NRCC), it was given a mandate for the scientific discovery of "the means of dealing with conditions relating to its [the North's] further development" (DIAND brief to Senate Special Committee on Science

Policy 1969: 4369). A Deputy Minister of NANR wrote to the Treasury Board in 1954, for example, that the anthropology to be conducted by the NRCC would assist the Eskimo in moving from the Stone Age to a new way of life (PAC, RG85, vol. 549, file 1003-1-4, 1954-57 NCRC).

To meet the broad assimilative and protective objectives of the era, social researches were conducted by in-house staff, seasonal employees, contract staff, and grants to institutions. Because northern research was still new, there were seemingly few restrictions on topics; some were suggested by researchers but many came from requests by departmental divisions to do studies on topics such as the effects of relief payments and the relationships between culture and educational success. At meetings of the departmental research committee representatives proposed research and reported on the progress and dissemination of reports. Problems arose about priorities and coordination of efforts among staff, including a debate about the relative benefits of basic long-term research, favoured by the NCRC, and the short-term problem-oriented studies with fast, focussed results demanded by the bureaucracy (PAC, RG85, vol. 1659, NR4/2-2, pt. 2; researcher p.c. Dec. 8, 1982).

References occasionally appeared in government correspondence about the actual utilization of reports. Most social research reports seem to have been read primarily by lower level civil servants and field staff. V. Valentine of NCRC advised in 1959 that a series of regional handbooks be prepared for government personnel. A report on Tuktoyaktuk by J. Ferguson was purportedly used to plan programs (PAC, RG85, vol. 1659, file NR4/2-2, pt. 2). A book by Frank Vallee, Kabloona (1962), was recommended by the Director of the Northern Administration Branch (NAB) in 1962 to field staff as factual and objective (RG85, vol. 1897, file 1003 1-4, pt. 3 NRCC 1962-65). Similarly, the work of VanStone (1963) on the community of Snowdrift was widely read, according to a NCRC staff member (RG85, vol. 1656, file NR2/3-3, pt. 2).

The Area Economic Surveys sponsored by the Industrial Division of NANR in the 1960s were among the studies most utilized; these were independent of NCRC and were used for program planning and review. The Division contracted summer surveys, and worked on strict deadlines for reporting (RC85, vol 1276, file 251-1-4, pt. 3-5; researchers p.c. July 19b, 1982; Dec. 7, 1982; DIAND Brief to the Senate Committee on Science Policy 1969).

The NCRC lost some of their independence once they came directly under the control of DIAND in 1968. At the time there were many advocates within anthropology of "action research," with an optimistic creed that social investigation could affect policy constructively. Yet there was also growing academic discontent that the government was hasty with results and was using studies only to justify, not to shape, policy. The Treasury Board and the political process flowing from it were responsible ultimately for decisions which social science, with the best of intentions, could not touch. A list of NCRC social projects begun between 1955 and 1963 shows that of 45 completed reports, only one resulted in action taken in the policy arena; 31 were distributed, but of this number, 7 had only limited distribution to government agencies (RG85, vol. 1661, file NR7/1-1). Most researchers interviewed by the writer felt that their work had no impact on senior levels; their results may have been read by junior administrators for interest. One former NCRC staff member commented that he had never attended a meeting in the Department where any study was seriously discussed (researcher p.c. Dec. 8, 1982); academics at the First and Second Research Conferences (1967, 1968) lamented the lack of utilization of social science -- both that already conducted and that badly needed (Kupsch, ed., 1968; Bond, ed., 1969; see also researchers p.c. May 11, 1982; July 19a and b, 1982; July 16, 1982; Oct. 5, 1982; Oct. 6, 1982; Feb. 17, 1983; Nov. 10, 1982; Dec. 8, 1982; Dec. 9, 1982).

The Department was in fact often critical of social science, while anxious to avoid any criticism of its own operations. Comments appear in correspondence from the 1950s and 1960s which explicate expectations; administrators expected reports to be factual, objective, unbiased, and practical. They also wanted them to have magical insights into the northern Native psychology and sociology (researcher p.c. May 19, 1982). Long-term studies were often seen as unnecessary and subjective. At a Research Committee meeting in 1959, a Deputy Minister queried those present about whether the studies underway were really practical to field administrators (PAC, RG85, vol. 1659, file NR4/2-2, p.t 2). In 1962 a senior official questioned why a study on the Métis had been approved, since they already knew all they needed to know about living conditions (RG85, vol 1897, file 1003-1-4, pt. 3 NRCC 1962-65). These comments echo those reported in the preceding chapter from civil servants in the Delta: our experience can tell us as much, or more, than the anthropologist can; he/.she must be more objective than us and describe reality better, or he is of no use.

In spite of the quest for practical results, a fundamental rule of research utilization was that the investigator was not to interfere with the work of the bureaucracy or to criticize the results of its policies; this rule was to be a major source of regulatory conflict between academically-based researchers and the government institutions (researcher p.c. Oct. 5, 1982; May 9, 1982; May 10, 1982). Researchers told of having to omit or suppress comments which might offend prominent community citizens, criticize departmental personnel, or explicitly show a failure of policy (researchers p.c. May 10, 1982; July 19b, 1982). At least one anthropologist was prohibited for a period of time from getting a license after he wrote about an administrative incident in the community he was investigating; others were censured from looking too closely at non-Native staff: commenting on their social habits and

looking at records, for instance (researchers p.c. May 19, 1982; May 11, 1982; July 19b, 1982).

As noted in Chapter Five, a report on the Aklavik relocation, commissioned by NANR to investigate resistance to the move to Inuvik, was never published because it was critical of government operations and their long-term consequences for the social health of Inuvik and Aklavik (p.c. May 10, 1982, July 19a, 1982; RG85, vol. 1656, file NR2/3-24, pt. 1). At about the same time, in 1957, a researcher who was examining the adjustment of Eskimos to Coral Harbour became interested in the activities of non-Native residents. NANR administrators criticized his report as relying too heavily on rumour rather than on fact, and suggested that he was spying on the non-Native population. The researcher responded by alleging that the NCRC was interfering with academic freedom in telling him what to write (RG85, vol, 492, file 530-136-1, pt. 12).

In 1966 NANR was merged into DIAND: the late 1960s was a period when DIAND was planning to speed up the assimilation of Native people, as formulated in the draft "White Paper" of 1969. Weaver (1981) has documented how one government-sponsored study of the status Indian population in Canada, the massive Hawthorn-Tremblay Report (A Survey of the Contemporary Indians of Canada 1966, 1967) was overlooked when that policy was drafted. The Delta had its own large-scale study at the same time which had similar purposes. The NCRC and its successor under DIAND, the Northern Science Research Group, planned the first large-scale multidisciplinary project in northern social science, as described in Chapter 5. The Mackenzie Delta Research Project (MDRP), recommended by the Research Committee, was to utilize scientists from different disciplines, obtain a comprehensive portrait of the Delta populations, and provide the basis for programming the shift from a traditional/wage economy into a developing industrial one. The Delta was a modernizing area, with a

diverse population, a potential for oil and gas development, and an accessible Research Lab; these criteria made it a ripe area for such an investigation. One key person in the project noted in interview that social scientists were just beginning to understand how southern decisions were affecting the North, and were anxious to examine those impacts without yet focussing on the decision-makers themselves (researcher p.c. July 19a, 1982). As the work progressed the interests of participants shifted somewhat from the purely intellectual to the potential of the results in directing and planning change (researchers p.c. July 19b, 1982, May 10, 1982).

When the MDRP was planned, officials from various agencies such as NANR, Indian Affairs Branch (Citizenship and Immigration), National Health and Welfare, the RCMP, the churches, the territorial government, and the commercial Hudson's Bay Company were invited to make suggestions about research projects; these were compiled by NRCR and the Research Committee (researcher p.c. July 19a, 1982). Some preliminary studies were conducted in 1965, after which key problem areas were defined. In 1966-67 the major studies were undertaken, followed by the publication of reports. The majority of researchers in the Delta at that time, many of them graduate students, were associated with the project. Two conferences were held involving government officials and social scientists to discuss results and plan actions (Senate of Canada, DIAND Report to the Senate Committee on Science Policy 1969: 4388-4394; Report on Second MDRP Conference ms. Dec. 6, 1966).

Most, but not all, of the studies were published by NSRG. Although they are still read and used as data sources on the Delta, there is little evidence that they had the envisioned effects on directing northern policy. Not all of the reports included recommendations, and they were criticized for it. Apparently there were some internal directives as to the data and interpretations suitable for publication, as local officials were anxious about the exposure of some social issues (researcher p.c. May 10, 1982).

In April of 1967 the Coordinator left the project, and personnel began to disappear to other responsibilities, leaving a gap in the central planning of the project. Plans for the third phase, that of feedback of results to the North, to federal and territorial government agencies, and to academics, were shifted through the existing group and finally shelved. These plans had included use of photographs, radio broadcasts, video projects, and workshops as well as the standard reports (researchers p.c. July 19a, 1982; July 22, 1982; Oct. 6, 1982; Feb. 17, 1983; May 10, 1982; Senate of Canada, DIAND Report to the Senate Committee on Science Policy 1969: 4394-395).

Thus the largest project attempted by the NRCC/NSRG has had no apparent long-lasting legacy. By 1968, when NSRG lost its status of reporting independently to ACND and became an arm of DIAND's Northern Affairs Program, interest in social research was declining as investments in non-renewable resource development began to eclipse the protective stance toward Native people (researchers p.c. Oct., 6, 1982; July 19b, 1982; July 16, 1982). The P-O research conducted under the NCRC, especially the Mackenzie Delta Research Project, was on the wane as an acceptable use of research time. Hugh Brody was one of the last to be sponsored to do depth research on Native culture and sociology. Following a year investigating Edmonton's skid row (Brody 1971), he was sent North to investigate Eskimo reactions to change. Brody (1975) did a long-term P-O study, under NSRG auspices, of Native perspectives on government intervention and development which was very critical of government roles and attitudes in the North. NSRG did not publish it, but it was printed by a private publisher and was subsequently widely read.

An incident at the end of the 1960s involving a NSRG employee, Peter Usher, was thought by several individuals to have signalled the end of the freedom and independence of the research unit in terms of methods and reporting. It exemplified fears of government censorship of social science, placing the unit under a new regime whereby

basic research has been further discouraged and underfunded. Usher had done cultural geographic/economic studies of the people of Banks Island, beginning when he had worked on an Area Economic Survey of the Island in 1965 (Usher 1965). He had done further researcher there in 1966-67, and, while an employee of NSRG, he returned briefly in 1969, 1970, and 1971 to follow up his earlier work. While there, he realized that the government had given permits to two oil companies to do exploration without the consent or knowledge of the Native people, who had been granted trapping rights to the same lands. A controversy arose in 1970 when the Native people, backed by the newly-formed Committee for Original People's Entitlement (COPE), publicized the matter and threatened court action until a compromise agreement was reached (researchers p.c. July 19a, 1982; July 19b, 1982; Usher 1971a: 35-61).

Disillusioned, Usher returned to Ottawa and publicized the situation as a third volume to the earlier Banks Island reports (Usher 1971a). This led to a public argument with the Department; officials were unhappy with Usher's critical view and his role in the publicity of the crisis. Usher was confined to Ottawa until he left DIAND to begin work with COPE. The working atmosphere within DIAND apparently deteriorated after this. Researchers feared that DIAND would further suppress any criticism, and they no longer had many illusions that their work could affect government policy as either advocacy or as an alternate view of "reality" (researchers p.c. July 19b, 1982; May 19, 1982; Oct. 6, 1982). The old loyalties expected of social scientists, that they protect the government, were eroded, and a new concern with the ethical consequences of research with Native people arose. Whereas the government had never consulted Native people about their research needs, the social scientists now began to ask the question.

The 1970s and 1980s have found basic social research competing with, again, short-term administrative needs, with mineral exploration imperatives and industrial

infrastructure, and with assessment and evaluation contracts. Other studies were sponsored in the mid-1970s, in areas such as the role of cooperatives, prejudice in textbooks, the justice system, and some long-term studies of languages, but many investigations within the government were short term, with a more applied focus than was previously evident (MOSST Inventory of Federal Northern Science Projects 1965-76). Research has increasingly been done by in-house staff, and DIAND's northern social research group has, since the mid-1970s, been operating in conditions of reduced independence, scope, and staff. In 1982, at the time of fieldwork, several individuals who had been or were connected with DIAND's research group commented on the "death" of social science as an independent enterprise (researchers p.c. July 16, 1982; July 19a, 1982; July 19b, 1982, Dec. 8, 1982; Dec. 9, 1982). Most research within DIAND at that time was centred on program support, such as land claims research, and on specific needs of the department's "managers" for program development and implementation (SSHRC Nov. 1982: 8; DIAND 1980:10). The same was true for the territorial government, which in the mid-1970s was doing program evaluations and social studies relating to development; not all of these were published for public utility (MOSST 1975).

The use of social science in the bureaucracy, then, is fraught with problems of competing views of relevance, political manipulation, duplication and poor coordination. In the rest of this chapter we will look at some of the regulatory efforts which have both promoted and tried to ameliorate these dilemmas.

2. Licensing

Licenses to conduct scientific research in the North have been issued by the Canadian government since the 1920s. In the early part of this century, expeditions from foreign governments, such as Denmark and the United States, were entering the

northern territories to do scientific investigations and to explore. This was a threat to Canadian sovereignty; there was fear that science was being used as a cover for expeditions making territorial claims. In addition, there were growing concerns about the need for information about the activities of scientists in the North, the need for logistical data about their travels, and the need to coordinate and control scientific endeavours. In the early 1920s an American explorer, D. MacMillan, was the subject of some furor when Canadians believed he had entered the Canadian North without permission (although in fact he had been granted such permission); in 1924 another American, H.A. Snow from the Oakland [California] Museum, received permission to mount an expedition. Snow was soon reported for mistreating animals and "Natives" and for freely exporting specimens (PAC, RG85, vol. 85, file 202-21-1).

In 1925 the NWT Act was amended to allow for licensing, and in 1926 the Commissioner of the NWT passed an ordinance regulating the activities of scientists and explorers through licensing. Knud Rasmussen and his Danish Fifth Thule Expedition were among the first to go through the new licensing procedure, in 1926, which required certification that the applicants had no political associations (including the intent to claim sovereignty), basic information about the expedition, notification of the RCMP of their travel plans, and the submission of a report at the conclusion of the expedition. Licenses were first issued through the Commissioner of the NWT, then by a succession of offices. Although the licensing requirement was originally aimed at foreign scientists, information gathered through licensing was increasingly used to obtain centralized information about all research activities, including domestic ones. It also gave officials a means of regulating interaction between scientists and Native people, as we have discussed in preceding chapters. For example, in 1949 the Deputy Commissioner told one applicant to restrict his interaction with Eskimos in order to reduce dangers of epidemic disease (RG85, vol. 85, file 202-2-1). An anthropologist

in the Eastern Arctic was warned not to shoot game in his attempt to live with the Eskimos in 1955 (RG85, vol. 524, file 530-136-1-8). In 1957 a question was raised by the Chief Medical Officer, NWT, urging protection of Eskimos from amateur studies (RG85, vol. 457, file 530-136-1-11).

The ordinance underwent several amendments over the next decades. In 1959 the Northern Research and Coordination Centre was given the responsibility of issuing licenses, since part of the mandate of NANR (of which NRCC was a part) was to foster research. Copies of the licenses were then given to the RCMP, and to the Secretary of the ACND, affiliated with NRCC, who then distributed them for information to all federal departments associated with northern research.

By the 1970s the volume of research in the North had increased substantially, as had northern concern about it. In 1974 the GNWT took over responsibility for licensing in order to exert some control over the process, and a new territorial "Ordinance Respecting Scientists" replaced the old federal ordinance. The territorial ordinance was passed in the aftermath of debate in the Territorial Council about duplicating invasions of social scientists. The people of Baker Lake, NWT, had passed a resolution banning scientists after residents objected to demographic questions asked by an American anthropologist. Debate revolved about procedures, academic freedom, and the amount of control to be held by the GNWT; government employees conducting job-related researches were exempted from licensing but it was hoped that they would nonetheless follow the procedures outlined in the ordinance for obtaining permissions. Social sciences are included but archaeology is covered under a separate ordinance (Whiteside 1974: 1-4; Globe and Mail June 15, 1974; Council of NWT Debates, 53rd Session, 7th Council, June 20, 1974; Freeman 1980: 12).

Social scientists applying for a license were still to supply information about logistics, source of funding, affiliations, and project plans and methods; there was also a

reporting requirement asking investigators to file a copy of their reports with the GNWT. The most striking departure from the earlier federal ordinance is in an associated policy of obtaining permission from communities. The applicant must furnish evidence that the communities have given their permission before a license can be granted. This has the potential of giving northern communities some control over research, although a denial of permission may be overridden in exceptional circumstances. As we will see, however, there has been some doubt about whether communities fully realize that they have the power to deny permission, and over whether such powers and procedures are always used in an informed manner by scientists and communities. Some scientists have failed to apply for permits, and many others have not submitted reports (p.c. Dec. 1, 1981; researcher p.c. June 26, 1986). At the time of the writer's fieldwork in 1981, licenses were issued by the Science Advisor/Science Administration Officer of the GNWT, Dept. of Renewable Resources. In 1986 the new Science Institute of the NWT took over the licensing of research, and began to require researchers to visit settlements to explain their work and to provide summaries of their objectives, in layman's terms, for translation into aboriginal languages.

3. Regulatory Frameworks

The development of frameworks for northern science can be traced through two kinds of initiatives by government, academics, and northerners. The first is the attempt to find a mechanism to coordinate northern research and reduce duplication. The second is a series of ideas for a university of the North. The discussion here will focus on the first of these two initiatives. In some of the earliest proposals for a university of the North, in the 1960s, the two initiatives were conceived with a common purpose, and this will be noted. But by the late 1970s the two had diverged. The NWT did obtain an

Arctic College in the 1980s which is increasingly seeking academic recognition beyond a standing as a technical institute and certificate program, and which began in 1988 to offer first year general university courses to northern students in Yellowknife.⁵⁰ Although it is likely that the College will play an increasing role in northern social science accordingly, this will not be discussed here.

The following outline will present some of the ideas and models which have been proposed as tools for northern research coordination and regulation; it does not include every proposal, and it should be noted that most frameworks have never seen fruition as originally planned. Discussion in the next section, on the perspectives of the actors, will refer to and amplify the intent of the proposals.

