

INTRODUCTORY NOTE ON “THE METHOD OF ETHNOGRAPHIC RECONSTRUCTION” BY ERNEST S. BURCH, JR.

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In September 2010, Ernest S. (Tiger) Burch, Jr., unexpectedly passed away at his home in Camp Hill, Pennsylvania, at the age of seventy-two. The loss of northern anthropology's most eminent ethnographer was a shock to his many friends and colleagues, some of whom have included retrospection on Burch's impressive career as part of their grieving process. Among other results, this exercise renewed interest among certain of his colleagues in one important paper he had never published.

The paper in question is titled “The Method of Ethnographic Reconstruction.” Tiger may have started writing it as early as about 1975, but his first presentation on the paper's subject matter took place in March 1981 at the Eighth Annual Meeting of the Alaska Anthropological Association in Anchorage. In October 1988, he presented an expanded version of the paper at the Sixth Inuit Studies Conference in Copenhagen. The latter is the version included in this issue of the *Alaska Journal of Anthropology*. The manuscript was provided by Tiger's dear friend Igor Krupnik and is published with the permission of Deanne Burch, Tiger's wife. Legitimately, some readers may question the editorial decision to publish this paper the way Tiger had written it more than twenty years ago, and with only minor revisions. The following comments are meant to address any such concerns by placing the paper in its proper historical context.¹

Tiger Burch was an extremely productive, widely respected scholar who had an unusually high commitment to transparency. That is, Burch was diligent about stating his research objectives, explicitly identifying and defining the methods and concepts he was using to attain them,

and openly acknowledging his own past errors of fact or interpretation (see, e.g., Burch 1991a). His research was always thorough, and the associated findings were reported in clear and precise language. He carefully evaluated existing concepts relevant to his work, often modifying them to improve their utility; if his research indicated a concept was unsound, Tiger did not hesitate to call for its rejection (e.g., Burch 1976; cf. Burch 1998:307–308).

His deep commitment to scientific research and improving the methods, concepts, and techniques of anthropology are plainly evident in this paper. It is uncertain why Tiger failed to submit the paper for publication, but he may have considered it too long and didactic for an anthropological journal. The paper was unquestionably intended to be an educational tool; as such, it will become part of Burch's enduring contribution to anthropology. In this paper, Burch describes in detail a methodology he developed through trial and error for the conduct of “retrospective research,” the domain in which the majority of his work occurred. Significantly, the paper pays particular attention to an array of issues related to the collection, evaluation, and utilization of oral history data from indigenous populations. This is noteworthy given that Tiger devoted much of his career to demonstrating oral history's relevance to and reliability in ethnographic reconstructions.

When Tiger first began to emphasize and heavily rely upon oral history accounts in his work he was bucking existing scholarly trends, especially with regard to his conviction that oral accounts could illuminate events from the deep past (i.e., the late 1700s, early 1800s). Fellow social scientists and other scholars were skeptical of that position.

1. After careful deliberation, one thing that was not modified is the male bias (i.e., references to male informants and male investigators; no use of the female pronoun) that permeates the text, since it reflects the state of the discipline at the time the paper was written. Given the chance, however, Tiger would surely have corrected this bias; he did have female informants and he recognized and respected his professional female colleagues.

His paper “From Skeptic to Believer: The Making of an Oral Historian” (Burch 1991b; cf. Burch 1996) was a direct response to such criticism: in it Tiger admits that he formerly shared his critics’ attitudes about the limitations of oral history, an admission that made the paper all the more effective. His manuscript on “The Method of Ethnographic Reconstruction” was the foundation for many of the arguments he made regarding the validity of oral history as a source of historical information. Accordingly, it also had a major role in the formulation of Tiger’s remarkable and encyclopedic trilogy on the Iñupiaq peoples of Northwest Alaska (Burch 1998, 2005, 2006).²

It has now become standard practice for anthropologists to incorporate oral data in discussions concerning the history of Alaska Native peoples; Burch’s extensive work with indigenous oral history no doubt helped bring about this development. The growing public and scientific interest in processes and impacts of climate change in Alaska will further increase the consideration given to Native oral history. Although the increased appreciation of research involving oral history is a positive change, it also has some drawbacks. Most notably, the trendiness of oral history research in Alaska gives rise to numerous quality control problems. Some researchers treat the data produced in ways that suggest virtually every Alaska Native oral account is important and has historical validity. In other words, oral history data—and the informants from whom such data derive—often are not subjected to the types of critical evaluation required to verify their accuracy and demonstrate their relevance as information sources for scientific research. This is contrary to the scholarly rigor characteristic of Tiger’s own work with oral history and to the valuable guidance he provides in “The Method of Ethnographic Reconstruction.”

The publication of this paper makes it accessible to students and scholars interested in learning the tenets of ethnographic reconstruction and/or oral history research with indigenous peoples. By extension, Tiger’s main objective in writing the paper may finally be realized: i.e., anthropologists and other scientists have an opportunity to learn from his mistakes and the many practical insights he gained through over fifty years of anthropological work in the North.

ACKNOWLEDGEMENTS

I wish to thank Deanne Burch for granting the *Alaska Journal of Anthropology* permission to publish “The Method of Ethnographic Reconstruction.” I also thank Erica Hill for editorial assistance in preparing the paper for publication, and Igor Krupnik for helpful comments on a draft of this introductory note.

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2. Less obviously, the paper also complements his effort to develop an approach that might enable archaeologists to reconstruct prehistoric societies in northwest Alaska (Burch 1988).

THE METHOD OF ETHNOGRAPHIC RECONSTRUCTION¹

Ernest S. Burch, Jr.

ABSTRACT

This paper outlines an approach whereby one can acquire accurate knowledge of past events, ways of life, and individuals by means of orally transmitted information. Included in the presentation are discussions of (1) types of evidence, (2) sources of information, (3) time, (4) space, (5) subject matter, (6) bias, and (7) tests of reliability. General points are illustrated with examples from the author's research on the histories of the Caribou Inuit of the central Canadian subarctic and the North Alaskan Inuit [Iñupiat].