Some of the first proposals did indeed concern a university-like structure. In the first two Northern Research Conferences called by DIAND in 1967 and 1968, speakers addressed the need for a secretariat or a college to integrate existing science facilities (Kupsch, ed., 1968; Bond, ed., 1969). Around 1970 interest increased, stimulated from several directions. Jim Lotz, who had worked with the NRCR (DIAND) during his career, argued for a university of the North in his book, Northern Realities, published in 1970. Lotz was critical of the government's role in science and called for a university which would introduce southern students to the North, conduct research, disseminate information, and provide a "neutral" ground for the development of ideas. He felt that a university structure similar to those in the south would ensure an institutional independence for northern science (Lotz 1970: 239-49).

The Man in the North (MIN) project of AINA was urging consideration of a northern university at the same time (Gourdeau 1973). The Association of Colleges and Universities of Canada (AUCC) planned a conference for 1971 on the idea, but this conference was never held once it was realized that a separate foundation, the Mid-Canada Development Corporation, had begun a rival initiative. A group known as the

University of Canada North (UNCANORTH) had been organized by the foundation, with both northern and southern representatives, and it held a two-day conference in Inuvik in 1971 concerning a northern university. There was little agreement on the form of the university, its teaching subjects, the amount of academic content, and the funding. The only real agreement came from northern and Native people, who wanted northern control and independence from the south (Kupsch and Caillol 1973: v-viii; Inuvik Drum Dec. 1, 1971: 1). The UCANORTH group continued but lack of funding aborted its activities.

In 1975 T.H.B. Symons recommended a northern university again as part of a study on Canadian Studies programs in the universities commissioned by AUCC (Symons, To Know Ourselves, vol. II: 149-51). By this time the concept was shifting away from an endorsement of a physical structure in the North and toward a series of programs for research conduct and coordination. At the same time that Symons was doing his study, the sub-committee on Science and Technology of the ACND was interested in developing a northern science policy; they recommended that a set of guidelines be developed and instituted (H. Faulkner 1978: 3).

"A Seminar on Guidelines for Scientific Activities in Northern Canada 1972" was held in October, 1972, sponsored by DIAND and the ACND sub-committee (DIAND 1972). The conference was a starting point for federal initiatives toward a science framework. Participants from government, academia and industry called for a science policy which would echo the new federal northern guidelines and balance preservation of Native culture and renewable resources with the integration of the North into an industrial economy, including planned development and the settling of land claims. Social research should likewise balance the pure with the applied, the southern theory with the northern expertise, they felt. In 1976 the "Guidelines for Scientific Activities in Northern Canada" were published by ACND. The 14 guidelines provided for a

continuing role by the federal government, particularly ACND, in prioritizing and coordinating northern research science; northern people were to be given a supporting role in consent and feedback, and academic scientists were to be the watchdogs for science quality. The guidelines reinforced the norm of "participation" by southern scientists in the North in a political as well as scientific sense, but they were unenforceable without a co-existing northern science institution.

As these guidelines were being publicized, however, both DIAND and various science bodies were working toward developing frameworks for such an institution, still incorporating ideas for universities and for regulatory bodies. In 1977 and 1978 the Science Council of Canada took up the theme of science frameworks; a 1977 document, Northward Looking: a Strategy and a Science Policy for Northern Development, represented the culmination of a three and a half year investigation into the role of science in northern development. The document supported the federal guidelines and the creation of a northern university. The Council envisioned a physical structure, a set of campuses, much like that endorsed by Lotz seven years earlier, which would not only function academically like a southern university but which would incorporate the federal government's coordination mandate. As a first step toward this type of university, the Council recommended in 1978 that a Northern Resource Centre be set up, intended to give southern scientists a permanent base (institutionalized participation) in the North wherein they could blend pure science with local practical applications (Science Council of Canada 1978).

Other science groups supported the formalization of northern participation and coordination. The working group of the Sub-Program 4 (Science and the North) of the Man and Biosphere program (MAB) urged the creation of a permanent secretariat on science to bring together information about the North; this secretariat would have more of a bibliographic function than a regulatory or university role (Canada/MAB Report 8:

13). And the Association of Canadian Universities for Northern Studies (ACUNS), which began in 1978 out of those early northern research conferences sponsored by DIAND, also supported the development of a northern science body. This idea began at once in 1978 with an endorsement of a university of the North by the newly-created Committee on Research (ACUNS Annual Conference Proceedings 1978: 40).

As discussions continued over the next few years at annual meetings, ACUNS developed a position that there should be a body of northern-based scientists and that the southern university establishment should act as overseers of northern science method and utilization. A 1981 proposal for a Council on Northern Research, which would set objectives and distribute funds, carried this idea further toward institutional control of northern science. This proposal came to compete with DINA's simultaneous plans for a Science Institute, however, so that in 1982 ACUNS instituted a Canadian Northern Studies Trust which has continued to operate throughout the 1980s. The trust has become a funding agency for northern scientists and students, and ACUNS has sought funding through government, foundations, and industry (see also Chapter Four).⁵¹

By the early 1980s the idea of a northern university had become somewhat separate from the idea of a regulatory body for northern science. The federal and NWT territorial governments have created two separate institutions, an Arctic College, as mentioned, and a Science Institute. Although it is likely that the College will increasingly become a focal point for northern science, attracting social scientists as researchers and teachers and training northern students in science, the regulation of science has been given at present to the Science Institute.

In the late 1970s, after the creation of the federal Guidelines, DINA realized the need, as did the various science bodies just described, for a stronger regulation of science to enforce the guidelines. There was obviously a need to reduce duplication of investigations among the actors, and a need to promulgate research with tangible benefits

to the public and to Northerners in particular. In 1979 the then-Minister of DINA, Hugh Faulkner, asked the Science Advisor to undertake consultation with the major actors about the creation of a framework. Keith Greenaway, the Science Advisor within DINA, held these consultations, produced a working paper ("A Framework for Scientific Activities in the Yukon and Northwest Territories"), and held a Seminar on Science and Research in 1979. The Seminar was chaired by Dr. T.H. B. Symons and discussed the implementation of the framework Greenaway had created. Academics, government officials, and a few northerners discussed the paper endorsing the concept of a science institute which would be located in the North, would respond to the interests and needs of northern residents, would foster and protect the integrity of science, and would provide a science resource for educational establishments (DIAND 1979: 4).

Greenaway's institute would be modelled as a private corporation: one each for the Yukon and the NWT which would be responsible to territorial governments. They would be managed by Boards of Governors, including "northern residents;" activities would include science support, applied and basic research programs, science training, and the provision of advisory services. The concept passed through a new Conservative and a new Liberal minister at the turn of the decade. By 1982 the proposal was losing federal priority in spite of support by Native organizations and other major actors (researcher p.c. July 21, 1982; Sept. 30, 1982). The Science Advisory Board of the NWT (described in Chapter 4), however, had reservations. They did not want the Institute without more territorial control and more funding. They suggested that the Igloodik, Frobisher Bay (Iqaluit), and Inuvik Research Centres be turned over to territorial control along with a core grant from DINA for maintenance and research activities. They also wanted funds to train northern researchers and to build a Yellowknife centre, and they wanted more responsibility in planning and coordinating northern science activities (SAB response paper Sept. 18, 1981; Harrison 1983: 4;

Upton 1983: 1). These requests were in line with northern momentum to have science controlled by the North, and the Legislative Assembly asked the SAB to prepare a proposal implementing changes to the federal framework (News of the North March 11, 1983).

The Science Institute concept finally regained momentum under the 1984 Conservative government, and it was approved by the Legislative Assembly in 1985. It effectively absorbed and replaced the Science Advisory Board. The objectives and activities are similar to those in Greenaway's 1979 proposal, but they are more oriented to northern needs and control, as requested by the GNWT. The Institute has been mandated to issue licences, coordinate research reports, train northerners in the sciences, advise the Assembly, and sponsor science activities and programs. The executive director serves as a Science Advisor to the GNWT, and the Board of Governors, appointed by the Executive Council of the GNWT, has four northern residents and six southern ones representing a variety of science disciplines and northern expertise. The Institute has responded to requests for information from the Legislative Assembly, has fostered the training of northern people as researchers and assistants, has been involved in the planning of a learning centre for traditional Inuit knowledge (Inuit Silattuqsarvingat), and has sponsored, with ACUNS, a study on northern research priorities, for example (Lange 1987; 1986 Annual Report of the GNWT; Northline April 1985: 5). On November 1, 1988, the Science Institute took over responsibility for the Northern Research Centres, including the Inuvik Lab, with a plan to use more forms of local community management (Northline October 1988: 6). The Institute has also undertaken work to facilitate liaison among members of the northern science professional community, to blend pure and applied science (as discussed above) and to enhance the preservation and credibility of northern Native science knowledge (see Janes 1987).

A final initiative in northern regulation will be mentioned here. In 1985 the then-Minister of DINA, David Crombie, put together a small task force of individuals with northern interests to advise him on setting up a National Polar Institute. The Polar Institute would be a national body representing Canada in international science and sovereignty, and it could also provide a bibliographic and coordinating role as well. As the number of institutions involved in northern science has proliferated, the need for coordination has required increasingly higher-level coordinating bodies.

The task force held extensive consultations once again, and encountered doubts about the need for a new separate institution. In their report, they criticized the federal government for not supporting and coordinating its northern science community and failing in a commitment to long-term independent studies. The task force felt that Canadian science should be integrated with -- and current with -- science developing in other countries. They recommended the creation of a Canadian Polar Research Commission which could report independently to senior levels of government and advise it of research needs. In addition, they recommended an expanded Polar Information System, increased financial support for northern research institutions, and a "Polar House" centre where various government, academic, and northern actors could meet to "network" (Symons 1988: 94-5; Northline vol. 7: 2: 1-2). These recommendations were submitted to the federal government (then-DINA Minister Bill McKnight) for further action, and DINA subsequently appointed T.H.B. Symons to do an intensive study of the proposals.

B. PERSPECTIVES ON REGULATION

Three groups of actors will be discussed in this section: the government (primarily Indian and Northern Affairs), the academic establishment (primarily ACUNS), and the northern Native groups. There is overlap in perspectives toward

northern social research among these groups, and certainly internal division regarding perspectives, but some general patterns can be identified. The primary concepts for comparison and review are access to resources and results, prioritization of research needs, accountability, participation, and validity. All of these concepts have been discussed throughout the thesis, and all are vital to the links among methods, results, and regulation in the research process.

1. The Government View

The primary actor here will be the Department of Indian and Northern Affairs, designated here as DINA. The Government of the NWT (GNWT) will be mentioned briefly, as will the Science Council of Canada (SCC); although the latter body is not itself directly involved in regulation, and serves often as a voice for academic opinion, it has provided advice to government in this area.

Access: As noted in previous discussions, DINA has often attempted to intervene in the conduct of research, and in the publication and dissemination of results harmful to its programs or policies. These attempts have not been part of a master plan or policy, but they have been part of a piecemeal effort to establish control over the use of the products of social research. This effort has been duplicated in numerous bodies and committees, each lacking in an ability to coordinate the flow of science information and to set a comprehensive science policy, and thus making control of science use a particularly contentious issue -- to the extent that research has been deemed important at all. Even the partially effective ACND sub-committee on Science and Technology, perhaps one of the most important research bodies, has suffered from a low esteem for research within government, particularly for social research (researchers p.c. July 16, 1982; Dec. 8, 1982; G. Francis 1983: 11).

The term "coordination" has been used most frequently to describe access to research funding and information; it has many connotations, some of which imply

control. This has been part of the mandate of most DINA-sponsored bodies related to research, from the early Northern Research Coordination Centre to the recent Science Institute. There have been, however, jurisdictional disputes within government about where (either within DINA or outside its structure) the focus of coordination should lie, as revealed by the discussion on alternative frameworks for regulation. The history of the Inuvik Lab has been fraught with difficulties relating both to its role in coordinating local research and to the duplication of efforts by various agencies monitoring the Lab itself.⁵² Much northern research is outside the jurisdiction of DINA, of course, and much of that sponsored by training grant or contract cannot be directly monitored by the agency.

Yet, because part of DINA's mandate is to foster science, it has frequently argued that it should have a central role in controlling access to northern research through policy and/or an institution. Although the creation of a science policy has been part of the mandate of the northern research office within DINA, and frequent consultations have been held to this end, most attempts have been nebulous and without a framework for implementation (Kubiski and Associates 1983: 57; Dubé 1983: 2). The 1976 ACND guidelines were an attempt to coordinate science by rulebook; they fell short of being a coherent policy and were unenforceable (Faulkner 1978: 5). They restated the then-existing structures within government, such as the coordinating role of ACND, and they did not build a rationale in themselves for coordination (Larkin 1978: 125). The various proposals for a science centre included forums for coordinating actors and enforcing policy, primarily by controlling funding and reporting. The disputes over jurisdiction with the GNWT involved assertion of rights to translate coordination into the direction of science programs to meet particular interests.

Priorities: The twin of coordination/access is the setting of research priorities through policies and institutions. Again, DINA's efforts here were never

well-integrated, but, rather, were responsive to immediate needs of agencies within government. DINA has increasingly demanded that research meet its own short-term program needs, as we saw in Section A, rather than broad, interdisciplinary objectives addressed to the capacities of science and the needs of the North. As we have seen, the federal government adopted the theme of "balanced" development in the North in the 1970s, and, as Jean Chrétien, Minister of DINA in the early 1970s argued, science was to play a role in balancing northern and southern priorities for development (Chrétien 1972: 272-75). Northern and southern expertise, needs and personnel were to be in balance; long-term basic research was to be matched with that which could be applied to the development of the North. The 1976 Science Guidelines were also modelled on the balance theme. But since the terms of balance are not described and the use of the concept of "northerner" is not defined, the scale can be "balanced" to meet short-term institutional needs; a "balance" is a promise that the scale can always be weighted in another direction in future. Policy thus becomes a rationale for a mechanism by which the government's flexible needs can receive science advice, rather than a commitment to prioritizing research funds. A "balanced" policy is a framework without commitments.

Accountability: By the late 1970s and early 1980s the federal government was devolving responsibilities to territorial governments. A new buzz-word began to appear in science policy statements. Research must be "responsive" to the North and its people, rather than in balance with them. This term also clouds issues of implicit control and priority. Responsive research is relevant research, but it also implies that science may react to immediate pressures rather than to on-going needs. Still, this new direction opened the door to the possibilities of more northern input into priorities. The second objective identified by Greenaway's consultations for DINA's Science Institute proposal was that science "respond to the interests and needs of northern residents" (DIAND1979: 3); such a structure should be "responsive and flexible." One of the goals

of the Northern Affairs Program (DINA) Operational Plan for 1982-83 was "to make northern science and research more responsive to the needs and concerns of northerners" (Kubiski and Associates 1983: 63). In 1983 the SCC also supported these statements, suggesting that science policies should be responsive to northern-defined needs (G. Francis 1983: 1). These statements pave the way for science accountability, but fall short of specifying a means for this to occur or an assurance that responsiveness be viewed comprehensively.

Participation: Part of accountability is participation, both the participation of scientists in the North and the participation by northerners in science. We have already argued that the model for social science has been the methodical participation of southerners in northern life and in creating concepts of northern "culture." The first two ACND (1976) Guidelines stated that Native people must be encouraged to participate in science activities and that there should be prior consultation, informed consent, and feedback as steps to the research process. Thus participation can mean these support procedures, or it can mean use of northern assistants and knowledge, and/or it may signify the involvement of northern or Native people in planning and prioritizing research. In the 1970s participation was largely limited to licensing consultations, some feedback, and occasional consultations about science policy. Consultations have taken place with the leadership and at community meetings, with the result that participation has often been shaped by southern rules and norms of expertise (see Chapter Five).

This has been gradually changing in the 1980s to an involvement of northerners in the planning process itself, as we will further describe in the next subsections. The 1979 discussions of a northern science institution framework recognized participation in all aspects of science, including conduct, as well as a need for a locally-based cadre of scientists. The latter idea has become fundamental to the Science Institute and the

northern college system (see Senkpiel and Easton 1988; Janes 1987; Bielawski 1987), and has been intended to facilitate the growth of indigenous science, the acknowledgement of northern expertise, and long-term communication between northern peoples and the science establishment.

The GNWT has been a leading proponent of more extensive participation, arguing that northern people should have the control over access and priorities that DINA had long sought. They have enlarged the concept of participation to include planning, reporting, and ethical conduct as modes by which northerners increasingly set the parameters for "responsiveness" and "relevance." We have seen this in the Legislative Assembly's concern over licensing and the successful efforts by the GNWT to get northern science institutions physically placed under territorial control. Through the SAB and the new Science Institute, the GNWT has encouraged long-term stays by scientists (where applicable) and the training of northern residents to assist in research (Upton 1983: 1; Janes 1987; Bielawski 1987; Arnold 1987). The Arctic College, the Prince of Wales Northern Heritage Centre, and special programs like the Northern Heritage Society (see below) have contributed to this as well.

Validity: Both DINA and the GNWT have perceived science as providing advice and data resources for government planning. DINA continues to sponsor some in-house research to this end, and the Science Institute is responsible to the Legislative Assembly; of the two, the GNWT has been prepared to decentralize such planning and advice to the northern community level. The most persistent problem with this aspect of participation and accountability has been, of course, that science has often been used to rationalize existing policy rather than to mould it, as we have argued. This has meant that access to resources and research priorities has been validated by those who set the terms for participation and accountability. Science is valid if policy is valid, therefore; while the inherent value of science is recognized, this very value, with its internal

intellectual legitimacy, is useful insofar as it supports planning goals (see Rowat 1976). Otherwise scientists, as we have suggested, are accused of being biased and of producing faulty research -- of letting participation get the better of them.

Both the Science Council and ACUNS have been perceived by the federal government as advisory bodies in spite of a prevailing view of the independent expertise of science as a guide to policy. ACUNS has had disputes with DINA's Northern Affairs Program over its role. ACUNS was funded to provide information and advice, to represent the network of northern scholars in giving this advice, to coordinate science in the institutions, and to provide special projects and services. We will return to this in the next subsection. ACUNS has resisted attempts by DINA to narrow these terms, including matching training grants to government planning objectives (Kubiski and Assoc. 1982: 68; Kubiski 1980: 5-9, 22-27).

Similarly, the SCC grew out of the Science Secretariat of the Privy Council Office in 1967. Its purpose was to examine the potential of science and technology in federal decision-making (R.W. Jackson 1976: 12-13). It is a southern based agency, which seeks information and opinion from prominent scientists in government, universities, and industry, and its northern development models provide an elaborate validating rationale for the involvement of science itself in the North, as well as for progressive, controlled development. Its recommendations are not necessarily followed, but they can be used to add legitimacy to the given role of science -- consulting northerners and others in order to be consulted. In its 1977 report on northern science, the SCC did argue that the 1976 ACND Guidelines did not sufficiently reflect northern or Native goals and that there is a need for more northern participation in the research prioritizing and validating process (1977: 74-75). Although the Council did not foresee the developing political power of Native northerners in controlling science, it did outline an intermediary role in validating knowledge that the academic establishment can take

between government and the Native population. We will turn now to an examination of that role.

2. The Academic Perspective

Again, some generalization is necessary here. In essence, this section discusses the perspectives that university-based researchers take concerning the role of the universities in northern science. In addition, the focus will be on social science although some policy statements do not make a distinction among the branches of science. Social science, through the academic perspective, is described as science perpetuated through university-based research and the guidelines of disciplinary scholarship. Delta researchers have included many who have had post-graduate training in their research fields, whether they have worked for industry, government, Native organizations; the nature of this employment may or may not affect the scientist's conceptualization of his work, including the regulation of it, so that clear distinctions are hard to make. Nonetheless, there has for several decades been a voice from the universities about the kind of science they endorse and the perceived role of that science in northern problem-solving.

The primary actor here will be the Association of Canadian Universities for Northern Studies (ACUNS), which was established to represent the universities in northern research and applications.⁵³ Some data also come from interview and from other documents from agencies such as the Social Sciences and Humanities Research Council (SSHRC). The Canadian Arctic Resources Committee (CARC), a citizen's group which plays an advocacy role in the North, usually on behalf of Native people, also has links with the academic world from which it draws much of its support. Thus the full panoply of academic perspectives cannot be covered here, but an indication of the range of issues can be made.

Access and Priorities: The role of the universities in northern studies has gone through several incarnations since the first Northern Research Conference was held in 1967 (a predecessor to the formation of ACUNS). It was argued at that first meeting that science should be independent of government and of the North, providing information and advice. Funding should be allocated through the university system rather than through political forums, argued J.K. Stager, a prominent social scientist working in the North (Kupsch, ed. 1968: 108-13). This kind of political neutrality was echoed in later meetings; by 1978, when ACUNS was founded, the Research Committee argued that the 1976 ACND Guidelines did not give an adequate role to the universities (ACUNS Annual Conference Proceedings 1978: 40). The Committee continued, in 1980 and 1981, to argue for long-term, fundamental, university-based research which could be used to make informed choices for rational policy in the North (ACUNS Annual Conference Proceedings 1981: 94). At that time a proposal for a Northern Research Council, which never saw fruition, awarded universities the task of reviewing and assessing Canada's northern science needs. ACUNS did not perceive itself as a regulatory body, but wanted academia to play a "neutral" role through bodies like the Council in assessing and accessing science resources.

The kind of independence that northern scientists sought was one where they could control access and priorities for science; research should be prioritized by the dictates of science but also by the practicalities of application to northern problems. For instance, reaction by scientists to the 1974 GNWT policy of requiring community consultations for licensing has been mixed. Some have felt that northern communities should have this stake in research access (see Poonwassie and Sprague 1982: 10-12; Bielawski 1984, 1987; also researchers p.c. Dec. 9, 1982; Dec. 10, 1982; Dec. 7, 1981; Dec. 6, 1981). Others were cautious, warning that northerners should not be allowed to participate, uninformed, in the justification of science (researchers p.c. July

16, 1982; Oct. 4, 1982; May 19, 1982). Scientists ideally wanted the right to make these deliberations and set priorities for funding. Yet they knew also that the federal government held the levers of funding power, and that their independent role consisted largely of making research accessible to those who could make use of studies. It also involved a stance of objectivity, to which we will return below. Many researchers from the institutions began to plea for increased "relevance" for research in meeting northern needs, and to confront the government on issues of legitimacy. This meant a new definition of role in terms of participation and accountability.

Accountability and Participation: Thus in 1983 the Executive Director of ACUNS stated at the Annual Meeting that scientists can no longer assume that their independent role can be considered "relevant" and "responsive" (Judd 1983:1-2). Scientists are not outside the "system" of northern science and policy, but they are part of it. ACUNS has come increasingly to phrase its role as one of intermediary between the southern institutional establishment of science/government and northern peoples, and indeed between science and government itself. This is in line with ACUNS objectives and activities in overseeing the Grants program, the organization of conferences, and the integration of public and private sources through its funding arm. A 1983 assessment of ACUNS concluded that it was most successful at that time in providing a dialogue between northerners and southerners (Kubiski and Associates 1983: 64-66).

The suggestion of an intermediary and ombudsman role came in ACUNS' reaction to the proposals for a Science Institute and for a National Polar Institute.⁵⁴ The membership tended to see the Institute as unrealistic. They did not see that the interests of northerners could "balance" with those of government, but they did perceive the universities as potential intermediaries to reconcile the interests of the other actors. Northern expertise needed to be fostered, but not without the guidance of science, they felt. This could be done by training northerners to do research using the methods of

science and by establishing a body of researchers as northern residents. In 1983 the Research and Field Facilities Committee further suggested that ACUNS might take an ombudsman role in adjudicating disputes between researchers and communities, and that there should be a clear separation between the purposes of the community in regulating science and the purposes of science itself (ACUNS Annual Conference Proceedings 1983: 1-3; Northline April 1983: 2).

We noted in the previous subsection that the GNWT has supported a concept of science participation which includes the growth of indigenous science in the North. Both the training of northern people as scientists and assistants, and the permanent residence of southern-trained scientists in the North, would provide an opportunity for northerners to build relationships with science, and it would facilitate a local science capacity to meet immediate needs for research and application. These ideas have been supported by many university scientists since the 1960s. They were an integral part of University of the North proposals and of repeated ACUNS discussions. (Salisbury 1975: 9-10; M. Lewis 1981; Harrison 1983: 4; Rees 1986; Lockhart and McCaskill 1986; Bielawski 1984, 1987). Karl Francis, for example, piloted a project in the western Sub-Arctic in the early 1970s which trained and used Native assistants (K. Francis 1973). The training of researchers was also advocated through a resolution at the 1979 Annual meetings of ACUNS and at the 1979 seminar on the Science Institute concept (ACUNS Annual Conference Proceedings 1979: 3; DIAND 1979).

The training of northern scientists has been regarded as a step toward developing a northern institutional regulatory base for science. It is also a step toward collaborative research, and this is another evolution in the role of the university. In some instances the researcher participates in the system as a facilitator rather than, or in addition to, a role as intermediary. This means that the university researcher helps the northern community to do its own research, based on their priorities, assisting in

planning, training, and analysis. Participants in a series of workshops sponsored by SSHRC in 1982 to plan a program in Native Issues (which did not materialize) discussed this kind of role, as well as training Native researchers, as part of community-based research, although they debated the means by which it should be accomplished (see Poonwassie and Sprague 1982 and SSHRC reports Nov. and Jan. 1983).

We will return to this in the next subsection, but one aspect needs to be broached here. How will the academic establishment be involved in research regulation through a facilitator role? Who will set the guidelines for participation, and who will evaluate the results? If scientists train northerners to do science, it can be argued that they are imposing southern perspectives on knowledge as well as southern institutions. The issue of control is critical in a political sense, but it is also central to the idea of validity. The more that communities control access, priorities, and, in turn, the funding of research, the more they will exert political control of the research process. If so, the control that the facilitator exerts in community based research is primarily one of validity, and a tenuous battle of world view through collaboration of knowledge.

Validity: The science establishment is very aware of this issue; indeed, science has long defined its role in terms of its rights to determine the "objective" legitimacy of knowledge. The role of facilitator can be far removed from the independent, neutral assessment role often envisioned. Participation as validity is at stake, and the scientist/researchers can use their training to instill criteria of validity for both process and product of community research. The social sciences, particularly anthropology, represent a separate case in relation to participation, since participation is integral to some fundamental research models. We have seen how the participating researcher occupies a dual stance similar to the intermediary role, operating to integrate two existing frameworks of knowledge into a new one. The participatory model stresses duality and the reciprocal negotiation of information and validity. As northerners

demand a more dynamic role in controlling the process, social scientists are having to recognize that intersubjectivity may be redefined using other than the traditional science criteria of objectivity. They are recognizing that participation must acknowledge control and its context in the meeting of minds and objectives, and that the difference between pure and applied research is slim as long as research takes place within a political frame which shapes its conclusions.

Participatory control, then, will ultimately involve the criteria of validity and legitimacy. These will include accountability and commitment, and the demands of accountability are being used by Native people to measure social science, as we will see. For social science to regulate science through validity, it must be aware of the context of participation in each community, and social scientists must ground method in these relationships and responsibilities rather than in a rational epistemology. The model of P-O, as we will see when we conclude our deliberations with discussions of community-based research and ethics, may become the tool of its own transformation.

3. The Native Perspective

The Native actors in the research process have no single organizational voice; instead, the perspectives relayed by some northern Native organizations will be discussed here, including secondary information about them from publications by outside researchers. Some reference will be made to information about community-based research in southern Canadian Native communities as well. As has been true throughout the thesis, the terms "northerner" and "Native" are sometimes used interchangeably in documents. To the greatest extent possible, the Native northern perspectives will be singled out for our purposes.

It should be clear from preceding chapters, especially Chapter 6, that Native views of the research process are often different from those of southern-based

institutions. This comes from different knowledge traditions, but also from Native experience as passive parties in the colonialism brought about by these institutions. The more experience with social research that Native groups have had, the more they have come to realize how it is controlled -- and the more they want a stake in the regulation process. They have often rejected southern standards of independence and validity in science, the pressures on their time and resources demanded by research, and the kinds of applications of science used (or not used) in the past. Instead, they want control over their knowledge and environment; they want practical results; they want respect of privacy; and they want immediate responses to the pressures of rapid change (see Kubiski and Associates 1983: 64-5; Lange 1987; DIAND 1979: 2-4).

Access and Priorities: Native organizations have argued for an increased role in the science process on several levels, including licensing/community access; funding of research; management of research projects and facilities; and access to results. As we noted in the first section, Native people in the North provided the push to get community consultation as a necessary step toward licensing, so that people could learn about projects in advance and make decisions about participation. In 1974 the Inuit Tapirisat passed a resolution requiring all social scientists wishing to work in member communities to seek permission from representative local and regional organizations (Marquand 1975: 29). At the time of fieldwork, Igloolik, which also had a federal laboratory, was turning away projects through the Community Advisory Committee because they felt the Committee was not sufficiently involved in science activities (P. Lewis 1981: 6). In a submission to the SSHRC planning process for the strategic grants program, the Dene Nation recommended that SSHRC not fund any social science projects among them unless they were conducted under the auspices of the Dene organization (SSHRC Division of Planning and Evaluation, Jan. 1983: 10). Other communities in both the North and the South have at times denied permission to

researchers, particularly anthropologists who are considered threats to privacy (see for example Poonwassie and Sprague 1982: 15-16).

Licensing is only one part of a comprehensive approach to research access, however. Deciding which researchers to allow is for many Native groups an overly passive process. They are increasingly asked for an increased role in research planning and funding. As early as 1971, at the UCANORTH conference in Inuvik on the idea of a northern university, Native participants insisted that it be managed by their own people (Kupsch and Caillol 1973: v). Most participation has to date been at the project level, in community-based research, but even here Native groups have run into obstacles. Native organizations have to compete with other actors for funds research funds, and in some cases they have found that the studies that they would like to see done in community assessment and planning have been difficult to fund because they are more inter- or multidisciplinary than funding guidelines allow (Hedley 1986: 93-4; Lockhart and McCaskill 1986:165-66; Lange 1987: 18). The alternatives are for Native groups and communities to control block funding for research, for them to sit on funding bodies, and for them to work jointly with academics in accessing funding on a project level; the latter will be discussed in the next section on participation.

One alternative is for organizations to set up their own research institutes or consulting corporations. We have noted examples of this in Chapter Four. Most northern Native organizations have staff allocated to work in culture, language, and education. The Dene Nation has the Dene Cultural Institute, established in 1986 to work on traditional culture, including research in oral tradition, environmental knowledge, traditional medicine, education, and cooperation with scientists using traditional information (Native Press Feb. 19, 1988: 23; Nov. 13, 1987: 21). The Makivik Corporation, created as part of the James Bay and Northern Quebec Agreement for the Inuit of Northern Quebec, set up a research unit to design and execute studies, to attain

skills and expertise, and to use Inuit knowledge and concepts. It has hired non-Native researchers to help train staff to do project design, conduct, and grantsmanship. It has three research centres and has done work on renewable resources, land use, impact assessment, and cultural studies. The Avataq Cultural Institute of the Kativik Regional Government conducts cultural and archaeological studies (Kemp 1987: 39-42). The Inuit Cultural Institute, founded in 1974, has also facilitated investigations as part of programs in history, language, cultural heritage, and curriculum development (Lange 1987: 39-40; Shouldice 1987: 45-46).

Native groups are gaining representation in some bodies, such as the Science Institute, but they are far from being in a position to challenge the institutional dispersement of funds from agencies such as the Environmental Studies Revolving Fund (ESRF), SSHRC, DINA, or private foundations. In 1980 the Inuit Circumpolar Conference resolved that each member organization seek, from its government, representation in any bodies undertaking research affecting policy (Stenbaek-Lafon 1981: 200); recently, the Tungavik Federation of Nunavut tried to get the ESRF out of DINA's control and to get Inuit board members, but these endeavours failed (Lange 1987: 66-7). Native groups from the North and South proposed that they have representatives on the committees for granting and review on the proposed (and now shelved) SSHRC granting program on Native issues (Poonwassie and Sprague 1982: 31-34; SSHRC Nov. 1983: 17).

Some effort has been made to involve local Native people in the northern Labs, as mentioned. When the Igloolik Lab was constructed, an advisory committee was set up to advise the scientist-in-charge on community relations, and a resident was appointed to the committee to choose this official (Rowley 1975: 15; researcher p.c. July 16, 1982). Relations with the community have not always been good, as residents have felt that only token efforts have been made to involve them in decisions (Warner 1975).

Similar problems have occurred in Inuvik; there have been no Native people serving even in an advisory capacity there. The first manager tried to involve people by actively informing them of projects and letting the community use the facilities, but gradually both parties lost interest (see Warner 1975: 80-87; Espie 1980: 23-25). At the time of fieldwork, most residents had little idea of the operations of the Lab, and local organizations were not involved, although some attempts were made to use the Lab as an instructional centre for the schools, and to communicate with the residents through the radio station (P. Lewis 1981: 5).

At the time of fieldwork, some local residents expressed interest in having a greater role in Lab management; indeed, the Science Institute proposal current at that time recommended a series of local management committees for the Labs incorporating both community citizens and scientists in planning activities and preparing budgets. One individual felt strongly that more local people should be employed by the Lab, as had been originally intended (p.c. Feb. 5b, 1981), but most Native commentators noted that there was widespread apathy toward involvement with the Lab which was born of continued non-involvement (p.c. Nov. 17, 1981; Sept. 22, 1981; Feb. 20, 1981). They pointed out that working in a consultation capacity was itself inadequate unless residents could make effective decisions about applying science to local issues.

Certainly, the Delta hosts and other Native groups have argued that improved access to funding, in competition with other recipients, was an important criterion in establishing a regulatory role for Native organizations. Some have suggested that organizations operate to channel research funding and researchers to communities, and that they play a role in setting priorities for those communities (K. Francis 1973; Rees 1986a, b; Upton 1983: 1; Janes 1982: 7; Bielawski 1984: 2; Hedley 1986; Lockhart and McCaskill 1986). This is intended to bypass the intermediary role of universities and to make academics joint or facilitating participants in research design. Community-

based research, which we will discuss further below, would become the groundwork for regulating the meta- structures of science and government administration, rather than the reverse.

This vision of community-based research has another aspect of regulation of access which comes at the end of the process, and that is the issue of access to data and analyses. Most contracts between Native communities and consultants/researchers include clauses outlining the disposition of data, and often communities retain the right to review drafts and disseminate results. This is a controversial issue, especially when academics also need to protect their input and rights to publication -- as well as their independent assessor role (Poonwassie and Sprague 1982: 20). At the very least Native communities want to ensure that feedback occurs and copies of reports are returned North (Lange 1987: 23-4, 28, 40, 53). In broader terms, participants must decide how the results are to be disseminated and used (to the extent that the latter can be controlled outside the immediate jurisdiction of the community). Based on a joint academic/Dene study, the Dene Gondie project discussed below, Rees (1986: 155) has presented recommendations to academics about working with Native communities, including allowing them the right to review and interpret manuscripts, working out contracts about the use of results, and assisting Native actors in getting their own analyses published.

Participation: Participation by Native groups in research is of course part of the question of access; total participation means involvement in research from planning to conduct to analysis. The focus here will be on conduct, particularly on two phenomena already noted: the training of northern Native research assistants, and community-based research. The latter incorporates the former, but training alone need not take place in the context of a particular community-based project.

Native people have supported the idea of training local researchers for many years, and regard the accumulation of skills as one of the tangible benefits of participating in research projects (Lange 1987: 23,28, 43; K. Francis 1973). D. Whiteside completed a survey for DINA of Native response to social research in the mid-1970s and recommended a program of training Native people; the program would combine scientific method with personal strengths and communication skills to facilitate a blend of insider/outsider expertise (Whiteside 1975: 2-12). We have already noted that the GNWT and the Science Institute have for several years supported programs whereby local people were funded to assist scientific parties; this was going on at the time of fieldwork through the now-defunct Science Advisory Board, for example. The Prince of Wales Northern Heritage Centre has facilitated the building of northern science skills through programs in local museum assistance, an internship program, teaching at Arctic College, and field training programs (Arnold 1987: 55).

One of the most successful programs has operated in northern archaeology since the late 1970s. The Northern Heritage Society program began when two archaeologists working on Somerset Island, Ellen Bielawski and Sally Cole, began to hire northern students to work as field assistants. They were concerned about negative Native reaction to social science, and felt that communities should know more about activities. In addition to community consultations and meetings to explain their work, the women felt that the hiring of young people as assistants would not only increase local understanding, but it could contribute to the personal and professional development of those students (researcher p.c. Dec. 7, 1981).