KEYWORDS: Alaska, Iñupiat, Caribou Inuit, oral history, ethnohistory

INTRODUCTION

The need to describe phenomena that existed at some time in the past is common in social research. The variety of studies that might have need of retrospective information is vast, ranging from the reconstruction of life in a small camp of fifty thousand years ago to the determination of the details of a festival that has just been completed. One critical problem confronting all retrospective studies, regardless of their time depth or scope, is the fact that the investigator cannot see or measure directly the very phenomena he wishes to describe. Consequently it is necessary to rely on indirect evidence, be it a set of artifacts or other physical remains that have been left behind or the statements of others who witnessed or participated in a particular event.

Within anthropology there are two basic approaches to the reconstruction of past phenomena: archaeology and ethnohistory. The former involves making inferences about social, cultural, or demographic phenomena on the basis of the physical evidence the people concerned left behind. Ethnohistory, on the other hand, depends for its

information on written documents, which may or (more often) may not have been written with anthropological issues in mind. Archaeology and ethnohistory are major disciplines and their methods and techniques are the topic of considerable discussion, debate, and publication. An approach that has received relatively little systematic treatment in this regard is ethnography, yet it, too, has considerable value in retrospective research.

Ethnography is generally conceived of as the description of ongoing social systems, i.e., those of which the investigator can obtain at least some firsthand experience and observation. However, everyone who has ever done ethnographic research has had to inquire about customs that are no longer practiced, beliefs that are no longer held, or periodic events that do not happen to take place while one is in the field. Other investigators go further and try to reconstruct, on the basis of interviews, performances, or other sorts of oral evidence, social or demographic patterns and even entire societies that no longer exist. But regardless of variations in time

1. Ernest S. "Tiger" Burch, Jr., died in September 2010, leaving several projects unfinished. Igor Krupnik, Tiger's friend of many years, had a copy of the manuscript of this paper, which was originally written between 1981 and 1988 and presented at the 1988 Inuit Studies Conference in Copenhagen. Deanne Burch, Tiger's wife, kindly granted permission to publish it in *AJA*. As discussed in Kenneth Pratt's "Introductory Note" (pp. 125–126), the paper is reproduced here as originally written and with very minor edits. Although he avoided the exclusive use of the masculine pronoun in reference to informants and investigators in his later works, Tiger's original usage has been retained here, as it reflects scholarly attitudes at the time.—*Ed.*

depth or subject matter, the general problem of retrospective research is there.

At least some authors (e.g., Lee and DeVore 1968:146, 148) contend that ethnographic reconstruction beyond a very short period of time is impossible. Similar opinions have been expressed to me informally by both students and experienced field workers. According to this view, if contemporary written accounts do not exist, thus permitting the corroboration of field data through the use of ethnohistorical material, a reliable reconstruction simply cannot be made. Vansina (1985), however, has demonstrated that this position is untenable, at least as a general proposition. My own experience has shown it to be false even in the study of hunters and gatherers, people often considered as having been devoid of history prior to contact with Europeans. In principle, at least, the reconstruction of the past can be carried out by means of ethnographic techniques in *any* kind of society. Those who fail to make use of ethnography in dealing with historical questions often cut themselves off from an extensive body of valuable information.

The major purpose of this paper is to present a method whereby ethnographic reconstruction can be done. In other words, I outline an approach whereby one can collect reliable information about the past through personal contact with living people. By "method," I refer to the logic, or type of reasoning, that underlies this type of research. The focus is on the middle ground between epistemology, on the one hand, and the specific techniques or procedures of data collection, on the other. Although my remarks obviously relate to both of those areas, to deal adequately with the former would require a philosophical treatise, whereas many of the latter already have been described elsewhere (e.g., Dean and Whyte 1958; Irvine 1978; Langness 1965; Lewis 1962; McCracken 1974; Rogers and Black Rogers 1978; Sitton et al. 1983). Many of the elements of this approach have been discussed by others, particularly in Vansina's excellent work (e.g., 1970, 1985), but I attempt to systematize them more rigorously here than they have been heretofore.

A second objective of this paper is to present a statement of the strategy upon which much of my own recent research has been based. For nearly two decades I have been attempting to reconstruct the social and demographic structures of two Eskimo populations. One is the Inuit-speaking Eskimo population of Alaska as it was in the early and middle nineteenth century; it is referred to subsequently as the "traditional North Alaskan Inuit" (see

e.g., Burch 1980). The other study population is "the traditional Caribou Inuit," who lived in the central Canadian Subarctic, immediately west of Hudson Bay [in what is today Nunavut], during the late nineteenth century (e.g., Burch 1986). I do not claim to have applied the method fully: indeed, I made a number of mistakes. But it was precisely in the respects and to the extent that I failed to apply it that my most serious errors were made. Thus, while I illustrate general points with reference to my own research, the examples are sometimes presented as ones to avoid rather than ones to emulate.

TYPES OF EVIDENCE

The raw material of ethnographic reconstruction consists, in principle, of any kind of oral expression that, either directly or by allusion, refers to an event, tradition, social system, individual, population, or state of affairs that existed at some previous point in time. Examples include, but are not necessarily limited to, the following: narratives, performances, dances, epic songs and poems, tales, legends, myths, proverbs, riddles, jokes, anecdotes, and even offhand remarks (Dundes 1968; Vansina 1985:3–26). At the most general level, virtually any utterance should be considered a possible source of information about the past.

The major criteria for determining the relative significance of the several possible forms of evidence in any given study should consist of (1) the local cultural emphasis with regard to the conveyance of historical information, (2) the specific subject matter of one's research, and (3) the degree of time depth being sought. If the local emphasis is on dramatic performances, then they will provide more information than legends and myths. If one is attempting to reconstruct an event that happened only a few months ago, anecdotes and offhand remarks, perhaps augmented by question-and-answer sessions with knowledgeable informants, may suffice. If one is attempting to reconstruct a battle that took place three thousand years ago, epic songs, poems, and legends may be more informative.

The basic point here is that there is no *a priori* or programmatic way to determine which of the several possible forms of ethnographic evidence is best. The answer to the question will vary according to the time depth, possibly the subject matter of primary interest to the researcher, and the particular tradition by which historical information is conveyed in the culture concerned. While the former may be known prior to the research, the latter

may require both intensive and protracted field research to determine.

SOURCES OF INFORMATION

Ethnographic data, by definition, are those derived through the personal contact of an investigator with living people. Members of the population with whom the investigator actually makes contact may be referred to as the “resource population” who live in the “research period.” The population whose culture (or whatever) is being reconstructed may be called the “study population,” and they lived in the “study period.” The resource population may—or may not—consist in whole or in part of people who previously belonged to the study population, and its members may—or may not—be directly descended from the study population. The individuals who actually provide information to the investigator, by whatever means, are “informants.”

The absolute prerequisite of any successful reconstruction is a supply of qualified informants. Considerable care must be paid to their qualifications, recruitment, handling, and evaluation.

THE AVAILABILITY OF QUALIFIED INFORMANTS

The presence or absence of qualified informants in any resource population depends on a number of variables. These include the following: (1) the local emphasis on the retention of historical knowledge, (2) the nature of historical criticism in the resource population, (3) the education system, (4) transmission problems, (5) retention problems, and (6) the time depth of the research.

“Historical knowledge” is defined as any knowledge, information, supposition or allegation about past situations, events, people, things, and processes. The extent to which the retention of such knowledge is considered important by members of a resource population obviously has significant bearing on the number of individuals in that population who are likely to be qualified informants. The greater the *general* interest in historical matters, the greater the number of qualified informants there is likely to be. The possibilities here vary considerably, from an almost total rejection of the past as a topic of interest, at one extreme, to a general fascination with and systematic retention of historical information, at the other. A related variable is the specific type of information concerned, since even in populations where historical data are collect-

ed and retained, some kinds of information will be considered important while others will be virtually ignored. Sturtevant (1968:464–465) has described a number of examples illustrating this variation. I focus my attention here on two arctic populations with which I have experience.

The Caribou Inuit and the North Alaskan Inuit [Iñupiat] differ considerably in the extent to which they value the retention of historical information. The North Alaskan Inuit have a relatively well-defined historical interest, and they draw a distinction between legendary and factual accounts (Jenness 1924:1–2; Rainey 1947:269), whereas neither was true for the Caribou Inuit. Members of both populations, however, had a keen interest in folklore (Ostermann and Holtved 1952; Rasmussen 1930).

In North Alaska, for at least the last several generations, there have been a number of individuals who have been interested in remembering the present and learning about the past. This interest was not purely idiosyncratic since such individuals became recognized and appreciated as historians, they were consulted as authorities by their contemporaries, and they were in increasing demand as sources of information as their knowledge grew. I interviewed several native people who had previously and independently conducted self-conscious research into various historical questions. They had done so by means of systematic interviews with older people who had either lived during the period in question or who had similarly investigated the topic among representatives of still earlier generations.

Among the Caribou Inuit, on the other hand, this historical interest was relatively poorly developed. The members of this population tended to be much more existential in their orientation, focusing on the present and immediate future and caring rather little about the past. The best Caribou Inuit informants could present a general account of their own life experiences, and they could generalize reasonably well about the customs and practices they had observed during their youth. However, they were able to recall relatively little about specific events and places, and they knew even less about events, places, or individuals in the years before they were born.

The nature of the historical criticism that is practiced in a resource population is a second important variable affecting the general availability of informants. As Pitt (1972:55) and Sturtevant (1968:464–465) have pointed out, even among peoples where the collection and retention of historical knowledge are important, there can still be wide variation in the specific criteria used to evaluate

historical sources. Consequently there can be different considerations employed in handling information, and even different types of information collected between one society and another. In one population there may be considerable emphasis on factual accuracy, while in another there may be complete disregard for it as long as certain esthetic standards are met. Other variables include (1) the form in which information is presented; (2) personal characteristics, such as the age, sex, or wealth of the source; (3) the amount of detail; (4) the political or religious significance of an account; (5) chronology; (6) entertainment value; and (7) relative emphasis on ideal versus actual patterns.

The two resource populations in my research differed in the nature of their historical criticism. Among the North Alaskan Inuit, sources are evaluated primarily in terms of four criteria: factual accuracy, proper sequence of events, the amount of detail, and the manner of presentation, in that order of priority. The most devastating criticism that they make of their own historians is that they got the facts or the sequence wrong. Storytellers who make such errors are privately ridiculed and, while their presentations are tolerated, they are otherwise ignored. A secondary consideration is the amount of detail included in an account. Sources who recall vast numbers of details are highly respected—as long as the facts and their sequence are correct. In addition, sources are evaluated with respect to their style of presentation, good speakers being appreciated more than poor ones—as long as the other three criteria have been met. Finally, the North Alaskan Inuit distinguish clearly between the ideal and the actual. Informants who rank high according to the other criteria often go to some lengths to make certain that the anthropologist-interviewer keeps the two separated with equal clarity.

The Caribou Inuit, by contrast, have little interest in sequence, and they are not particularly concerned with the differences between actual and ideal. Consequently, except when dealing with personal experiences of fairly recent date, informants tend to generalize rather vaguely, report ideal rather than actual patterns, and are often difficult to pin down on specific illustrations. But, since they are not particularly interested in history anyway, it is not surprising that their criteria of historical criticism are not well developed. On a more positive side, the best Caribou Inuit informants, like their North Alaskan counterparts, rarely present speculation under the guise of fact. If they

do not know something they tell you so rather than guessing about what might have been the case.

The education system is a third important variable affecting the general availability of informants in a resource population. In general, it can be assumed that the frequency of qualified informants will vary in direct proportion to the extent to which historical information is an element in the education of the population as a whole. If such information is transmitted to both males and females, for example, the pool of potential informants will be much greater than if it is passed on to just males. A second aspect of the education factor is the span of time over which the relevant education is carried out. If instruction is limited to a few weeks or months prior to an initiation ceremony of some kind, there will be a strong emphasis on excellent memory. The number of good informants in the resource population will be lower under those conditions than if historical education begins in early childhood and continues to be reinforced and expanded more or less uninterrupted throughout one's life. On the other hand, the comparatively few informants of the first type may be exceptionally well informed.

A third consideration is who does the teaching. Is historical teaching carried out by recognized experts in the field, or is it left to each set of parents to conduct with respect to their own offspring? If the latter there may be a large *number* of informants in the resource population, but they may not be as knowledgeable as they would be if specialists were involved in the process at some point.