The two archaeologists sought funding to begin a regular summer field school, and the project became part of the Northern Heritage Society, a non-profit organization, in 1981. In addition to running the field school, the Society has instituted an ecology and earth sciences program, and it attempts to place its students and other northerners with

scientific parties needing assistance. Students in the archaeology field school have come from across the Arctic; they are between 17 and 22 years of age and are chosen on the basis of interest and personal characteristics such as independence. The project involves training in archaeological theory and techniques, feedback of information to schools and communities (meetings, displays, curricula), and the establishment of long-term relationships with communities. (Todd 1981: A11; Bielawski 1984, 1987; Northline Jan. 1986: 5). It was hoped that, through the building of these participatory relationships, better cooperation and communication would result in appreciation and preservation of northern cultural heritage. The people (mostly Inuit) gain an appreciation of what science does, which will hopefully enable them to make informed decisions about science and their participation in it. It will also allow university-based social science to increase its legitimacy in the regulation process (Cole and Bielawski 1981: 1-14; Bielawski 1984: 3-4, 1987; researcher p.c.Dec. 7, 1981).

A number of examples of community-based research now exist in Canada, including northern Canada (see Hedley 1986; Lockhart 1982; Poonwasie and Sprague 1982: 12-19; Lockhart and McCaskill 1976). Although each project differs according to contract and objectives, community-based research is usually that which is regulated from the community, and it often includes academic facilitators, locally-training investigators, and community/committee roles in planning, funding, designing, and analyzing research. Many of these use academic researchers as partners and facilitators. The university-based researcher or the consultant may help design the project, seek funding, create survey instruments, train fieldworkers, and analyze data. The local community or organization frequently designates committee members who will work with the academics in prioritizing research, informing and consulting the community membership, selecting and training fieldworkers, and reviewing data interpretations. The amount of control by each actor is negotiated, and this can be a

source of difficulty as these relationships are worked out and funding is allocated. If community-based research is predicated on control of funding and reporting by outside agencies, partnerships can be aborted if local communities cannot see projects through from design to completion (see Hedley 1986: 98-99; Lange 1987: 20-23).

One of these projects took place near the Delta, in the communities along the Mackenzie River affected by the Norman Wells pipeline. The Dene Gondie project, mentioned in Chapters Four and Five, was a collaboration between the University of British Columbia School of Community and Regional Planning (SCARP) and the Dene Nation. The Dene had received a \$1.25 million allocation of funds for impact assessment and monitoring as part of an impact compensation package for the Norman Wells pipeline, and this is one of the projects which came out of this funding. SCARP approached the Dene in 1984 with the idea of a partnership. The Dene were then having problems accessing the funds, and a DINA-sponsored impact monitoring project was floundering because several Dene communities were boycotting it over objections to design and procedure. Once the funding was secured and DINA cancelled their own fieldwork in boycotting villages, the Dene began a collaboration process. An initial meeting led to a workshop attended by SCARP and representatives of local organizations and communities affected by the pipeline. A joint Steering Committee was set up on the basis of the recommendations made in the workshop, and the terms of the project were focussed on the experiences of Norman Wells workers and the perspectives of community members on impacts. Two surveys were designed and tested, and Dene fieldworkers were trained (Rees 1986).

The roles of community members and SCARP staff had to be clarified, as well as the disposition of results. For instance, the SCARP staff members designed the survey instruments, and the Dene fieldworkers tested them. The Dene had the right to make decisions about field conduct and about the fundamental direction of the operation. The

project was regarded as successful by both Dene and the academic researchers, apparently; the latter had access to data derived from accountable methods, and the former had a chance to direct and participate in a project whose results could be meaningful for their own planning (Rees 1986). As suggested above, however, partnerships do not always readily succeed simply because each actor frequently resorts to more conventional and comfortable assumptions about roles. Scientists are reluctant to give up their independence and hierarchical positions in the institutional framework in order to train inexperienced assistants and to let Native people without science knowledge design studies. Many Native people are in turn reluctant to challenge those institutions which have always done the work for them, and often they see science as too firmly based in non-Native culture to be interesting to them (Bielawski 1987: 30-31; Lockhart and McCaskill 1986: 162).

Several factors enter into the success of Native participation in social research, then. Fieldworkers must be chosen who in fact respect the rules of P-O. In addition to training and interest, they must live in the area, know and get along with residents, be accessible, and treat their informants as subjects, not as objects (Bucksar 1969b: 20). They may have additional expectations placed upon them that they will get along with the people better than outsiders will, but at the same time they may be criticized for lack of "objectivity." Partnerships with outside researchers/analysts can help take the burden of the dual stance away from the single individual, but intersubjective communication is critical, as is the definition of validity, as we will see. The self-awareness gained by the capacity to generate data can contribute to reflections and the growth of insight by both fieldworkers and residents, provided they take this responsibility seriously, too (Lockhart 1982: 115-16).

Accountability and Validity: The nature of research conduct is basic to these relationships and their evaluation by actors. The overseer role vaunted by government,

and the intermediary assessment roles assumed by academics, will be challenged as Native hosts come to define the terms of conduct by their own regulatory endeavours. Two facets of regulation are at stake: political control over information flow and utilization, and assignation of validity of research interpretations (the latter is especially critical in social science). The former is accountability, to rules and relevance and responsiveness, and it may be in the end inseparable from validity itself. Native people are asking that social science be available for their own priorities, and they want to incorporate science procedure into their own "realities" about their universe. This means placing their organizations and representatives in roles which, if they do not supercede government control, will at least mediate between the community and the national science structures, including universities. It also means that rules will be set for accountability, and these will include the distribution of responsibility and reward. They will include the objective bounding of knowledge and the evaluation of the relativity of knowledge. Access, planning, feedback, and collaboration are all part of the legitimacy of process, and each actor must be given roles according to their perceived responsibility in each aspect of the endeavour.

This reshaping of roles is in itself a reshaping of the context of participation, a bounding of learning through recognition of objective rules of involvement. Participatory methods may be evaluated by both Native and science rules, and by both traditions of knowledge. Native response is shaping P-O as ideal as well as reality, and social scientists who respond to these rules may find their dual stance accorded more "objective" legitimacy. As part of the monistic, intersubjective bounding of knowledge asked by Native people, the recognition of Native forms of knowledge is crucial, as we saw in the last chapter.

Native people are now asking that all science realize the integrity of their knowledge of themselves and of their environment. Recent progress has been made in the

area of renewable resource management in integrating science and Native perspectives on wildlife behaviour, for example (see for example Freeman and Carbyn, eds., 1988; Green and Smith, eds., 1986; Freeman, ed., 1981; Wonders, ed., 1988). This does not necessarily mean incorporating Native visions into scientific truth, or contrasting the epistemologies, but it does mean respecting alternative rules of evidence in evaluating data and answering research questions. Joanne Barnaby, a Native woman from the Dene and Metis Negotiations Secretariat, addressed the 1986 ACUNS meeting in Yellowknife by saying, "your science and my science can come together in a mutually beneficial way." Barnaby asked scientists to recognize their "incompetence" in understanding the North as indigenous residents do in order to open their minds to other forms of knowledge (Sankey 1986: 3).

While some academics see this perspective as simplistic, others endorse the need for social scientists, particularly to recognize this knowledge and its bases (M. Lewis 1981: 222; Cruikshank 1984, 1988). The abstract and universalistic frames of the scientist can recognize the concrete and experiential references of a Native view, says A. Lockhart in reference to a community-based research project with the North Coast Tribal Council (1982: 164). He has advocated a "concept of learning which places the very different knowledge frames possessed by outside consultants and inside clients on an equal footing and that eschews pretense and risks conflict in the belief that deep insights leading to new possibilities will eventually emerge in the context of growing trust and mutual appreciation" (p. 167). Social scientists can also make use of Native knowledge in the pursuit of their own objectives and methods, such as the use of folklore in ethnohistory and archaeology (Cruikshank 1984: 1-19; Bielawski 1984: 5). This should not destroy the internal credibility of that knowledge, however.

These alternatives to using and respecting Native knowledge represent real challenges in participation and in the genesis of interpretive understandings. The two

frameworks may be partially reconciled through the use of the regulatory process, including ethical appraisals of conduct, as we will see, and the setting of rules of accountability. Ideally, long-term research in a partnership framework will allow for gradual growth of intersubjectivity. Native people expect that the scientist will add legitimacy to the mutual understanding by their expertise and perspective, as was argued in the preceding chapter, just as the scientist adds legitimacy from acceptance of his version of "truth" by his hosts. Each tradition of epistemology can gain reference and credibility from the other by negotiating the rule of accountability, thus bypassing some of the pressures for a rational reconciliation of dualism. Participation on the community level can transform itself into new sets of formulas for knowledge generation.

As long as this intersubjectivity and communication fails, as long there is not equality and respect, Native people will continue to see the two kinds of knowledge as dualistic and relative to each other. The scientist will be accused of failed commitment. "Ideological relativity" may result from an emphasis on the relative differences between the two kinds of knowledge, and this can become a political tool for arguing for a role in the regulatory process. In essence, regulation is a means to establish epistemology and validity; it is control of crucial dimensions of conduct. The social scientist can continue to have a stake in validating knowledge through science, but only by negotiating his participatory roles with government and with Native people so that validity is a jointly accountable, rather than a relative, endeavour. None of the actors needs to control legitimacy itself, but they must have a stake in the process which produces it. Social scientists will be forced to account for Native involvement in the legitimizing process through an expanded understanding of their own facilitating role and through an awareness of the participation of hosts in science process.

In the final chapter we will link regulation with process through a discussion of research ethics and ethical codes. In addition, we will link the case study with the theoretical framework to suggest alternate perspectives on P-O and response in the Delta.

CHAPTER EIGHT: SUMMARY AND CONCLUSIONS

In this final chapter the various parts of the thesis will be drawn together. Several themes run through all of the preceding chapters, including the concepts of objectivity, relativity, culture, accountability, validity, role, image, duality, marginality, and regulation; all of these are integral to participant-observation as process. We have redefined culture, objectivity, and relativity in relation to participant-observation. This was done in order to produce a framework for understanding participation which will allow a more intersubjective truth than can be obtained through the singular efforts of the researcher/participant. We have also suggested that regulation can facilitate this intersubjectivity through providing avenues for communication and assessment of validity. The first section of this chapter is devoted to a discussion of ethics as a form of regulation. The second section will re-examine the framework of participation to explicate the history of Delta research and response.

It should be noted once again that the field area need not be taken as representative of all field sites where anthropologists have used participatory methods. In each situation the history of the area, the assumptions and experiences of the hosts, and the personality and behaviour of the fieldworker form a unique blend. For instance, the model of summer P-O found in the North is born of climate, accessibility, and university schedules. There is nonetheless a general pattern of interpretation which has come to characterize not only anthropological views of P-O but the facets of host interpretation; this is partly a result of similar associations of research with colonialism cross-culturally and partly a function of method itself. It is the process of

participation and its consequences which interest us here, and it is hoped that by an examination of this process a framework can be devised which can be used as a guideline for interpreting one particular field situation and assessing the way in which research participation has functioned within that setting. The framework can be "tested" by such applications, but it need not be proven -- or disproven -- for all situations, since it is an interpretation rather than a series of hypotheses. There is obviously a thin balance between generalization and example, as is usual in social science, which sometimes leans toward universalism and sometimes toward insularity. The reader is given the task, then, of exploring the potential of ideas presented here for application elsewhere.

A. ETHICS and PARTICIPANT-OBSERVATION

Participation in regulation, as we have seen, has taken place on different levels. The inclusive frameworks suggested by some regulators, primarily government, are on a "meta" level and are largely external to the science process. They place both researchers and hosts in an advisory relationship to a body which sets standards and priorities for all actors. On a somewhat different level, the mediating role suggested by universities and some Native organizations encompasses actors on a more egalitarian level. That is, the scientists tend to believe that the promotion of science in an advisory capacity is a primary value, but the contributions of all actors to the regulation of this function are valued equally.

A third level is that of the host community, wherein regulation as participation takes the form of accountability of interpersonal conduct at all stages of research planning, conduct, and utilization. The model grounds participation in the political context of northern lives and aspirations, using this as a guide for regulation and legitimacy. All of these levels concern the structuring of relationships among actors, including the objective recognition of knowledge and relative appraisals of its value. A system of ethics for northern research must be concerned with the responsibilities of

actors to maintain relationships on at least one of these levels, and can provide guidelines for the researcher's relationship with other actors on all levels. The anthropologist is particularly vulnerable to ethical concerns because he must often translate hierarchical institutions of science and policy into egalitarian modes at the community level, and the "grounding" of method in community is particularly crucial to the interpretation of his legitimacy and expertise.

1. Defining Ethical Concerns in Social Science

Interest in ethics in anthropology has been present throughout its professional history and has been an integral part of the institutionalization of the discipline. For many years anthropologists believed that scientific objectivity and truth were paramount and that most research activities could be justified if an objective truth could be obtained to be used for the general benefit of mankind.⁵⁵ There was, as we will discuss, a moral dimension to truth. In the 1960s there was a general revolt by many host groups around the world who questioned the general role of anthropology as participating in the mechanisms of power and subjugation through social inquiry. Hosts also questioned the justification for methods which they considered unethical. Questions about methods arose inside the discipline in company alongside scandals about the involvement of social scientists in government programs which could potentially subjugate the freedoms and well-being of host cultures.⁵⁶ Discussions took place about the ultimate responsibilities of researchers to hosts, to sponsors, and to science. Cultural relativity lost its urgency as moral code, as many professionals asked whether there were not universal standards of research conduct, or at least universal principles of moral responsibility (Hatch 1983: 131-32).

These debates led to an extensive literature within anthropology and sociology about the principles of ethics and their application to field methods. It is impossible to

recount this discussion here, but a few themes can be mentioned. Many P-O ethics discussions begin with some variant of the assumption, vital to P-O, that the researcher should not violate the "natural context" of the society of study (Chilungu 1976: 458; van Maanen 1983: 270; Dillman 1977: 407; Hilbert 1980: 61; Jay 1974: 371; Wax 1980: 276). Yet, most agree that the presence of the researcher is usually disruptive to some degree. Ethical concerns arise when the roles and behaviours of the "natural context" of social relations are betrayed to some purpose external to that context, such as the pursuit of science and of policy planning. Ethical codes, resolutions, and principles are intended to resolve conflict of interest according to the values of one or more actors; thus the definition of unethical behaviour can vary with the moral imperatives about the natural context held by the various actors.

For example, there is much literature on the issue of deception and covert role-playing. Unless the role of the "researcher" is considered a natural one, then often the outsider must play a role acceptable to hosts. The more the researcher plays the role, the more immersed into context he may be, and thus the research stance may be hidden within the role: covert role-playing (Emerson 1983: 258; Jarvie 1969: 506; Galliher 1983: 306; Hilbert 1980: 55). Some have argued that this is necessary in many situations to protect context and to obtain accurate results (Agar 1980: 184; Thorne 1980: 291). Although social scientists differ on the amount of covert activity they believe to be tolerable, most agree that there must be considerable human value in the results in order to justify covert role-playing.

Similarly, the issue of informed consent concerns the amount that the hosts know about the research role and about the use to which the results will be put prior to making a decision to participate. Again, opinions differ; some believe that a few representative individuals can give the consent of the group (F. Cohen 1976: 89-91; Wax 1983: 295; Thorne 1980: 292-93), and others believe that fully-informed

consent is neither feasible nor necessary in some instances (see Thorne 1980: 285-87; Agar 1980: 56). Like role-playing, this is a factor of the quality of the host-researcher relationship. It affects the quality of outcome, obviously (Jorgenson 1971: 327; Gouldner 1967), but it is difficult to define: how covert is covert; how informed must the consent be? Ethical disputes often centre on the moral dimensions of making these assessments, and they are very difficult to resolve out of context.

Social scientists have tried to put forth principles and codes which will help researchers make decisions and resolve dilemmas, but these principles have no universal endorsement. One principle, as we have suggested, is that knowledge as achieved by science is inherently good and potentially beneficial, so some variation in method can be justified to this end (May 1980: 358-59). Most social scientists now reject this as an absolute, arguing that science "truth" as applied to human behaviour is not separable from the political context of its discovery and use. Nonetheless, as we saw in our discussion of academic perspectives on regulation in the preceding chapter, many social scientists still assert the validity of this end and the importance of "balancing" host concerns with the independence of the scientist as assessor -- even to the extent of assessing ethical relativity itself. If ethical interference with objectivity cannot be dispensed with, then the social scientist should at least be aware of it and account for it in results.

A second principle derives from a Kantian dictate that the dignity and privacy of the individuals involved in research is the ultimate "end" by which methods must be evaluated. Of course, this version of the utilitarian principle of ethics does not necessarily conflict with the preceding one, as long as results are used to support this dignity and well-being, but it is recognized that "truth" may not juxtapose with the realities of achieving dignity and well-being. The scientist may not always control the alternative uses of his/her research, but he can control actions in the field. Thus

privacy and dignity must be regarded as legitimate ends in themselves and the methods of science, as well as the consequences, should respect them (see Cassell 1980: 36; Geertz 1968: 140; Jay 1974; Xiaotong 1980; Deloria, Jr.1980: 271; May 1980: 363-65).

This principle does not transcend cultural relativity in that there are still potential problems in defining the "good" of the host. The host is not inherently good and right, the researcher/sponsor bad and wrong. But the use of the principle may assist the researcher in his/her dual stance in understanding that he is not outside the "system" of science and power but very much a part of it. Equality, Geertz (1968) points out, is not usually inherent in fieldworker/host relationships, but it must be negotiated in the field as a quality of individual relationships. A respect for hosts' dignity and well-being, their "goodness" as they understand it, can perpetuate the development of this rapport.

Two critical questions derive from the application of these principles and are vital to ethical issues and regulation. First, the researcher must undertake some kind of risk/benefit analysis to determine the "good" of the ends relative to the means. Second, it must be determined who is participating in what: the researcher participates in host community contexts, as we have asserted, but hosts are also participants in a broader context of science utilization and regulation which encompasses them, as well as researchers, as actors. The researcher makes his/her assessment relative to that broader context, and as actors in it the hosts must also have a role in ethical decisions. Ethics codes, as we will see, attempt to frame the relative responsibilities of the actors in making decisions about participation.