The two resource populations of concern here were similar in that historical instruction was not restricted to any particular age group or to the members of one sex; hence historical instruction was an element in the education of the general membership of the population. Everyone had some knowledge of history. However, since the Caribou Inuit were not particularly concerned with teaching history in any case, instruction in that area was unsystematic, and it was not subject to quality control by way of explicit criticism of historical accounts. In North Alaska, on the other hand, history was a subject of general interest, and discourses in historical matters were subject to criticism. In both populations historical information was transmitted by both experts and "laymen," but there was a clearer distinction between the two in North Alaska. One knew who should be believed and who should be ignored by the time one reached adolescence.

A fourth variable affecting the number of informants in a resource population is the number and severity of

“transmission problems.” By this I mean an event or process that inhibits or prevents the transmission of historical information from one generation to the next. There seem to be three relatively common information bottlenecks which can break the continuity of transmission of historical information and which can limit the supply of knowledgeable people in a resource population as a consequence. These three are: (1) disaster of some kind, (2) population movement, and (3) rapid and profound cultural change.

Of the many types of disaster that can befall a population, famines and epidemics seem to have the most serious consequences for the transmission of historical information. Such events typically eliminate the oldest and the youngest age groups, which represent the major sources and the primary recipients, respectively, of historical knowledge in the population at the time. Famine may be the more detrimental of the two because even survivors seem to experience some memory loss as a result of the experience, apparently for physiological reasons (Mayer 1975, 1976). Other calamities, such as war, earthquakes, and floods may constitute less serious intellectual bottlenecks if they do not lead to a famine or epidemic and if they do not strike particularly hard at the older members of the population concerned.

Both of the resource populations in my research have been subject to severe epidemics and serious famines, and a tremendous amount of historical knowledge has been lost as a result. In North Alaska such disasters struck with particular force during the 1880s, while in the Caribou Inuit area the years from about 1915 to 1925 seem to have been the most critical ones. There was a difference between the two areas in the scope of specific events, however, one that accounts at least in part for the greater availability of reliable historians in North Alaska. In Alaska most famines and epidemics seem to have been local or regional rather than all-encompassing in scope. At least some people knowledgeable about the history of a particular group were normally living or visiting in another region when disaster struck their own country. Subsequently, they were able to bridge, at least in part, the information gap resulting from it.

The same cannot be said about the Caribou Inuit, who experienced at least two disasters in the twentieth century that reached every inhabited locality in their country more or less simultaneously. The first and most serious was the famine of 1915–1925, which led to the deaths of more than half of the human population. A famine of this magnitude would have had particularly severe effects

on the older segment of the population. The second general disaster came in 1956–1957, when famine and disease struck the Caribou Inuit area. This event resulted in many deaths, but its consequences were ameliorated to some extent through the provision of government welfare and medical aid to the stricken population. In addition to these two general disasters, the Caribou Inuit have been subjected to frequent local and regional famines and epidemics of various kinds since the early twentieth century. I strongly suspect, although I cannot demonstrate, that it was this succession of disasters and not a fundamental disinterest in the past that led to the comparatively low level of historical knowledge in the modern population. Members of neighboring Inuit populations seem to have had a higher level of recall than members of the Caribou Inuit population (Arima 1976).

Population movements can also affect the availability of informants in a resource population, even if they are not associated with disaster of some kind. Emigration, for example, can disperse the members of a population and result in their absorption into other groups with different histories and traditions. Even when emigration does not have a dispersal effect, it takes people out of the country in which their previous history occurred. Far from landmarks and historical places of one’s youth and/or ancestors, it becomes increasingly difficult to retain the knowledge of what transpired there.

Population movement has significantly affected the distribution of informants in North Alaska. The descendants of people from virtually every region are now scattered over a huge area, while at the same time the populations of individual villages tend to be made up of people whose ancestors lived elsewhere. Kivalina, for example, is made up primarily of people of Upper Noatak, Point Hope, and Seward Peninsula ancestry, supplemented by just a few descendants of mid-nineteenth century Kivalina people. If one wants to reconstruct the 1905–1915 period in the region, one can obtain much useful information from informants in the village. But if one wants instead to reconstruct the situation as it was in the 1870s or earlier, one has to seek informants elsewhere.

The third and final transmission problem is rapid social change. Perhaps the ultimate development in this direction took place during the 1940s, when World War II brought the abrupt arrival of thousands of foreign men, tons of mechanized equipment, and radically new ways of life to many areas previously isolated from the rest of the world. Suddenly the past history of the indigenous populations of

such areas became irrelevant, and people simply failed to pass on information about it to their descendents.

In both the Caribou Inuit and North Alaskan areas, social change, though rapid, was gradual enough until recently so as not to constitute a serious hindrance to the transmission of historical knowledge. There are a few exceptions to this rule, however. For example, in some North Alaskan villages during the mid-1950s there was an abrupt change in the language used in the home. On the assumption that ability to speak English was an important factor in getting good jobs, bilingual parents, who constituted a majority of the population in villages concerned, began talking to their children exclusively in English. One result of this trend was that many monolingual Inuit-speaking elders could not communicate effectively with their equally monolingual English-speaking grandchildren. The result was a significant hiatus in the transmission of information across generational lines. Recently there has been a strong reaction against this trend, but it has come too late to retrieve much that was lost during the previous thirty years.

The final variable affecting the general availability of informants in a resource population is the time depth of the research. If one is attempting to reconstruct a system that was in operation only five years previously, virtually anyone over about six or seven years of age might be able to provide at least some information on that system as a result of their personal participation in it. The further back in time the study period is, the smaller the supply of informed people is likely to be. Although the *best* informants are not necessarily individuals who were themselves members of the study population, the greatest *number* of informants in a resource population is likely to be found where survivors of the study population are fairly numerous.

In my Caribou Inuit research the study period was 1890–1910, but the field research was carried out in 1968–1970. The time gap between the two was thus sixty to eighty years. In 1970 there were only forty-seven people in the resource population of 1,271 (or 3.7%) who had been alive during the study period (Canada DIAND 1971), and many of them had not been members of the study population. The number of potentially helpful informants was much greater than that, of course, because many aspects of traditional life in the central Canadian Subarctic persisted for many years after 1910. If I had been trying to reconstruct the 1930 situation instead, I could have drawn on a pool of 198 individuals (or 15.6% of the resource population) who had actually been one year old or older during

the study period (Canada DIAND 1971); the number of qualified informants would have been greater still.

In North Alaska, on the other hand, the study period was the early and middle nineteenth century; my research was carried out after 1965. In this case not a single member of the study population survived to be a part of the resource population. Indeed, all of the members of the latter had been born long after the study period had ended. This meant that reliable informants, if they existed at all, were likely to be specialists in historical matters, and they would have to be located, recruited, and interviewed on that basis. None of them could provide any information on the study population as a result of personal experience. In the Caribou Inuit case, despite the fact that relatively few expert historians were to be found in the resource population, a few dozen individuals could provide information about the resource population on the basis of personal experience. Careful interviewing elicited much valuable information from such individuals despite their lack of interest in historical issues.

THE SELECTION OF INFORMANTS

The selection of informants involves a number of steps, the precise number depending on the availability of candidates in the resource population. In general, the greater the number of candidates, the greater the need for careful sampling procedures of some kind. The smaller the number, the greater the emphasis can be on a complete coverage of the individuals involved.

Even where the number of informants is not particularly large, it is often useful to select a stratified sample just to make sure that the most important aspects of one's study have been investigated. The criteria used to establish cohorts may vary widely, of course, depending on the time depth, the geographic breadth, and the general subject matter to be covered by the study. Someone interested in learning how women used to make and decorate pots twenty years ago will certainly be concerned with a different set of criteria than someone interested in how men used to hunt whales a century ago.

In my research in the both the Caribou and North Alaskan Inuit areas I was particularly concerned with geographic breadth because I was interested in reconstructing the general social and demographic situation over two large geographic areas. In particular, I was interested in the locations of major social and ethnic boundaries. The resource populations, however, were concentrated in a rel-

atively small number of communities, and large sections of both study areas were uninhabited at the time I did my research. Therefore, the primary criterion in stratifying the samples was the specific region about which people had the greatest knowledge. On the basis of prior library research and preliminary interviews, the Caribou Inuit area was divided into five districts; in Alaska the number of districts began with only three, but expanded to twenty-five as the research proceeded. Cohorts consisted of individuals known or thought to be knowledgeable about each of these districts.

Some cohorts turned out to contain zero individuals and some just one or two, while others contained a dozen or more. It was necessary to try to get information from everyone in the smaller cohorts. With respect to the larger ones, further sampling criteria had to be applied because I lacked the time to interview the entire group. In choosing these additional samples, I sought the expertise of people with the most comprehensive knowledge of the region in question.

To determine expertise I had to make preliminary contact with the individuals involved and interview them informally so as to form an (admittedly subjective) assessment of how much they knew. Sometimes I made what were found to be serious mistakes because these initial assessments were wrong. More often, however, I was able to make good choices because I took advantage of other knowledge to locate qualified individuals. In Alaska, for example, some individuals were *known* to be experts on a particular area. All I had to do was ask who they were. In cases where no obvious experts were available, I relied on my knowledge of Inuit socialization techniques to guide me. During preliminary surveys I inquired about who was raised by their grandparents, and for what length of time each had lived with them. Some of my best informants turned out to be individuals who had not lived in the region in question, but who had grown up in households that included grandparents who had.

The selection of a sample is, of course, just the first step in the exercise. There inevitably follow the tasks of (1) locating and establishing rapport with the individuals selected, (2) discovering which of the individuals chosen have the memory, physical endurance, interest, and time to be effective informants, and (3) actually conducting the interviews. For numerous practical reasons, the initial sample often has to be revised. All of these matters fall under the heading of “techniques” rather than method. Hence they fall outside the scope of this paper; they are

also covered in every general text on field research procedures. But I cannot emphasize enough the importance of *method* in the selection of informants. Being adopted into a Native family, learning the language, and various other field techniques, while important, cannot substitute for proper sampling procedure in historically oriented research. One must carefully consider which criteria to use in sample selection, and one must make a determined effort to identify everyone in the resource population who meets those criteria. Only then can one know with whom it is *useful* to establish rapport, and, knowing that, begin to work on doing so.

Informant selection procedures are important in any field research, of course. They are particularly so in retrospective research because literally all of one’s information is acquired from them. A considerable proportion of the researcher’s time therefore has to be spent in identifying individuals who meet the criteria relevant to one’s project. By considerable time I mean that the search may be *the* dominant activity for as long as six to nine months of a twelve-month field study, and it probably should never cease altogether. If the selection criteria have been properly determined, and if the search has been productive, the actual interviewing often can be done effectively in a fairly brief period.

WORKING WITH INFORMANTS

Having located and selected one’s informants there remain the problems of recruiting and interviewing them. Most of the issues appropriately included under those headings fall within the area of field techniques. A few extend to at least some degree into the domain of method: these are communication, recall, and subject matter.

Communication between researcher and informant is obviously a critical problem in reconstructive research since literally all of the former’s results depend on information provided by the latter. One question is, what should be the medium of communication? It is generally agreed that the informant’s language is best, to be preferred over the language of the interviewer, a third language, or an interview conducted through an interpreter. Written questionnaires and other such devices are usually ineffective. Reconstructive studies pose a particular problem because the language of the resource population may—or may *not*—be the same as the language of the study population, depending on the time depth of the study. Even when the former is a direct descendent of the latter, one must keep

alert to possible changes in both forms and usage between the two periods.

A second point about communication is that the use of interpreters, a third language, or some other less than ideal medium of communication does not necessarily mean that the results are less *reliable* than those deriving from interviews conducted exclusively in the informant's language. If the informant is fully fluent in the interviewer's language, or if the services of a highly competent interpreter have been acquired, then one might have considerable confidence in the results as long as other measures of reliability can be applied. Reliance on anything *but* the informant's language usually results in at least some loss of descriptive detail, though, and often in some loss of understanding as well. Whether this is critical or not depends as much on the specific subject matter of the study as it does on the character of the communication between informant and researcher.

A second methodological aspect of working with informants concerns informant recall, although this topic extends directly into the technical area. The basic problem is that most of what a person knows is subconscious. In addition, much of what a person once knew is forgotten, although there is tremendous variation from one individual to another in both respects. The challenge for the ethnographer, particularly where give-and-take interviews are involved, is to get the informant to render explicit what may have been implicit previously and to recall on request things that were long since forgotten, or at least not thought about for some time.