The quantification of people in science is never easy; the quantification of morals of people in science is nearly impossible in the calculation of risks and benefits. Anthropology has often confused science "truth" with moral imperatives simply because the methods endorsed by science can be considered unethical by hosts; we will return to

this below in relation to the North. For example, Chilungu (1975,1976) argues that some science techniques like random sampling are unethical insofar as they treat all members of a culture as alike; detachment in the field can also be problematic from the hosts' perspective. The onus is on the fieldworker to evaluate data in relation to the techniques used to obtain it and to justify science objectivity as moral objectivity (Cassell 1980: 35; Appell 1980: 355; May 1980: 360). This adds to the dualist burden of the fieldworker; the individual investigator must evaluate risks and benefits to determine whether methods such as covert roles are justified in terms of potential and real benefits.

Some consequences can be predicted. Although most social scientists are fairly innocuous in context and are subject to the daily controls of hosts, it does happen that the researcher's presence can seriously alter patterns of behaviour, and this can often be observed. In some cases the researcher can know when publication of data might cause harm. But more often anthropologists have little control over the utilization of data, and they often do not have full information about local or institutional settings to make decisions about potential outcomes or about the moral imperatives of hosts (May 1980: 362-63; Cassell 1980: 37). Often, the best that can be expected is that researchers be willing to acknowledge the principles used to decide on "ends" and the value judgements used to evaluate "means." The process of calculation must itself be open to discussion (Agar 1980: 57; Hansen 1976: 133; Cassell 1980: 30-31; Thorne 1980: 288; Hessler, New and May 1980: 328).

If hosts are to participate in research context, they, too, must be able to make informed decisions and to examine the moral bases of their decisions. This process is regularly subject to factionalism and the operation of "gatekeepers" who regulate relationships with outsiders. Everyday decisions about whether to reveal or conceal information depend upon the decisions made in relation to the political structure of the

community and the role of the researcher within it: the "natural context." What happens then, if researcher and host calculations are different; what if they perceive different contexts and values to inform these decisions and make different judgements on the relation of "inside" behaviours to "outside" purposes?

Ethics disputes are resolved on an immediate basis in the field, through meta-structures regulating research process, and through public debate. Ethics codes as regulation have been derived to guide the decision process for both researcher and host community. Ethics codes began to appear in social science around 1970. Most professional anthropological societies have adopted codes since then, and these vary from brief statements of principle to elaborate procedural outlines.⁵⁷ All are written from the perspective of researcher professionalism, and all allow for the possibility of some form of peer review. In all cases, primary responsibility for resolution of ethical dilemmas is placed with the researcher. The codes respect the role of researcher as mediator, as egalitarian leveller, even though the researcher usually does not come from a power-neutral situation.

Some of the codes spell out rules of accountability, and the major dilemma again focusses on the use of principles for accountability. Some, like the code of the American Anthropological Association and that used by SSHRC proposal review committees, place priority on the individual rights of hosts to privacy and information. The code of the Canadian Sociology and Anthropology Association, on the other hand, implies that the pursuit of science has ultimate priority. Others, like that of the Society for Applied Anthropology, avoid the setting of priorities. All, however, encourage honesty and accessibility in relation to hosts, and all assume that the researcher will consider the hosts' interests and well-being in making calculated decisions.

The codes concern distribution and use of research data, then, in order to be honest but to protect privacy and the integrity of participation. To be legitimate, data

must be a product of a voluntary, competent dialogue; to be accountable, researchers must honour that dialogue even though demands for honesty and covers for privacy may conflict. The codes recognize the importance of field methods in the reproduction of "objective" truth through the individual researcher, but they have some serious problems. Unfortunately, they do not usually deal well with the relationship between the fieldworker's participation in the social setting and the hosts (community) participation in science policy systems, personally or institutionally. Codes may make the principles used in decisions clear, but they do not provide a mechanism for the application of those principles; and they often provide little guidance for risk/benefit evaluations according to those principles. In order to facilitate the mutual discussion of principles and methods, both researcher and host must be informed and both must have the capacity to enforce their own decisions. The codes must allow for a re-definition of participation away from a narrow view of the researcher as participant in the host community and as the instrument of ethical decisions. Rather, they must consider the second aspect of the application of principles mentioned above, the community's participation in the broader regional or national context from whence the research derives. Each actor has a responsibility for the outcome, and ethics codes must be placed within a regulatory framework which will facilitate informed discussion and the application of responsible decisions. In our discussion of ethics in northern and Delta research, we will again address the issues of responsibility and accountability.

2. Ethics and Northern Hosts

Many of the concerns about social research raised by residents of the Mackenzie Delta and other parts of the North fall into the realm of ethics. Social scientists also mentioned ethics, although several noted that interest within academia arose primarily in the 1960s and 1970s as hosts themselves reacted against "research exploitation" and as social scientists became increasingly aware of the restrictions put upon them by government sponsors (researchers p.c. Feb. 17, 1983; July 16, 1982; May 10, 1982). Prior to that time there was less professional self-consciousness about ethical principles.

The pattern of P-O which developed in the North was also built upon an assumption of a natural context, abetted by widespread public attitudes about a "natural" North as living laboratory. Researchers were never taken for insiders in the North (although Stefansson claimed to have been mistaken for an Inuk), but they have acted as teachers or writers and there have nonetheless been concerns about role-playing by Native northerners, as we saw in Chapter Six. Hosts have felt betrayed by the dual stance of the researcher. They felt that the use of knowledge outside the field context, not always fully understood, was a deception and violation of privacy. When the researcher role became defined, hosts were participating in the negotiation of role and status. They were limiting what the dual role could accomplish, although continuing negative reaction to researcher role and image by some reflects distrust of the investigator's commitment.

It is difficult to assess the degree to which investigators have used informed consent procedures in the past. This has become standard procedure in social science in recent years, but several researchers interviewed stated that they told their hosts what

they felt the hosts would understand or accept, and they admitted to some manipulation of role and image to facilitate access to natural contexts -- or they accused other social scientists of having done so (researcher p.c. July 22, 1982; Dec. 4, 1981; July 19b, 1982; May 9, 1982; Dec. 11, 1981). It was suggested to me by both researchers and some hosts that it was bad science to admit to being an anthropologist, because people would be too careful in what they said.

Yet many hosts felt that they had not been told the truth, and in my own fieldwork I decided that in spite of the limits imposed by the anthropological role that there was ultimately no point in investigating people's reactions to role deception by perpetuating the problem. Other hosts suggested that they did not understand because they were unfamiliar with the research context, or because of apathy. Both researchers and hosts espoused a basic principle of mutual respect; complaints about violation of privacy were complaints about the failure to respect the integrity of the individual's view of culture.

This respect was seldom automatic, however; it grew from observance of the rules of P-O and of local rules of hospitality and reciprocity. Consent was negotiated as a period of trial and error initiated each investigation. Until recently, many northerners have not felt that they had the power to formally reject a study, but they have used more informal methods of rejection such as ostracism and "fooling" the anthropologist. Consent by one group or individual does not necessarily imply consent by another, especially for Native northerners who believed that such decisions cannot be made on behalf of other people. The operative principle here seemed to be that the moral imperative of conflict resolution could emerge only from the consensus of individual members.

Power and equality were important issues in the North, then; northerners often resented researchers who stood on the power of a southern institutional base rather than

on the merits of their investigation and behaviour. Hosts were, as we have seen, afraid that information would be put to harmful uses; those more experienced in the research context realized that often studies were not used at all, but they too rejected any pretence of power. The egalitarian fictions of field interactions, past and present, were indeed often delicate. Friendships did occur, but productive interactions were based on mutual respect and the accountability of the relationship itself. Native hosts, we have argued, often judged validity of interpretations according to this accountability and according to the legitimacy of the interactions which produced the data. The ethical principle they endorsed was respect for the individual and his/her integrity and privacy.

The tendency to evaluate product by process was somewhat less true for those with more experience with the southern institutions generating science, and many non-native hosts (particularly civil servants) took a substantially different view. They evaluated results by a universal set of science criteria, demanding a distanced objectivity rather than a personal involvement. They often believed that fieldworkers should follow the same rules of balanced participation in the North that they followed. Too much involvement could interfere with objectivity but also with the administrative dual roles set up for civil servants. Process and product should be separated in evaluation. Although few spoke directly of ethical resolutions, and they seemingly respected the ethical "end" of host well-being, they also rejected science which was not capable of serving its own ends.

These decisions, for hosts and researcher, involve a risk/benefit analysis, as we have argued above. All ethical decisions are decisions of costs and consequences, and this was true for the Delta also. Because of the diversity of interest groups in the North, the calculation will not be the same for all, and these groups will increasingly compete for control of vital resources, including representation of culture. In addition, northern groups must in turn compete with southern institutions; both the government and

academic liaison groups like ACUNS have argued they must have primary control in setting objectives and using data. At each stage of the competition, risk/benefit analyses must be undertaken, and values used by Native northerners endorsing community-based research, for example, may conflict with those used by government representatives in Ottawa. Each has a different view about what is "natural" for the North and its cultures, and about the costs and benefits involved in each portrayal. As we will see in the concluding section, objectivity, relativity, and cultural validity are all outcomes of decisions made about the correctness of product in relation to process.

There is no clear resolution to the dilemma of the right to impose ethical priorities on other peoples; research priorities must take into account these other objectives, and there is no agreement about the setting of priorities. The ethical framework for regulation discussed below, an ethics code, attempts to sort out principles and alliances, but it can do so only partially until there is a framework for expressing and negotiating joint researcher/host participation. Can an ethical decision be made by the researcher alone, or by the community? There must be a process whereby the calculation of costs and benefits can be made public and jointly accountable decisions made.

In the 1970s a set of ethical guidelines was developed by the Man and Biosphere (MAB) program, sub-program 4 on Science and the North. A subcommittee of the Working Group for this sub-program consulted representatives of all actors as well as the ethical guidelines in existence for the various science professions in order to compose their own. In 1978 a draft of the ethical principles was produced by the subcommittee and circulated as a discussion paper in MAB Communique #6 (Canada/MAB March 1978).

ACUNS was one of the groups interested in the guidelines, "Ethical Principles for the Conduct of Science in the North." The Committee on Relations with Northern Peoples

was given responsibility for adapting the guidelines for the organization. An amended version, referred to here as the MAB/ACUNS guidelines, was adopted at the 1981 Annual Meeting (ACUNS Annual Conference Proceedings 1981: 120-22). Reaction by other actors has been positive. The SCC endorsed them in its 1977 report on northern science (SCC 1977: 75). SSHRC has used them as a basis for its ethics policy. They are also part of official DINA and GNWT policy; copies of the principles have been sent out to licensing applicants by the GNWT, for example. Applicants for the Northern Scientific Training Grants issued by DINA must agree to comply with them. Native groups have also responded favourably, although there is probably not yet much awareness of them at the community level. In 1982 ACUNS sent out a questionnaire to northern communities and groups asking for response to the guidelines. Although many places were not aware of them, others commented on the guidelines and the issues, reiterating the need for regulation of conduct (Report of the Committee on Relations with Northern People, ACUNS Annual Conference Proceedings 1985: 1-3). The Inuit Circumpolar Conference has adopted the principles, as has the Inuit Broadcasting Company, and they have been used by the Inuit of Quebec in negotiating a contract for a social research project, for example (Savoie 1983: 972; Stenbaek-Lafon 1981: 201; Lange 1987: 33).

The 18 MAB/ACUNS principles (ACUNS 1981 [in Freeman, ed.] focus on the community, although they are not codified or institutionalized at that level. The preamble states that northern people relate to research as subjects and informants, but they may also be associates, users, and planners. Thus the guidelines recognize that hosts are participants in the larger context of research design and execution. The principles, which relate to informed consent, community involvement, and reporting of results, provide a rationale for structuring relationships between researchers and local people. The first three principles define the ultimate "good" of research, for community research should take into account the "knowledge and experience" of the people"; it

should "respect the language, traditions, and standards of the community" as well as "privacy and dignity". The fourth principle states that the "person in charge of the project is accountable for all decisions on the project, including the decisions of subordinates."

As with many other professional ethics codes, the onus of responsibility is placed on the researcher. The code also respects the rights of the community to participate. The thirteenth principle has been somewhat controversial in that it gives the community (broadly defined in the ACUNS guidelines to include regions) the right to terminate research if it becomes unacceptable. The criteria for unacceptability are not defined, so that hosts may terminate the study for political as well as scientific reasons, as part of their ultimate rights to protect themselves. While this has an implicit importance in recognizing the inseparability of scientific and political criteria, there are also problems, as we will suggest below. The communities also have rights after the study is completed, including rights to comment on reports, rights to feedback and use, and rights to credit. Once the study enters the external science structure, however, community rights become less imperative, and the researcher can proceed to use the resulting information within a larger regulatory frame, and to impose interpretations. The hosts do not necessarily have rights to do their own analysis of data, or to stop dissemination of reports once the study is completed, although these could become part of individual contracts. The guidelines promote community-based regulation, and they respect rights to involvement at various stages of engagement, but they still allow the existence of external sponsorship and regulation.

There are no mechanisms to accompany the code for bridging the levels of structure in working out rights and responsibilities; it has been suggested that this might be the next step (Canada/MAB 1978: 18-19). At the 1981 Annual Meeting of ACUNS, there was talk of forming a committee to act as a monitoring and appeal board for

ethics violations, but this was never implemented because of the difficulty of setting criteria for sanction (ACUNS Annual Conference Proceedings 1981: 122). In 1985 the issue was discussed again by ACUNS, and again they decided that they would be unable to regulate all actors or even all peers. The Committee on Relations with Northern Peoples agreed with many northerners that the guidelines could best be enforced at the level where the rights are focussed, that of community participation. They did not recommend how this should take place, or by whom (ACUNS Annual Conference Proceedings 1985: 1-3).

One of the key aspects of this kind of ethics statement is responsibility, and, as noted, the focus here is on researcher responsibility. While the principles emphasize the rights of community to make sure that the researcher participates with accountability, the guidelines do not endorse the idea that the communities also be "informed" about their decisions, and that this is their responsibility as well as their right. In other words, ethics statements need to go beyond a narrow and ultimately unproductive vision of the research as active and the community as a passive victim. This has never been true of participant studies, and it should not be codified as such. One of the guidelines omitted by ACUNS from the original MAB version concerned responsibility:

Investigators, communities, and research participants should fulfill all agreements associated with consent to undertake the research (Canada/MAB 1978: 13).

While this guideline was vague, it did suggest the use of one particular mechanism, the agreement, to approach the task of resolution.

Ethical responsibilities must really be faced on all levels, along with the recognition that science is inseparable from political context. The meta-structure of regulation should take account of this, providing for adjustment of funding, time frames, and other factors to require provisions like informed consent and feedback. In essence,

these procedures should be taken from the realm of ethical disputes and instated as effective science practice. Similarly, the role of the community must be recognized, and procedures defined which will ensure that communities are able to receive and use information effectively. These might include community institutions to process consent and feedback communications, and to make sure that sufficient information and comprehension is present to make participation meaningful and science applicable to local concerns. They might also include an endorsement of a process of negotiated agreement rather than a narrow view of consent.

Principles like these cannot be universally applied any more than science methods can, but they can be adapted by the actors in these kinds of active processes. Again, the political contributions to "truth" need to be comprehended and acknowledged by all parties before this adaptation takes place and diverse values are discussed and applied. Communication and negotiation are at the heart of the participatory model of social research and accountability. They provide means whereby science rules and northern rules of validity can be expressed in the bounding of knowledge and in the quality of interpersonal relationships. The development and acceptance of an ethics code requires structural support from the institutions of science, but it is also a commitment to negotiate the emergence of science in a manner true to the monistic principles of P-O: the emergence of text from context, and vice versa, rather than the rationalization of text through context.

In the last section, we will return to the framework of participation and the idea of the dual stance. Social research is molded by the layered context of response, accountability, regulation, negotiation and legitimacy, and we can trace the evolution of models of participation by looking at the way the North has responded to visitors from the south, including social scientists.

B. FRAMEWORKS for KNOWING

At the beginning of the last chapter on regulation we suggested that regulation can involve decisions about process and legitimacy. Ethics and regulation of participation also subsume the general issue of researcher dualism, or the expectation by both science and hosts that the individual investigator must bridge inside knowledge with an "outside" objective perspective. A commitment to research as an active process means a sharing of responsibilities for the process which produces data and interpretation.

In the first couple of chapters we explored the rules of participant-observation and their consequences. Two traditions of knowledge were identified, one which posited a dualistic separation between the knowing being and the known reality, and another which proposed a continuity between the two insofar as the knowing being constructed and imposed a vision of reality on the world at large. An empiricist, dualist tradition was widespread in the formative years of anthropological theory. The "exotic primitivism" extolled by the first observers of men was accepted as natural and accessible to the open-minded investigator. By the time of the Enlightenment, the budding science of observation was more reflexive, as explanations for similarities and differences in human conditions were sought in human nature itself. By the 19th century, with the evolution of the professional discipline of anthropology, scientists had devised ways to systematically compare others with themselves, to classify and order. But there was little attempt to question the empirical existence of this order even when it was identified within themselves.

By the 20th century florescence of anthropology, the concept of objective knowledge was codified into the methods of participant-observation along with reflexivity. Objectivity could be seen as either immersion in the field setting or as the reproduction of this experience in text. Both of these processes were confused with validation, and the observer essentially became the instrument of proving validity.

Because of this, the participant observer's own rational recreation of context, epistemologically, became confused with ontology, and his marginal dual position was resolved through an incomplete separation, within reason, between epistemology and ontology.

The monistic challenges to this positivism had been present in the social and human sciences since the 19th century, but they became increasingly prevalent by mid-20th century partly as a result of the expansion of fieldwork as a vital part of anthropology. The P-O method, which was originally predicated upon a dualist tradition of knowing, enforced reflection and an attention to the observer's role. Host response also encouraged researchers to challenge their own assumptions about knowledge, and as a result many versions of P-O have become more monistic. An extensive literature on monism and P-O has appeared which suggests that validity rests in the intersubjective process of knowledge creation. Dual roles and marginality became a central feature of this intersubjectivity. Monists have rejected the idea of an objective separation of Self from the Other; objectivity emerges from a dialogue between actors. Yet, monistic theories have not been completely successful in defining the relationship of epistemology with ontology, either. They have not in fact effectively defined ontology at all, placing it, epistemologically, on the essentialist plane of the interactive consciousnesses of participants. That is, ontology becomes a feature of the rational perception of discourse on behalf of each actor.