The acquisition of knowledge from an informant, particularly in reconstructive studies, is usefully conceived of as a developmental process. Accordingly, it should last over a number of interview sessions. One begins slowly with relatively superficial but comprehensive topics, and proceeds gradually to both greater time depth and to increasing detail of subject matter. As a result of this experience a good informant begins to think about the past in new ways and perhaps to a heightened degree; gradually his powers of recall may improve.

Recall can be increased sometimes through the use of stimuli of various kinds. For example, after preliminary work in a normal interview context, the investigator might accompany an informant to the location where the events under discussion actually took place. Once there the informant may be able to recall all kinds of details that did not occur to him in the more formal interview setting, particularly if he has not visited the place for some time.

In my own research this approach would have involved tremendous expenditures of time and money because of the distances involved; I simply could not afford to do it. However, I achieved excellent results through the use of topographic maps, by means of which informants could travel vicariously over large tracts of country. A good map greatly improved their powers of recall and also evoked information on a tremendous variety of topics, ranging from settlement location and economics to religion and world view. Although the most productive techniques no doubt vary from society to society, the methodological point still holds: just because an informant *does not* recall something when first asked about it does not mean that he *cannot* recall it under the right set of conditions. The challenge for the researcher is to find out what those conditions are.

The final methodological aspect of working with informants concerns the nature of the topics covered during an interview. If the resource population contains a number of knowledgeable people, and if one's recruiting has been successful, then one's informants will probably be very intelligent and knowledgeable people. But few individuals anywhere, no matter how knowledgeable or intelligent, carry a fully articulated model of their society (past or present) around in their heads. Furthermore, almost no one in any society can simply sit down and spew upon request all of the specific information a researcher wants to have. Instead, informants must be guided to the topics significant to one's research by specific questions posed by the researcher.

Permitting informants to talk about subjects of interest to *them* provides information about the informant, hence about the resource population. It probably will *not* provide much useful information about the study population, yet it is the latter that one wants to have. It is sometimes useful to give informants relatively free rein for one or two sessions because of the information it provides on the informant's perspective, but continuing the process for very long can be very time-consuming and may yield information peripheral to the primary research question. Given time and funding constraints, sooner or later the topics discussed must reflect the interviewer's interests.

The interests of the interviewer are determined by the choice of research problems and the analytic framework. The imposition of these factors on the content of an interview of course biases the results. *Failure* to impose the investigator's choice of topics also prejudices the results of an interview because it merely involves the substitution of the informant's biases for those of the investigator. Since

bias cannot be eliminated from ethnographic research, one might as well attempt to structure the situation in such a way that the results are biased in a manner that contributes the most to the achievement of the research objectives.

THE TEMPORAL DIMENSION

Time is an important factor in any kind of ethnographic research, but in reconstructive studies it is crucial. The general topic is discussed below under two headings: upstreaming and control.

UPSTREAMING

It would appear self-evident that research which proceeds systematically from the known to the unknown is more likely to be accurate than research which proceeds in some other way. Since the reliability of ethnographic evidence tends to decrease with time depth (Buckhout 1974:26; Pitt 1972:28), the obvious method to use in reconstructive research is to begin with the most recent time period for which data are available and to work from there to periods progressively more remote. In ethnohistory this approach is known as "upstreaming," and it seems reasonable to use that term in ethnography as well.

Given the nature of ethnographic research, the phenomena that can be described most reliably are those in existence at the time the research is being conducted. Thus, the systematic ethnographic study of the past is best initiated with a careful investigation of the present.

If it is true that (1) the most reliable data derive from the present, and (2) information becomes progressively less reliable with increasing time depth, then the following conclusion appears inescapable: a reconstructive study is more or less reliable to the extent that events and patterns that existed during the study period can be systematically related to those existing during the research period. The most reliable reconstructive studies are those which are in fact studies of social change, whether or not the study of change *per se* is an objective. This is true regardless of how long before the research period the study period happens to be.

TEMPORAL CONTROL

It is absolutely necessary in reconstructive studies to establish correct chronology. This requirement holds because only through control of the time factor can si-

multaneity be determined and sequence be established. Among peoples to whom chronology is important and among whom an absolute dating system is in general use, it may be possible simply to ask an informant when certain events took place and get a precise and accurate response. In most societies these conditions are not met (Vansina 1970:168).

Chronology is not considered particularly important in many societies; certainly most societies have not had absolute dating schemes in use. But even where the ideal conditions are met, most informants do not recall with precision just when specific events occurred or when certain customs were practiced. However, they often can remember when something happened in relation to something else. Consequently, particularly at the outset, the investigator is advised to place primary emphasis on some sort of relative dating procedure. At the same time, one should attempt to assign absolute dates whenever possible.

One useful way to begin to establish a chronology is with personal data about informants, who can often relate an event or development to a particular stage in their own lives. Women are particularly valuable in this regard. Certain "life crisis" events for females are biologically controlled and subject to very narrow variation from one population to another. Menarche and the births of the first and last offspring, for example, can often be assigned a rather precise relative date, and they are often events that are vividly recalled. Given assumptions or facts about the age of the informant and knowledge of the timing and duration of periods in a life cycle in the society concerned, the investigator can significantly narrow the range of time in which important events probably occurred. Systematic use of several informants of different ages to date the same event or sequence of events can narrow the range even more.

When employed carefully, life history data alone can sometimes reduce the margin of error to five years or less, even at a time remove of seventy-five to a hundred years. On the basis of knowledge of his own life history and that of his mother, one of my North Alaskan informants, who knew his approximate year of birth and his place in the birth order of siblings, calculated in 1970 that a particular event had taken place "around 1899," eleven years before he was born. An historical source subsequently confirmed that it had taken place *exactly* in 1899.

Genealogical data are also useful in the development of a chronology, particularly when used in conjunction with life history data on the people included in it. When

collecting genealogies for this purpose it is especially important to try to establish the correct birth order of siblings, since an informant can frequently date events with reference to the birth of specific individuals. When dealing with greater spans of time, phenomena can often be associated with particular generations even when more precise dating is impossible. In societies where the collection and retention of genealogical knowledge is considered important it may be possible to establish a broad chronology for several centuries in this way. On the other hand, in societies where it is taboo to utter personal names, or where genealogical data are jealously guarded for political or religious reasons, this important means of establishing a chronology may be unavailable.