As we suggested in Chapter 2, we cannot resolve these philosophical questions here. But we can suggest that the confusion of objectivity with validity is part of the problem. Similar problems arise with the idea of relativity, which has been placed in opposition to objectivity. Relativity as a concept has been used in many ways, but ontologically it is taken to mean the absence of truth outside the known individual or culture. We looked at the importance of relativity for the growth of anthropological

theory in the 20th century, as cross-cultural generalizations gave way to the need for functional data bases and fieldwork was used to provide these. Rather than suggesting that the variety of accounts of world cultures was the result of the differing accountability of their interpreters, it was suggested by positivist theorists that each culture had a unique and positive existence. The internal relativity among aspects of each society (by which each element is justified by the others), exalted by the structural-functionalists who replaced the 19th century evolutionists, was generalized to relations between cultures, or external relativity. This extension of boundaries, from internal relativity within culture to external relativity between cultures, has placed, then, a burden on the anthropologist to demonstrate the nature of the relationships among world societies, between text and context, between one context and another.

Monistic theorists have elaborated the internal relativity of culture, often forcing the discipline to lay aside universalistic criteria of moral dictates. They have helped us to see the moral dimension of truth as they have focussed on the relationships between hosts and investigator which produce truth. The epistemological danger was that truth became extremely relative, imperative in its narrowest sense only to the parties to each intersubjective discussion. Only the observer, it could be argued, could make connections among relativized cultures by his/her own objective, marginal rationality. Again, the intuitive rationality of the observer is seen as the source of ontology at the same time as it literally contradicts, or overcomes, the strict dictates of externalized relativity.

The concept of culture grew in proportion to the ideas of objectivity and relativity. We saw how it developed as societies were observed and compared, and questions were asked about whether culture was universalist or particularist, about the relationship of the individual to culture, about the relationship of culture to other levels of reality, about the form of culture, and about the modes of its conveyance. The early

empirical theories which accepted an objective context of reality embodied that objectivity into a positive view of culture as a unique bounded entity external to the observer. Boas placed culture in the text and its interpretation, and Malinowski embedded it in the context of behaviour, but both viewed it as unique and discoverable through field work. Thus the scientist could use his/her marginal view to see the total reality of culture, and the insider, looking up from within, could only see a part. The outsider's version of culture must be validated by the insider, however, who "knows" culture from living within it. The researcher's epistemology must prove, through a modified inductive, or circular, field method, the existence of a valid ontology. Results must be tested deductively for validity on the inside.

There were problems with assessing this validity, however. Were the accounts to be believable, internally consistent, or "true"? What if there are competing versions of culture within the universe defined by the researcher? Given these problems with the validity of culture, then the epistemological discoveries of the fieldworker can easily be taken for the ontological validity of the pre-existing field culture, especially if he is given the task of defining what validity is. And, if culture is taken for granted as an empirical living object, then there may be little reason for testing the "truth" of its recreation. In Chapter Two we discussed more recent definitions of culture which have sought to overcome them. These included varieties of rule theory, Geertz's symbolic processual theory, alternate hermeneutic formulations, praxis theory, and relationism.

These kinds of theories shifted the definition of culture in the 1970s away from positivism and toward a view of culture as sets of rules and metaphors, situationally and intersubjectively defined. Text was no longer clearly separated from context, and there was a danger of circularity. Hermeneutic theories risked, for example, an overemphasis on the circular derivation of interpretation, with an assumption of a common context of reference as the source of textual understanding; but the existence of this context could

not be given credence outside the dialogue of field information. Culture thus lost its ontological status altogether and became epistemology: culture could be known by doing, not viewing. Praxis and relational theorists have recently taken a more practical look at P-O, trying to reintroduce a modicum of ontological existence. The fieldworker and his teacher, they suggest, must take the existence of at least two cultural traditions (their own) for granted in order to reconcile them through dialogue, or to interpret each through the other.

Participant-observation has therefore been developed around two traditions of knowledge, each fundamentally different from the other, but both placing the onus of locating "truth" with the fieldworker. The investigator must prove that his/her "objective" truth is valid, is relative to its source, and is bounded and unique. Because of the burden placed on the rational persuasion of the fieldworker in these procedures, there has been a confusion of validity with objectivity, of role with objectivity, of relativity with culture. We have also seen in looking at ethics and participation how the fieldworker has often been given this burden by science regulators. In Chapter 2 we proposed an alternate framework which might help sort these processes and also recognize the hosts as sharing this responsibility. The framework borrows from monistic approaches, but makes some modifications.

Objectivity should be seen as the bounding of knowledge, not as a calculation of validity. This can reduce the need for seeking ontological validity as part of the rational structures imposed by the fieldworker. The playing of participatory roles in the field can be seen as a correlate of objective knowledge. Roles are bounded expectations of behaviour which can be flexible as boundaries are re-defined by mutual consent. Relativity, the correlate of images, is the extension of boundaries. This conceptualization of relativity requires a recourse to the original view of relativity as internal and to the concept of relationism in the sociology of knowledge. The use of

relativity is the ability to see the relationship among bounded forms of knowledge. It properly resides in the perspectives of individual actors, but it is pure epistemology, a way of knowing, rather than a pseudo-ontology. The image of the fieldworker, especially the anthropologist, is a reflection of the process of relativity. The social scientist may embody ideological relativity, or the perceived relationship between two bounded frames of knowledge. Images are formed of intersubjective, reciprocal responses, and they show us how each party seeks to account for the relationship between what they know and what the Other knows.

Culture, similarly, was redefined to have two primary functions: validating and contextualizing. It is not the source of validity, but it is the process of validation. This occurs as visitor and host bound the text from context, creating a new context of mutual understanding and then validating it as legitimate. Context may be created, and it may not necessarily be assumed to be pre-existent, but it is nonetheless considered by the actors to be real. It is the substance of rules, of habitus, of practical solutions to problems, of metaphoric extensions of form. Culture is content for the form of objectivity. Validation is itself negotiated in the field and in the office as legitimacy and as accountability. As the latter, it need not be true for all members, but it must be responsive to context.

Let us return now to the case study. We will apply the framework to the evolution of participation in the Delta. From the measured involvement of the early explorers, to the selected intrusive reforms of the missionary and trader, to the large-scale mediation of southerners into the northern social, economic, and political structures, rules guiding the participation of the visitor have been insinuated into the development of new cultural forms. The explorers literally drew lines across the earth to denote social distance, and they had no doubt of their cultural distance from Inuit and Indians. To these early 19th century visitors, the culture of the Native person was both

collective and uniform, on the one hand, and located in the natural context of personality, on the other. Those who had extended stays in the North were forced to see the daily realities of Native living, but this was interpreted through attitudes about culture brought from the south. The explorers, fur traders, and missionaries all believed culture to be shaped by natural forces and to be immutable without changes in evolutionary design. Yet, as missionaries and traders intervened in local traditions through participation they encouraged individual Indians and Inuit to rationally segregate themselves from the collective consciousness, to rise above nature -- yet not to break the cohesiveness of socio-economic adaptation.

Thus the earliest visitors accepted an objective formulation of culture which could be viewed relative to their own. Even the earliest scholars of the people, like Petitot and Stefansson, were secure in their objective, learned stance. Both Stefansson (1913) and Jenness (1928), for example (like other scientists of their time) portrayed culture as a self-conscious blend of evolutionism and relativism. The fundamentals of this position remained intact as new transient visitors, the government workers, visited the North. These visitors stayed for much shorter periods than had the missionaries and traders but they accepted the same dual mandate: to reform and guide the Native people into cultural enlightenment while maintaining a distanced involvement which would allow the Native person to achieve independence. These visitors tended to insulate themselves into a frontier element of northern life.

As the number of transients grew, so did their intrusive involvement in the northern political economy, but marginality and frontierism remained themes for a developing subculture and self-image. These visitors increasingly separated notions of culture from individual performance. Culture remained a collective representation which was marketable as image and as ideological symbol, and which could be adapted to reinforce their own cultural norms. By the 1960s and 1970s, there was a prevalent

conceptualization that "real" Native culture, the collective soul, was a product of the bush, and it was featured by a child-like innocence, stoic temperament, and an enduring attitude toward environmental harshness. But it was also felt that many Native people had left this behind, living a marginal life in the towns, waiting to be acculturated into Euro-Canadian society. The rational aspect of character, the individualistic striving, could be tapped for conversion. While the teachers, administrators, and health care workers assisted in this, working themselves within their paradoxical dual role, they began to borrow elements from the marginal frontierism of the urban Native and to describe themselves as marginal, individualistic, eccentric, defiant, informal, and enduring: characteristics of the "real" bush life translated into town life and the interaction with southern-based institutions and expectations.

At the same time the number of transient social scientists began to grow; these new visitors combined elements of the solitary immersion of the first explorer scientists with the limits of summer schedules and emerging short term "visitor" roles. Many of them made use of the emerging P-O tradition in social science to follow a set of rules in gathering data, expanding methods from the use of a few key informants to the employment of a variety of techniques, including survey instruments and interviews. Conformity to pre-existing non-Native roles, such as that of the teacher, gave way to a "researcher" role which included both flexibility and a dangerous ambiguity.

A model of summer P-O came to represent much social science as investigators modified P-O rules to meet the limits of the northern "growing season" and academic schedules. Active participation in facets of Native culture, such as hunting, was encouraged as epistemology, and there was great self-awareness of role expectations and the dual stance. Native people criticized their role-bound distance and failure to endure local reciprocity, praising those who did stay for extended periods and who participated in bush life. Non-Native civil servants, conversely, encouraged these

researcher/visitors to be aware of role, to distance their involvement carefully while abiding by the overt rules of reciprocity and hospitality. Excessive "participation," such as drinking, was discouraged by other non-Native people, including other researchers, as undermining the distanced objectivity demanded by role.

Many researchers began to be aware of the contradictions of objectivity and the natural context, and northern hosts (Native and non-Native) came to expect a dual stance. They criticized method: talking to the wrong people, not becoming accountable, not showing personal responsibility, not engaging in reciprocity, and not being able to evaluate the truth of what is told, all affect the production of text and the "objective" truth of that text. Yet hosts also expected researchers to have an expertise outside the natural context. This included a superior ability to recognize truth and knowledge, but also an ability to use information which the average person would not have. The scientist was expected to be able to continue the process beyond the field and to use information in productive, correct, and beneficial ways. By the late 1960s the social scientist had become the epitome of the dual, marginal individual: expected to bring the power and expertise of the southern institutions and learning to bear on northern issues, and expected to do it correctly according to local norms.

Cultural studies became diversified. Anthropologists in early and mid-century, reflecting the new relativism and structural functionalism of the discipline, tried to give a balanced, relative view of culture, separating it from social and economic dimensions as a functioning domain. In the 1960s, there was a heavy emphasis on acculturation, with many studies bearing on the problems arising from marginality and insufficient assimilative adjustment. Many investigators criticized the role of government, showing how economic deprivation had caused the Native people to adapt through marginality. Others looked at internal factors of change (see for example Bissett 1967). Most of the MDRP studies, for example, described ethnic and class

relations, and while recognizing the role of government policy in determining this, did not fully explicate it (except for Smith (1971) and Wolforth (1970)). Thus the image of the "primitive" promulgated by the early visitors had been transformed into the image of the deviant; indeed, many social scientists of this period in the North noted that non-Native visitors were "adapting" the praxis of deviance to characterize their own northern subculture. There was an implicit assumption that culture could be gained or lost in its empirical objective form.

By the late 1970s there were yet further changes. For the non-Native visitors working in government and commerce, many of whom had made the North their permanent residence and others of whom were still transient, the view of culture shifted once again. By the time of fieldwork in 1981 people felt that the gap between the old collective tradition of culture and the individual bearer had grown too wide, and there were many uncertainties about what culture was. The "real" Native person was gone (p.c. Feb. 11, 1981; Nov. 9, 1981; Sept. 25, 1981; Oct. 21, 1981; March 3, 1981; Feb. 27, 1981; Nov. 5, 1981; see also O'Malley 1976: 69; Dyson 1979: 228). People felt that culture should be taught in the schools, particularly language, but could not agree on what "cultural inclusion" was (p.c. Feb. 5, 1981; Nov. 6, 1981). Culture was best glimpsed through individual expression and intersubjective dialogue, and, given the diversity of social and cultural groups in the Delta, it had lost much of its prior status as "natural" northern context.

Several events and processes were affecting this; they could be seen throughout the North as well as in the Delta. Native people were demanding a demonstration of commitment from the large numbers of transients in order to participate in government; for example, the Dene Nation came up with a proposal for self-government at this time which required a 10 year residency for voting. The prior physical segregation of towns like Inuvik had diminished as Native and non-Native people came to

live as neighbours. Various political groups were struggling for rights to govern and to control resources; and this process included claims to culture through history and residence. Each group made assertions about the nature of culture and its relation to natural rights, and each protested the intervention of others in this. Resource potential and public hearings like the Berger Inquiry spurred this as Native people laid claim to renewable resources and the importance of these resources in maintaining cultural viability. And Native leadership were given responsibility for culture. They were held accountable by their people; it is they who convey the northern context to outsiders, who represent the collective, and, in fact, often represent the split between the collective and individual dimensions of culture just as the P-O fieldworker represented the derivation of text from context. The public came to interpret the loss or revitalization of culture through their acts, and the diversity of political perspectives came to give an impression of cultural disintegration.

In social science research in the Delta, the idea of culture increasingly came to be lost also, and subsumed under political and economic rubrics as studies of pipelines and impact assessments featured semi-quantification and often an ignorance of cultural dynamics. Participation in science veered away from the structured yet informal dictates of summer P-O and toward a more limited and focussed view of participation. Independent science became rare in the Delta, as researchers have employed short-term surveys and hearings to obtain a rapid assessment of local perspectives. Both consultancy and hearings as methods have imposed a view of participation in which northerners are expected to participate in southern exercises. The participation is very much structured from outside: investigators come with a limited, focussed set of questions, seek out a few "representative" individuals (such as Native leadership), and frame the discussion by use of select short-term methods.

Therefore the northern people have come to be expected to participate in southern science, rather than the reverse; at the same time, they have increased their own pressures on the social science establishment for intersubjective accountability. The P-O fieldworker, particularly the anthropologist, has come to represent the dualism of northern society, the marginality out of which culture must be redefined. He is expected to have the knowledge of how to put a text together in such a way that it is representative and accountable. As long as the anthropologist has set the rules for engaging the social context, for the manipulations of reciprocity, and for the learning of language, he may not be fully accountable to Native versions. The more he claims representativity, the more he fails, and the more he has become a symbol of the failure of the outsider to understand. And with the recent narrowing of the parameters of participation, this problem becomes acute.

By understanding how the conventional model of P-O has operated within the norms of western science and polity, we can interpret this history of participation, the development of duality/marginality, and the definition of culture. We have seen how the portrayal of Native culture by outsiders (including researchers) forced a gradual separation between the individual and the collective dimensions of culture. At the same time, the researcher became the ultimate marginal individual and symbol of the meeting of cultures. If we view relativity and objectivity conventionally, we can see that the task of defining the ontology of culture and of validating it has fallen upon the researcher; this is true of anthropological P-O and it is true to some extent of Delta experience, except that other categories of visitor have contributed to this as well. The "objective" fieldworker, though, more than anyone else, is expected to be able to validate what he sees through accountability to the rules of learning. And, he becomes symbolic of the ideological relativity between cultures. The breakdown of culture as a unified concept has occurred as this ideological relativity has become enhanced, and the need for

it has grown in a setting of political competition for legitimacy. The individual leader, the visitor, and the researcher become marginalized from their culture, and must demonstrate their commitment to a renewed collective vision.

This view of research has been predicated upon a model by which the visitor "does research on" the hosts; the researcher is the active, invading force, and the hosts are the victims. The conventional model allows the creative process to operate through the rationality of the observer, as we have argued, and this rationality is informed by Self/Other dichotomies and the obedience of rules. Because of the rigidity of the model, it does not allow us to answer the central question: how can the investigator be simultaneously responsible for the creation of cultural portrayals and the destruction of culture? The "rational" fieldworker/ visitor who follows the expectations of objectivity and relativity must be reliable. The conventional model, even as it has recognized the vitality of intersubjective dialogue in fieldwork, has never adequately recognized the importance of host participation in bounding and validating the limits of rationality.

If we use the alternative framework and see the creation of text as truly an interactive process which extends beyond the field to the continuing dialogue of introspection and of structural regulation, then we can begin to recognize that the dichotomies are artificially drawn, the boundaries of culture are arbitrary -- and that many of the problems of anthropological role and images derive from a failure to account for host participation and from a focus on social science as the means by which bounding occurs. In fact, objectivity and relativity are processes which encompass all cultural participants, and which inform the means of knowing for both researcher and host. Indeed, many of the northern protests about anthropology are critiques of the conventional forms of social science rules and their consequences.

If objectivity is the bounding of knowledge, then we can see that one anthropological task is to evaluate how the bounding takes place. Relativity is part of the

dialogue: the researcher uses familiar relational metaphors to incorporate new "versions" of culture and social form into the field of his own understanding, and the teacher-host does the same. Role in the field is the bounding of expectations to a form which is eventually mutually acceptable; the bounding and ambiguity of the researcher role, the attribution of flexibility to administrative or teaching roles, and the preference for apparent role-less-ness can be seen as Native host responses to experience and as ways to enhance accountability in future encounters. There must be both knowledge and expectation for social life to function, and these are objectively bounded into sets of appropriate behaviours. But the individual is given responsibility for this bounding, for this accountability, through his personal involvement. Native people have asked that the anthropologist/researcher role not be adhered to rigidly; when this happens, the outsider becomes marginal, straddling the roles he plays rather than adapting them to each other in the daily context of intersubjective community life.

Relativity is the source of imaging, including the image of the anthropologist who comes to represent the relation among forms of knowledge, the ideological relativity. In field interactions, images are formed of intersubjective, reciprocal responses, and they show us how each party seeks to account for the relationships between what they know, and what the Other knows. The foolish, naive, irrelevant, deceptive investigator is only that juncture of paths of knowing. The alternate framework helps us to understand that anthropologists did not become symbolic of ideological relativity by validating culture through objectivity, but by drawing attention to the processes by which validation occurs. Roles and images are not independent functions of the imposition of science, but in fact have been manipulated by both hosts and researchers to resolve or increase the dual stance and its consequences in culture.

Culture, then, is the imputation of content to the form of image and role. It validates and contextualizes. For many Native hosts, validation occurs in commitment to

context, not as objective separation from it, and they perceived a failure of many outsiders to demonstrate that commitment. They have shown us that the bounding of culture is indeed arbitrary, but they have resented attempts by outsiders to give this bounding ontological status by imputing their own terms of validity.

Therefore the alternate framework helps us see that objective, relative knowledge is mutually accountable, and the anthropologist has become symbolic, to many Native hosts in the Delta and elsewhere, of a failure of accountability in a conventional research paradigm. Northern people have accentuated the dual stance taken by many social scientists by manipulating role and by highlighting the incompatibility between their demands for accountability and the researcher's retreats from it. By viewing hosts as active participants, we can see how the anthropologist has been viewed as having simultaneously created and destroyed the text and image of culture. He created it by bounding, imaging, and validating it, and he destroyed it by separating himself from the process of culture just as many visitors have expected Native individuals to distance themselves from their culture. When southerners went North and projected images of culture, they were assessing the people and attributing an external validity to their way of life. This external validity came to affect the way northern people saw themselves. Hosts have become aware of the process, and they are aware that their role in shaping the interaction which produces knowledge has been undervalued and dichotomized. The more the researcher drew away from commitment to insider rules of legitimacy, the more hosts lost control, and the context for participation has deteriorated. And as the context for participation deteriorates, as the rules become manipulated and changed and procedures confused, so the integrity of the text is lost.

There are several consequences to re-defining the paradigm of field accountability, and it is with these that we conclude. (1) Host societies like those of the Delta communities should be recognized as active participants in the venture of social

science. This participation should not be artificially limited by the use of conventions such as hearings, but extended to the regulation and rules of science. Anthropologists once assumed that they were the purveyors of the rules of P-O and the resultant reliability of knowledge, but they are increasingly coming to see that hosts have been and should be more vital to the bounding and validating of knowledge than has been recognized publicly in the science establishment.

(2) Host involvement extends beyond the field to the use of science. Conventional research models and indeed regulatory models like ethics codes have assumed a separation between control of field interactions and the interpretation and circulation of knowledge outside the field. Host societies are demanding an involvement in regulation which will recognize this continuity; they do not want to be the passive recipients of feedback but instead they want an active role in interpretation, dissemination and application.

(3) Regulatory models, whether codes of ethics or meta-structures which control access to research, must respect the ways in which the roles of the various actors are interrelated, the different levels of regulation (from community to organization to federal government) and their relationships, the comprehensiveness of host involvement, and the continuity of the research process beyond the field. Institutions for regulation can provide means to redefine participation as a mutual process such as funding for consultation and feedback as central parts of the research process (rather than as peripheral to it). The moral dimensions of truth should be recognized, and the idea of "control" should be seen to include a comprehensive involvement of all actors in all phases of research.

(4) Finally, northern peoples, Native and non-Native, are attempting to redefine the paradigms and images of their own cultural reality -- to themselves and to outsiders. Culture need not be seen only as unique and finite, but as capable of re-

forming and re-validating by relating old forms of understanding to new ones. By extending the ethical dimensions of accountability to all phases of the research process, and by realizing the inseparability of truth from control of process, social scientists can play a constructive role in assisting this rebuilding. They have already made contributions to this through the application of their work in public forums such as land claims and impact assessment. The integrity of participation must be redefined and realized to include all actors as ethical beings controlling their own legitimacy. Each actor must be honest about their own assumptions so that a negotiated dialogue can take place. The image of the anthropologist may ascend from the dissembled bricoleur, knowing all and nothing, to the assembler of learning.

NOTES

Chapter One

¹ Many social scientists have provided accounts of the strategies and outcomes of their participant-observation field experiences. Descriptions of the practice of the rule, "B" may be found, for example, in Saberwal 1975: 45-49; Rabinow 1977: 73-77; Wagley 1960: 398-405; Berreman 1962: 6-9, 1968: 347-49; Weidman 1970; N. Diamond 1970; R. Wax 1971; Briggs 1970; Agar 1980b: 27-30; Liebow 1967" 237-48; Henry 1969: 42-3; Read 1965:12-13; Chagnon 1974; Dumont 1978: 20-31; Whyte 1981; Beattie 1972: 9-14.

² Discussions of the practice of rapport-building appear also in many, if not most, field accounts. See for example Srinivas, et al. 1979; Bellwinkel 1979; Berreman 1968: 355-60; Lee 1970; Gonzalez 1977; VanStone 1970; Hart 1970; Keiser 1970; Carter 1972; 136-39; Shaffir et al. 1980: 185-89; Braroe 1975:16-24; and Middleton 1970: 13-23 which emphasize this process.

³ Many researchers have vividly described the processes by which they learned and tested the rules of their host culture. See for example Nunez 1972: 164-71; Marshall 1970; Denzin 1972: 205-16; Gonzalez 1977; Dentan 1970: 101-09; Kimball 1972: 182-91; van Binsbergen 1979: 205-09; Dobbert 1982; R. Wax 1971: 189-248.

⁴ Field accounts which emphasize reciprocity as strategy and manipulation include Uchendu 1970; Johnson 1975; Beattie 1965; Lowie 1959; Dentan 1970; Geddes 1967: x-xvii; Mayer 1975: 20-40; Briggs 1970; Freilich 1977b,c; Pandey 1972; Schatzman and Strauss 1973).

⁵ Several social scientists have tried to provide schemata which link roles with degrees of observation and participation (Gold 1969: 30-8; Denzin 1972: 189-94; Sykes 1978; Cassell 1980). The role of an observer may itself be legitimate in some settings, whereas others require more participation in tasks and activities in order to be effective in accessing information.

⁶ There is an extensive sociological and anthropological literature on role-playing in the field. For example, strategies of roles are broadly discussed in Emerson 1983: 177-89; Agar 1980b: 54-62; Dumont 1978: 50-66; Georges and Jones 1980: 64-106; Nadel 1939: 325-26; Reinharz 1979; R. Wax 1971: 52-55; McCall and Simmons 1969: 1-5, 28-9; Vidich 1969: 78-87; and Mead 1972a, 1977: 47-9, 67-78 as well in most separate field accounts. Because the concept of role is so integral to P-O, it is impossible to detail here all the sources in which it is discussed. Refer also to discussions of P-O concepts and of ethics in chapters Two and Eight.

7 For a discussion of the similarities/differences between the role of the researcher and that of the missionary, see for example Stipe 1980; Salamone 1977; Miller 1981; Cochrane 1971; Clifford 1980).

8 Strategies of problem selection and bounding are discussed, for example, in Georges and Jones 1980: 23-42; Agar 1980b; Dobbert 1982; Beals 1970: 36-8; Langness 1970; N. Diamond 1970: 115-20; R. Wax 1971: 249-67.

9 This rule can be extrapolated from literature on roles, on observer liminality, and on techniques of ascertaining "truth" and validity. (See for example Agar 1980b: 42-62; Schatzman and Strauss 1973; Becker and Geer 1969: 322-31; Becker 1970: 189-200; McCall 1969: 128-41.) Multiple studies of one community are one way of highlighting the variables of interpretation and the burden placed on the credibility of the fieldworker in evaluating "truth" of his/her observations. See for example Derek Freeman's restudy of Margaret Mead's Samoa (Freeman 1983) for a recent example of this.

10 Several sociologists/anthropologists have described the advantages and disadvantages of the insider trained as analyst, noting that although the insider may have greater access to cultural information, he may suffer from the same demands of marginality as an outsider would. See for example Vidich 1969; Stephenson and Greer 1981; Brislin and Holwill 1979; Gwaltney 1980, 1981; Whitehead 1980; Jones 1970; Lewis 1973a; Hayano 1979: 99-103; and Chilungu 1976.

11 See for example Briggs 1970; Braroe 1975: 14-24; Kloos 1969: 509-11; Keiser 1970; Middleton 1970: 70-2; and Powdermaker 1966 for discussions of this marginality/ambiguity.

12 The number of accounts which stress this element has greatly increased since the 1960s, although they were present at that time. See for example Agar 1980b: 4-6; Georges and Jones 1980; Dumont 1978; Keiser 1970: 223-36; Powdermaker 1966: 9-11; Bowen 1964: 290-95; Chagnon 1974: 102-3; Weidman 1970; Geddes 1957: xx-xxix; Cesara 1982; Reinharz 1979; Johnson 1978; Fenton 1972: 175; and Berreman 1968: 368-70.

13 Some of the professionals who have remarked on this point include Rosemary Firth 1972: 10-11; Levi-Strauss 1974: 40-42; Briggs 1973: 24-27; Lowie 1959: 4; Hill 1974: 408; Richardson 1975: 520-25; Salamone 1979: 47; Spronk 1973: 1-2; Pelto 1970: xi-xii; Codere 1970; Nader 1970; Gulick 1977: 89-91; Worsley 1970; Hayano 1979: 99.

14 Many fieldworkers going to non-western societies have encountered the suspicion that they may be some kind of spy; see for example Golde 1970; Cesara 1982: 119-20; Carter 1972: 139-41; Beals 1970: 40-1; Landes 1970; R. Wax 1971: 244; Berreman 1962: 3-8; Beattie 1972: 15-6; Middleton 1970: 13-5; Geddes 1957: x-xii; Read 1965: 17-18; Mayer 1975: 20-21. It has been reported also from fieldwork in western sub-cultures; see for example Keiser 1970: 227; Whyte 1981; Liebow 1967: 237-56.

¹⁵ For accounts of reciprocity and manipulation by hosts, see note 4 above; also see L. Bohannon 1960: 379-95; C. Kluckhohn 1960: 445-57; R. Wax 1971; Buechler 1969; Dumont 1978: 70-90; Liebow 1967: 237-56; Gulick 1977; Beals 1970: 41-6; F. Cohen 1976; and Gonzalez 1977 for more explicit accounts of host demands and interventions.

¹⁶ See for example Vine Deloria, Jr. (1969) for a critique by an American Indian scholars about anthropological work among Indians; Deloria startled the discipline with this stark commentary, and since its publication similar criticisms have been made by Native hosts elsewhere (see for example Strynadka 1970; Chilungu 1976; Jones 1970; Ortiz 1972; Medicine 1972). Brislin and Holwill (1979) showed samples of anthropological writings pertaining to their home cultures to 105 Asian and Pacific residents at the East-West Centre in Hawaii; they received comments frequently referring to inadequate understanding and bias. Morauta (1976) and Powell (1974) describe efforts of anthropologists and students in Papua New Guinea to do their own fieldwork and to overcome what they perceive to be the failings of non-indigenous fieldworkers in understanding their society.

¹⁷ Some of these image associations are described in accounts by Rabinow 1977; 78-9,91; Middleton 1970:12-22, 70-1; Beattie 1965: 16-7; Liebow 1967: 251-56; C. Kluckhohn 1960: 451; Carter 1972: 139; N. Diamond 1970: 129-32; Newman 1965: 13-4; L. Bohannon 1960: 389; Dumont 1978: 20-31, 45-51; Beals 1970: 38; Dentan 1970: 103-05.

Chapter Two

¹⁸ Pragmatism refers to a branch of philosophy which looks for the meaning of ideas in the behaviours which they stimulate, and the "truthful" coherence or productivity of those behaviours. Pragmatism also stresses the functional and practical necessity for cultural rules in meeting fundamental human and social needs, and it allows the individual an active role in the construction of meaning. William James and John Dewey were among the leading proponents of pragmatism. James was influenced by the ideas of Kant; he in turn influenced both Dewey and George Herbert Mead. The latter individuals were among the nuclei of the 'Chicago school' of pragmatism early in the century. Both Boas and Malinowski were influenced by varieties of pragmatism in their culture theories (Leaf 1979: 132-3, 180-8; Strauss 1964: viii-xvi).

¹⁹ Descriptions of the influence of positivism in P-O can be found in Giddens 1976: 130-38; Rabinow and Sullivan 1979: 8-13; Popper 1979: 81-105; Hobart 1982: 39-47; Jarvie 1972; Radnitsky 1970; Johnson 1975.

²⁰ George Herbert Mead is one of the founders of the symbolic interactionist approach. Mead, who absorbed the fundamentals of pragmatism from both James and Dewey, was a foremost social philosopher at Chicago in the 1920s. His interactionist approach began with the individual who must constructively solve the problems of adapting to his environment. Through this process - Mind - whereby the person and his environment interact, the individual creates concepts of Self and of Society; the former is self-consciousness of one's own attitudes, and the latter is the internalization of the dictates of society's institutions. In contrast to the structuralism of many sociologists of

the time, Mead focussed on the emergent qualities of social relations (Strauss 1964: xii-xv; Leaf 1979: 202-4; Morris, ed., 1934).

21 The ethnomethodological approach is most associated with the work of Harold Garfinkel, who developed many concepts used by the phenomenologist Alfred Schutz. Ethnomethodologists use an interactionist approach to ordinary experience and its composition in action and attitude. Garfinkel focussed on the rational attitudes of ordinary actors in constructing meaning and rules from daily events; rationality is in effect the way the actors account for meaning in striving for practical solutions to daily problems. There is however some problem within ethnomethodology in "accounting" for reality outside actor's interpretations of it (Giddens 1976: 33-43; Garfinkel 1974; Mitchell 1975: 130-53).

22 Objectivity as an empirical method has been described extensively in social science literature about P-O. See for example Riley 1974: 1-8; Tennekes 1971: 156-219; Cunningham 1973; Barnes 1974: 22-44; Gouldner 1974: 53-63; Goff 1976: 396-409; Goldstein 1968; Bauman 1973: 161-71.

23 Kuhn (1970,1977) describes "scientific revolutions" in which one paradigm of scientific belief replaces another. Each paradigm consists of a body of assumptions and theories taken by scientists in the field as true or as verifiable. Each is replaced as its utility is weakened and some of its hypotheses falsified; a new paradigm may quickly gain credibility to replace the old. This is the illusion of the revolution.

24 Popper's (1979) ideas on the philosophy of knowledge are complex. Objective or critical knowledge is the level of theory and conjecture about reality. This contrasts with knowledge of the empirical world and the subjective knowledge of the knowing subject. All knowledge, according to Popper, is conjectural; objective theories about the world can only be falsified, never proven. As self-conscious knowledge, however, objective knowledge is often the logical, rational knowledge of critical assertion and belief.

25 Polanyi (1966: 16-25) argues that some of the "objective knowledge" that Popper identifies exists in a tacit dimension. We are not always aware of the assumptions which inform us. They may be derived from science but operate implicitly as personal belief, praxis, and logic. This tacit knowledge can be particularly relevant to the 'intuitive' processes of P-O.

26 Alfred Kroeber (1976 [1917]; Voget 1975) is widely noted for his theory of culture as superorganic. He suggests that socio-cultural reality exists in layers; the organic, lower levels of social existence could set limits for but not determine higher levels, such as the superorganic level of culture. The superorganic operates historically, autonomously of individual will.

Leslie White (1968, 1976 [1959]) argued that culture is integrated at an abstract level, as a class of things or events dependent upon symboling. It can be studied as sets of discrete phenomena, using the methods of science, just as people "test" their subjective interpretations of cultural against their experiences with the external world.

Edward Sapir also produced stimulating and complex ideas about the nature of culture (1949, 1956). He distinguished spurious culture, the popular perception of how culture should appear, from genuine culture which has sincere meaning for the individual. Although Sapir believed in the separate reality of culture, as observed, he also believed that it was, in phenomenological fashion, subject to individual will and personality.

27 These questions are broached in most discussions of culture from early in the present century until recently. See for example Bidney 1976; Rice 1980; Vermeersch 1971; Kroeber and Kluckhohn 1980; Keesing 1974; Bauman 1973; and Kaplan 1976.

28 These generalizations were in part derived from discussions of the role of the culture concept in P-O in Agar 1980b; Dobbert 1982; Freilich 1977a, d; Stipe 1980; Honigmann 1976b; Kaplan and Manners 1972; Berreman 1968; Richardson 1975; deJong 1967; Weiss 1973.

29 The grounded theory method combines the qualitative processes of discovery with more quantitative methods for the development and testing of theory (Glaser and Strauss 1967; Charmaz 1983: 125). This method was influenced by the symbolic interactionism at the University of Chicago, which also used cyclical methods of deriving and testing rules of cultures. For further analysis of this kind of hypothesis testing in sociological P-O, see for example Becker 1969, 1970; Cicourel 1964; Emerson 1981, 1983; Katz 1983; Geer 1969; Denzin 1972; Bruyn 1970a; Schatzmann and Strauss 1973.

30 Alfred Schutz and his followers in ethnomethodology refer less to "culture" as concept than to the life-worlds of the actors as basic systems of knowledge and interpretation, intersubjectively created and shared. They look at how culture is constituted and used, not necessarily in terms of subjective meaning but in terms of the rationales for action by which social acts are made accountable (Giddens 1976: 33-43; Schutz 1970: 72-95; Mitchell 1975: 141-48). They also argue that much of this stock of knowledge is tacit.

I.C. Jarvie (1964, 1972, 1983) follows their work and that of Karl Popper. He examines rules of logic which in fact can guide behaviour cross-culturally. Although Jarvie never completely reconciles the relative with the empirical in culture, he does point out the importance of looking at the institutional perpetuation of culture in consequence of intentional acts, as does F. A. Hanson (1975a, b), whose analyses of culture are similar. Hanson suggests that understanding rules does not result in understanding the subjective states of their users, but in comprehending how rules account for or predict behaviour in particular circumstances.

31 Clifford Geertz, too, has written extensively on the idea of culture in its various manifestations. Claiming heritage in the work of Weber and Parsons (Geertz 1973: 249-54; Rice 1980), his ideas of culture have always been closely tied to fieldwork. While adapting the interpretative frame of hermeneutics and other monistic traditions, Geertz has also advocated the possibility of objective scientific knowledge as valid. He argues that interpretive knowledge is the basis of empirical knowledge (Sass

1986: 52-3; Geertz 1979: 175-79, 1980). The idea of the symbol provides a link - while the symbol is created in the subjective recesses of meaning, it is accessible to observation and interpretation by the fieldworker in context, who can then "empirically" discuss its origins and functions. This is similar to what the rule theorists have argued.