The third type of information that is useful in the establishment of a chronology consists of "watersheds" in the history of the society or region concerned. A "watershed" for this purpose can be any event or development that is of brief duration, yet significant enough to be clearly recalled by many people. Common examples of watersheds are natural disasters (floods, earthquakes, famines), wars, the beginning or the end of the reign of a particular ruler, the arrival of a particular explorer, the establishment of the first mission, and the like. Particularly valuable are watersheds that can be independently dated by means of external evidence, either historical or archaeological.

It is worth reiterating at this point that any careful dating scheme will begin with the present, i.e., with the time when the investigation is actually carried out. By tying a chronology directly to the present, one can begin with a precisely dated body of information that can be related to the progressively more obscure past through references to life histories, genealogies, and watersheds. The only exceptions to this generalization would be in cases where there are unusually complete ethnohistoric sources concerning the people or region concerned, in which case it may be possible to begin with the period dealt with in those sources.

In my own research, I established chronologies using a combination of life history, genealogical and watershed data. Neither the North Alaskan nor the Caribou Inuit are particularly concerned about absolute dates, but they are extremely interested in and informed about the relative ages of individuals, i.e., in the sequence of people. They are also pretty good at relating events and people to one another. Because of this juxtaposition of interests, I was able to establish fairly detailed sequences in both areas back as far as about 1890. I was greatly aided by the fact that

the approximate birth dates of all of my informants were known (thanks to government administrators) and that both areas were liberally supplied with watershed events whose dates could be independently established. In the Caribou Inuit area 1890 was about as far back as I could go with my informants. In North Alaska, however, the best informants were able to relate specific events and developments to particular stages in their parents' lives, and occasionally even to stages in their grandparents' lives. In some cases, this information, combined with genealogical data which connected individuals mentioned in narratives to people in the resource population, enabled me to date a few events to nearly two centuries before my research was carried out.

Detail, as well as factual accuracy, normally decreases with time depth (Vansina 1970:172). Consequently, the further back in time the study period happens to be, the greater the need to rely on analysis in terms of periods rather than specific dates. A "period" for purposes of ethnographic reconstruction is any unit of time in which events may be considered to have taken place *more or less* simultaneously, or in which relatively little change took place. Because of the loss of information with time depth, the further back the study period happens to be, the greater the advisability of including greater lengths of time within a given period. For example, working back through time from the present, one might begin with seasons, progress to years, then decades, and finally to generations. By this means one can preserve accuracy even when losing detail.

By working from the present back through time one should be able to obtain accurate information on *trends*. Once one has information on trends, one can extrapolate backward or forward through time into periods for which one is unable to collect much information. Given knowledge of any two stages in a temporal sequence, it is possible to establish at least a range of possibilities about the state of affairs in the third (Levy 1952:75). Of course one should not confuse a range of possibilities with a specific case, nor a hypothetical state of affairs with the actual one. But, if one proceeds carefully, the least one can do is formulate hypotheses about the situation at any given point in time and use those hypotheses to focus and guide one's research.

Perhaps even more important, one can formulate ethnographically based hypotheses about conditions at a certain time period and then test those hypotheses against data obtained through ethnohistorical and/or archaeological techniques. Sometimes even a very modest amount of

ethnohistorical or archaeological data can be made maximally useful in this way. Conversely, otherwise unverifiable ethnographic data can be evaluated by checking them against even a small quantity of ethnohistorical or archaeological information.

One final point with reference to time: it is important to extend one's investigation *beyond* the period in which one is specifically interested. By so doing one is able to bracket the study period in time, and hence control chronology at least to the extent of ascertaining whether events occurred before, during, or after the period of primary interest. Failure to follow this procedure can result in the confusion of the study period with one or more periods that preceded it. Such an approach leads easily to error, and often to naive notions about some kind of world in the past in which nothing ever changed (Rowe 1955; see also Sturtevant 1968:466).

THE SPATIAL DIMENSION

The determination of where events took place is just as important as when they occurred, and it is often just as difficult to discover. The diffusion of some practices, the loss or invention of others, and population movements of every kind can produce significant changes in the distribution of both people and customs over time (Sturtevant 1968:467). For example, the Cheyenne horse-borne hunters who inhabited the northwestern Great Plains in the early nineteenth century were direct descendents of pedestrian woodland people who had lived several hundred miles farther east only two hundred years previously (Hoebel 1960:1). Any twentieth-century attempt to reconstruct their way of life during the latter period on the basis of their distribution in the former would have been completely untenable. Of course this particular movement is well known because of earlier research, but in many parts of the world information of this kind is not available. In order to guard against serious error on this score it is advisable to proceed on the premise that the spatial relationship between populations or customs at two different points in time and space must be demonstrated; it can never be assumed.

The easiest way to begin to establish spatial control is simply to ask informants to locate events in space as well as in time. The procedure is easy enough in theory, but it is one that can be overlooked if the underlying problem has not been recognized. It is rarely easy in practice. In order to establish location effectively the investigator may have to have on hand detailed maps of large tracts

of country, and he may have to spend long hours poring over them with informants in order to locate the places being discussed.

Another way to establish spatial control—one that should be used in conjunction with the first—is to expand the geographic area covered by one's research. With respect to the research population this means seeking informants from a wider area than might appear necessary; with respect to the study period it may require at least the partial reconstruction of several populations in addition to the one of primary concern.

The ideal procedure is to “bracket” geographically both the resource and the study populations, and this is the primary methodological point. Bracketing the resource population involves expanding the search for qualified informants outward in space until one has passed well *beyond* the area where they can be found. With respect to the study population the approach requires that one attempt to reconstruct the location of both the population of primary concern *and* of its neighbors. Information from both inside and outside the study population makes it possible to define its boundaries with relative precision, and it is well worth the effort. In addition one can make a stronger case for the reconstructed location of the study population if one can show precisely what other populations were living in adjacent areas as well.

With respect to spatial control it makes a big difference whether one is attempting to reconstruct an earlier stage in the history of a specific population or society, on the one hand, or the general situation that existed previously in a particular *geographic area*, on the other. If one is interested in reconstructing the late eighteenth-century Cheyenne way of life, for example, one could probably do so by recruiting informants in each of the reservations where Cheyenne are currently to be found. If, on the other hand, one is interested in reconstructing the late eighteenth-century situation of a particular portion of the Great Plains, one would have to interview descendents of every population whose ancestors might have occupied or visited the area during the study period. The latter would be a much more difficult task than the former, both intellectually and physically.