32 Bourdieu's work has provided some recent inspiration for participant-observers reflecting upon the process of culture in the field. Influenced by phenomenology, Marxism, and positivism, Bourdieu (1977) tries to focus anthropological attention on the social creation of knowledge, much like the sociology of knowledge (Marcus and Fischer 1986: 84-5; Sass 1986: 54). He argues that the fieldworker projects not only bias and value but perspective on the field setting, thereby creating a version of culture true to his own interests. Bourdieu argues that social science must place more emphasis on "practice" in the production of meaning and less on cultural content itself.

Chapter Three

33 Information on the early contact period comes from accounts by explorers and traders in the 19th century, such as Hardisty 1867; Murray 1910; Jones 1867; and from the French missionary/ethnographer Emile Petitot (1970). Post contact ethnography on the Kutchin has been done by C. Osgood (1936) and R. Slobodin (1962) in the present century. See also S. Krech 1974, 1976, 1978a,b, 1979(cf); D. Smith 1975; Wolforth 1971; Stewart 1955; Eades 1971.

34 Information on the Mackenzie Inuit is sparse; few visitors recorded their lives before they were decimated by disease at the turn of the century. Some of the data come from accounts of explorer/scientists such as Franklin 1971 [1828], Simpson 1843; Richardson 1881; Hooper 1853; Armstrong 1857, and from the writings of Petitot (1981). We also have information from the Inuit writer Nuligak (1966), from the Anglican missionary Whittaker (1937), and from the ethnographic work of V. Stefansson (1913, 1919) and K. Rasmussen (Ostermann, ed., 1942). Usher (1971) (cf), McGhee (1974), and D. Smith (1984) have compiled some of this information.

35 Slobodin (1966) describes the growth of a northern Metis way of life distinct from Indian and European cultures, on the one hand, and from the Catholic Red River Metis culture on the other; these people retained the Protestantism of the Anglo-Scottish traders who came into the Mackenzie River area as well as aspects of northern Athapaskan culture (there were also people of mixed non-native/Inuit ancestry). Slobodin suggests that northern Metis participated more than other Native people did in the institutions of the fur trade. They incorporated some religious and family values from the Anglo-Scottish side, including an emphasis on the nuclear family, but they also utilized the extended family networks of Native kinsmen. Metis men tended to be mobile, participating both in the developing wage economy around the posts and in the bush economy. With their absence from the home, women often became strong figures in the household, counterbalancing European emphases on male authority in the home. Slobodin notes also that, with the decline of the paternalistic institutions of the fur trade, Metis people lost some of the power they had as cultural intermediaries and

tended to either become marginal to both Native and non-Native cultures or to blend into a general Native sub-culture.

36 John Franklin, on his second expedition to the western Arctic coast, 1825-27, explored Great Bear Lake and the coast from Alaska to the Coronation Gulf. While Franklin went west from the Delta, his associate Dr. John Richardson went east. Both encountered Mackenzie Inuit and published observations on them (Franklin 1971 [1828]). In 1837-39, Peter Dease, who had been on the second Franklin expedition, joined with Thomas Simpson to survey the coast from Franklin's furthest points west to Point Barrow and east to Chantrey Inlet, according to the missives of the HBC; Simpson (1843) also made notes on their encounters with the Loucheux and the Inuit.

Three other accounts of Delta people emerged from expeditions sent to search for the lost expedition (1845-48) of John Franklin. Richardson, who had a definite ethnological interest, produced observations from his 1847-49 trip down the Mackenzie and east to Coppermine (Richardson 1852). Alexander Armstrong (1857) was surgeon and naturalist for the ship Investigator, commanded by R. McClure, and wrote a record of his encounters with Mackenzie and Alaskan Inuit. McClure's ship entered the search from the west, coming up Bering Strait and moving east to Banks Island, where it was frozen in and had to be abandoned three years later, after two winterings. Its crew continued east on another ship to accomplish the Northwest Passage from the west. W.H. Hooper was on the Plover, commanded by W. Pullen, which also searched the western coast from Alaska eastward to Cape Bathurst (1849-50). This expedition went up the Mackenzie River to winter twice at Ft. Simpson; Hooper (1853) includes comparative notes on both Inuit and Loucheux (see also Baird 1949; Honigmann and Honigmann 1970; Finnie 1948).

37 There is a growing body of literature on the fur trade in this area and elsewhere in Canada, including some debate about the nature of timing of the consequences for Native people. While some are increasingly arguing that Native life did not change substantively in the early years of the trade, others hold to views that the impact was immediate and drastic (see Krech, ed., 1984; Asch 1986). To some extent this depends upon a correlate debate about whether the people took an active or a passive role in directing their own involvement in the trade. Evidence is increasing that many, including the Kutchin, did indeed take an active role, and that the relative dependence/independence of the parties depended upon both the nature of the local culture and relationship and on the changing circumstances of the trade itself, including the introduction of disease, of alternate transportation methods, and the nature of the competitive market itself. For description and analysis of the fur trade in the Lower Mackenzie and adjacent coast, see Krech 1974, 1976, 1979, 1981; Stager 1963, 1967; Usher 1971, 1971a, b; Wolforth 1971; Asch 1977, 1986; Nuligak 1966; Stewart 1955.

38 Saum (1965) has analysed trader/Indian relationships in North America, using the accounts and journals left by the traders. While noting the diverse orientations of the traders throughout the continent, he also suggests that most early relationships were dominated by the primarily economic and self-interested quality of trader/Indian strategies on both sides. The traders perceived the Indians much as the explorers did, according to the traits most obvious in the trading encounter, such as

physical appearance and custom, financial acumen, and the quality of manipulation of the interaction.

39 Key figures among the Kutchin included Fathers Grollier and Seguin, and the Anglicans W.W. Kirkby and Robert MacDonald, in the Peel River and Arctic Red River post areas. MacDonald, an Ojibway/Scottish Metis, married a Loucheux woman and spent his life among the people, translating hymns and prayers into the Kutchin language, training lay catechists, and educating the people (Grant 1984: 104-5; Stewart 1955: 290-317; Wootten 1966; Wolforth 1971: 33-7; Honigmann and Honigmann 1970: 35-7).

40 For analysis of the rules and structures of non-Native society, see Riches 1977; Koster 1977; Parsons 1968; Brody 1975. Brody, in particular, describes fully the nature of this society in Pond Inlet, in the eastern Arctic, in the late 1960s and early 1970s, including their relationships with the Inuit.

41 The Advisory Committee on Northern Development was chaired by DIAND, and its function was to coordinate the northern activities of the various government departments and agencies working in the north. Begun in 1948, it was dormant for a while, then re-activated shortly before the Aklavik decision was made. As will be seen, the ACND has had a role in setting northern development priorities and in directing northern research.

42 The Dene Nation was formed originally as the Indian Brotherhood of the NWT, in response to increased Native activism (which in itself was partly a response to the federal White Paper of 1969), to the desire to put forth a land claim, and to the discovery of oil -- with associated impending development pressures on the land -- in the Delta in 1970. In 1975 the Brotherhood issued the Dene Declaration, one of the first declarations of Native sovereignty in Canada, in which they proclaimed themselves a separate nation. They took an active role in the Mackenzie Valley Pipeline Inquiry, and soon after changed their name to the Dene Nation (1978). The organization represents status Indians in the territories, and it operates both through central offices in Yellowknife and the band council offices in the communities. The Dene Nation has been active in educational and cultural programs, in economic development projects, in sponsoring social research, and in land claims. They have filed a joint claim with the Metis Association, for land in the western subarctic which was never allocated to them when they signed Treaty 11. At the time of writing this claim is as yet unsettled, although an agreement-in-principle has been signed, and there are disputes about the boundary between Dene and Inuit territory to the east. The Dene Nation has also put forth a proposal for a government for the claims region, called Denendeh, which would incorporate several principles Indian traditional government (such as the authority of elders and consensual decision-making). It would be a step both toward Native self-government and possibly toward a provincial government for the west. The agreement-in-principle, signed in September of 1988, provided for lands and cash compensation, but was silent on aboriginal rights and self-government. Should the boundary disputes over the line for dividing the territories be resolved, however, the western NWT may move toward some kind of self-government for its Native citizens.

The Dene Nation has offices through the bands at Aklavik, Inuvik, McPherson, and Arctic Red River; Inuvik has only had a recognized band since 1982. Delta Indians

have also formed the Mackenzie Delta Regional Council, a group which proposes a separate regional Dene government and, possibly, a separate claim as well. The Dene also operate several business enterprises in the Delta as part of their economic development plans.

43 The Metis Association of the NWT has a similar history and function; it was begun about the same time as the Dene Nation by the Metis people of the upper Mackenzie River region. Originally it put forth separate proposals for land claims, but because Metis lands, and heritage, overlapped so extensively with that of the Dene, these claims were joined. Until recently many people of mixed ancestry lived with and like the Dene people, whereas others had distinctive traditions and occupations. The Metis Association, through its local branches, has helped to foster awareness of Metis identity in the Delta as elsewhere in the west.

44 The Committee for Original Peoples' Entitlement, or COPE, was a significant presence in the Delta in 1981 and has remained so. Its central offices were in Inuvik, with local offices in the other Inuit communities, including Aklavik, Tuktoyaktuk, Paulatuk, Holman Island, and Sachs Harbour. It was begun in 1970 to protect all Native interests in the Delta/Beaufort region; by 1973 the Inuit farther east had joined with the Inuit Tapiritsat, and the Dene and Metis had split to join their own representative groups, so that COPE became the organization of the approximately 25000 Inuvialuit of the western Arctic. They filed a separate land claim in 1976, reached an agreement-in-principle with the Office of Native Claims of the federal government in 1978, and concluded their land claim in 1984, after the time of fieldwork. In the meantime, they had become active in areas such as environmental/cultural safeguards against development impacts, language and communication, wildlife management, and business and economic development; in 1981, for example, they had a country foods outlet, a muskox harvest, guided arctic hunts, a taxi company, and real estate investments. These ventures were begun and expanded with monies received in advance against the settlement. COPE also began to negotiate with the territorial government toward the eventual structuring of a western Arctic regional government, a plan which has not yet come to fruition (see Usher 1973 and researcher p.c. July 19b, 1982 for history).

Some of COPE's other plans did meet success with the 1984 settlement, which included a \$45 million cash settlement to be paid by 1997, in compensation for 344,000 square kilometres of lands relinquished. Title to 91,000 square kilometres of traditional land was obtained, graded by surface and subsurface rights into various categories. Lands and monies are to be held by various corporations owned collectively by the Inuit, and a Social Development Fund was set up for researching and addressing social problems. These structures have been set up, and COPE has further expanded its social and economic activities, including a land use agreement signed with Esso Resources over future exploration of Inuvialuit lands (Inuvik Drum 1984: 19 (46)). This represents part of the new arrangements of partnership being made in the region between Native hosts and industrial developers.

Chapter Four

45 This evolved from a grants-in-aid program, begun earlier in the century, of sponsoring long-term northern research through institutions, expeditions, and individuals.

46 This has included a federal review of northern science in 1972, and the issuance of Guidelines for Scientific Activities in Northern Canada in 1976.

47 Since 1963 the National Museum of Civilization has been under the Office of the Secretary of State. The Museum has sponsored relatively little ethnological work in the Delta; it has sponsored some archaeological work, including recent survey and excavations west of the Delta at Old Crow, Yukon. Since 1972 the Museum's Urgent Ethnology program set as its priorities the sponsorship of research in areas where traditional culture was alive yet in need of immediate recording. Since the Delta was considered a place where little traditional culture yet flourished, it has not been a priority area for urgent ethnology (researchers p.c. Oct. 4, 1982; Dec. 10, 1982).

48 Although the writer does not have much information on how the funds have been used, one of the projects funded in 1985 was the Arctic Science and Technology Information System (a bibliography) from the Arctic Institute of North America (Northline January 1985: 5).

49 In 1960 AINA asked NANR, through the NCRC, to review project applications for merit and relevance, for example (PAC, RG85, v. 1229, file 1002-64-AINA 1953-60). The Northern Administration Branch was asked for suggestions concerning a new AINA director in 1964, and in 1967 the government was approached to back a major AINA-directed Centennial Fund for Northern Research (PAC, RG85, v. 1892, file 1002-62 (pt. 6) AINA).

Chapter Seven

50 The Special Committee on Education of the GNWT Legislative Assembly, 1980-82, recommended the formation of an Arctic College with decentralized campuses. The Adult Vocational Training Centre, built in 1968 in Fort Smith, became Thebacha College in 1981, and thence the nucleus of the Arctic College which was put into place in 1985. By 1988 campuses in Fort Smith, Iqaluit, Inuvik, and Yellowknife had a range of offerings including trades courses, certificates in business administration and clerical work, a teacher training program, and a selection of first year university courses (see Cleveland 1987 for description of the evolution of the college).

51 The ACUNS Canadian Northern Studies Trust Fund offers studentships and scholarships for Native and non-Native students. In 1985 a series of scholarships for Native students doing post-graduate work in economic development was instituted, in association with the Native Economic Development Program begun in the same year. The Trust has also held national northern student conferences, and they have begun a new program of logistical support for students in the physical and biological sciences (Northline Oct. 1987: 4).

⁵² Before the Inuvik Lab was turned over to the Science Institute for management, it was administered by a working group within the ACND sub-committee on Science and Technology, headed by DINA research staff.

⁵³ ACUNS has a series of objectives, including the advancement of northern research, education, and programs, liaison with other northern-related bodies, stimulation of support for northern studies, networking among institutional members, and to develop reciprocal relationships with northern peoples on matters of science concern. To meet these objectives, ACUNS has sponsored conferences (which have brought together the actors in northern research); it has administered DINA's northern training grant program; it has advised DINA on science matters; it has prepared special publications and services; and it has lobbied for research funding.

⁵⁴ These responses appeared in a letter from the then-President of ACUNS, J.K. Stager, to the then-Minister of Northern Affairs, Hugh Faulkner, dated May 2, 1979; they also appeared in a draft of comments by the Executive Director, P. Campbell Mackie, in consultation with the membership in 1981 (Mackie 1981). A discussion paper on the National Polar Institute was prepared for ACUNS by Kubiski and Associates (1986).

Chapter Eight

⁵⁵ Even then some scientists questioned this. Franz Boas, for example, was censured by American Anthropological Association in 1919 for his denunciation of scientists who worked for government or other interest groups as "spies" while researching an independent "truth" (in T. Weaver, ed., 1973:51-2).

⁵⁶ These scandals included the involvement of social scientists in the U.S. Department of Defense "Project Camelot" program of counter-insurgency, and in supplying information to counter-insurgency initiatives in Thailand (Agar 1980: 55; Jorgenson 1971:322; Jones 1971).

⁵⁷ The codes examined included the American Anthropological Association, Principles of Professional Responsibility, 1970; Society for Applied Anthropology, Statement on Ethics, 1973; Canadian Sociology and Anthropology Association, Code of Professional Ethics, 1978; Social Sciences and Humanities Research Council of Canada, Ethical Guidelines for Institutional Review Committees for Research with Human Subjects, 1981.

APPENDIX I

LIST OF ACRONYMS

- ACND - Advisory Committee on Northern Development
ACUNS - Association of Canadian Universities for Northern Studies
AINA - Arctic Institute of North America
CARC - Canadian Arctic Resources Committee
COPE - Committee for Original People's Entitlement
DEW - Distant Early Warning
DIAND - Department of Indian Affairs and Northern Development
DINA - Department of Indian and Northern Affairs
GNWT - Government of the Northwest Territories
INAC - Indian and Northern Affairs Canada
ITC - Inuit Tapirisat of Canada
MAB - Man and Biosphere
MIN - Man in the North
MDRP - Mackenzie Delta Research Project
MOSST - Ministry of State for Science and Technology
NANR - Northern Affairs and Natural Resources
NCRC - Northern Coordination and Research Centre
NRCC - Northern Research and Coordination Centre
NSRD - Northern Social Research Division
JWT - Northwest Territories
AG - Public Archives of Canada
3- Science Advisory Board

SCC - Science Council of Canada

SSHRC - Social Sciences and Humanities Research Council of Canada

UNESCO - United Nations Educational, Scientific, and Cultural Organization

APPENDIX II

LIST OF CONTRIBUTORS

[The people listed below were those who consented to be interviewed about anthropology and its political context in the North. They are referenced in the text by the abbreviation "p.c." and the date of the interview. Names were not inserted in the text to preserve the anonymity of the contributors. There were other people who occasionally contributed information but were not interviewed as such.]

Emma Dick
Derek Smith
Dick Clarke
Lloyd Binder
Dorothy Arey
Rosie Albert
Monica Peiper
JoAnne Lowe
Peter Westaway
Joe Veselisin
Tony Stewart
Brian Townley
Larry Osgood
Don MacWatt
Marie McInnes
James Martin
Richard Slobodin
Audrea Loreen
Skip Koolage
Cynthia Hill

Peggy Curtis
Father Croteau
Ruth Carroll
Doug Billingsley
Rennie Arey
Sophie Kayotuk
John Ostrick
Ishmael Alunik
Balbir Kuar
Angela Veselisin
Arlie Townley
Jennifer Rigby
Dan O'Neill
Danny Norris
Derek MacGregor
Agnes Semmler
Peter Usher
John Matthiasson
Dick Hill
Ruth Pulk

Nels Pulk
Ross McKay
Robert Williamson
George Miller
Geoff Dixon
Donat Savoie
Cece McCauley
Charles Hobart
Terry Norwegian
Victor Allen
Agnes Jones
Frank Hansen
Knute Hansen
Cezar Heine
Danny C. Gordon
Olive Gordon
Mary Gardlund
George Edwards
Milton Freeman
José Mailhot (by letter)
Don Bissett
Terry Fenge
Katie Cooke
Ellen Bielawski
Bob Bell

Graham Rowley
A.M. (Sandy) Ervin
A.J. Kerr
Keith Havens
Rosalind Dixon
Vic Valentine
Michael Asch
Vicky Paraschak
Edward Lennie
Bertha Allen
Greg Komaromi
Ted Hayes
Ron Hawkins
Kathy Greenland
Charlie Furlong
Sam Gargan
Bev Elliott
Ed Duggan
Percy Ionel
Edith Carter
George Parsons
James VanStone
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