In my own research I was interested in reconstructing earlier situations in both northern Alaska and the central Canadian Subarctic. Previous investigators had established a general relationship between the research and the study populations in both areas, but the details were by no means clear. In North Alaska I visited eleven out of

the twenty-five villages in the area. Although I did not visit all parts of the study area, I was able to interview people from all sections. This proved to be fortunate because several now-abandoned regions turned out to have been inhabited during the study period, while others that are now occupied were not. An additional discovery was that, although the best informants on some districts were sometimes found living in those districts, the best sources on others were found tens and even hundreds of kilometers away from them. There was no way to anticipate just where the best informants on a particular area would be found. In the central Canadian Subarctic three out of the five villages in the area were visited, with similar results.

There remained, however, the problem of bracketing the two study areas by visiting the districts outside but adjacent to them. My methodology called for complete coverage of the surrounding areas, but limitations of time and funding prevented me from meeting that requirement. Fortunately I was able to utilize a combination of historical sources and correspondence with people living in the relevant areas to fill in some of the gaps in field coverage, but even these were deficient in many respects. However, I learned enough to know that, had I not had access to this information, I would have made a number of major errors in my analysis. This disconcerting experience has greatly strengthened my belief in the value of bracketing both the resource and the study populations as a method of spatial control.

SUBJECT MATTER

In principle there is no limit to the variety of subjects that can be investigated by means of ethnographic reconstruction. There may, of course, be limitations imposed by the age or sex of the informants or the researcher, by the loss of certain kinds of information, or by cultural limitations on discussing certain topics. This is true of all ethnographic research, reconstructive or otherwise. However, there is one methodological point to be made here, and that is that one should attempt to reconstruct as much of the system as possible, regardless of the researcher's particular interests. In other words, the principle of bracketing should be applied to the subject matter of one's investigation just as to the temporal and spatial foci.

Subject matter bracketing is advisable for two reasons, both of which relate to tests of reliability. The first is that the more comprehensive the subject matter that is to be covered by the work, the greater the opportunity the re-

sults will offer for consistency tests. The second is that the more comprehensive the subject matter, the greater the likelihood that at least some topic will be covered that might be checked against an independent source of information, either another investigator or an ethnohistoric or archaeological source. Since those are the only tests of reliability available in ethnographic reconstruction, the significance of subject matter breadth may be understood to be of considerable importance to the overall outcome.

BIAS CONTROL

Bias cannot be eliminated from ethnographic research conducted by normal human beings. Bias can, however, be recognized and described, and therefore controlled to at least some degree. In general, ethnographic research is more or less reliable to the extent that bias is explicitly indicated by the investigator and/or can be established independently through an examination of the researcher's field notes and published reports.

There are several areas in which bias enters ethnographic research. The investigator's cultural and personal backgrounds and research interests are important sources of bias. In addition, there are the cultural and personal biases of the informants, both individually and collectively. The recognition and description of all these biases requires considerable time and effort, and complete control is impossible. Nevertheless there are several steps one can take to establish at least some measure of bias control, and the greater one's efforts in this direction are, the more reliable one's results are likely to be.

Investigator bias is created through the choice of certain problems for investigation, the selection of specific techniques for studying those problems, the general theoretical approach being used, the general development of the field at the time, and the cultural milieu in which the researcher has grown up (Pitt 1972:47, 52, 56; Sturtevant 1968:461-462; Vansina 1970:172). Whether *any* researcher can really analyze his or her own biases is debatable, particularly when actually conducting the research in question. However, one can at least try to be explicit about one's choice of problems, techniques used, etc., so that others can make judgments on this score at a later date (Sturtevant 1968:460-461).

Information on investigator bias often makes for dull reading, and editors have a tendency to excise all but the briefest summaries of the topic. But relevant data are invariably contained in field notes, grant applications, corre-

spondence, and unpublished manuscripts. These materials can be made available to specialists, or through donations or requests to archives for subsequent examination and assessment. As Sturtevant (1968:461) has pointed out, field work involves the production of primary ethnographic documents and “the author should feel an obligation to assist those who will apply the historian’s canons of criticism of sources” to them. I might add that, in another generation or so, the raw field notes of the last century will constitute *the* primary sources on a large number of societies. Unless we take steps to ensure their availability to future generations of researchers an invaluable body of information will be lost.

Informant bias often can be established more precisely than investigator bias—at least by the investigator himself. The method for doing so is analogous to the one used by historians to establish the authenticity of a document (Pitt 1972:47). Among the areas of assessment are the following: (1) the learning context, (2) the interview context, (3) informant accuracy, (4) attitudes toward the subject matter of the research, and (5) the tendency of informants to emphasize ideal over actual patterns.

“Learning context” refers to the situation in which the informant acquired the information that he subsequently imparts to the ethnographer. Did the informant personally see (participate in, belong to, experience) the phenomena he is describing? If so, what was his role in the proceedings, how old was he at the time, and what were his attitudes toward what was happening? If the informant was not directly involved, on the other hand, one must find out who his source was, assess *that* source’s biases, and ascertain the context in which the informant acquired the information. All of these factors will color a person’s perception of social phenomena. Collection of the information listed will enable the investigator to make a number of judgments about the biases involved.

Another area of analysis in the control of informant bias is the interview context. In this connection a number of questions can be asked. These include: (1) the nature of the audience, if any; (2) whether or not the interview took place in strange (familiar, threatening) surroundings; (3) the extent of the informant’s experience in interview situations; (4) the extent to which interpreters were involved in the interview and the nature of their participation; (5) whether the informant was cooperating voluntarily or under duress; and (6) the informant’s physical and emotional state during the interview. Most of these factors are reasonably apparent to an experienced inter-

viewer whether or not he is specifically looking for them, but they must be recorded in order to be of subsequent value. A seventh is less apparent, this being the specific set of questions posed by the interviewer. This factor is at least as important as the others, however, since specific questions frequently encourage particular responses (Buckhout 1974:27; Loftus 1974), hence they significantly bias the outcome. In ethnographic research it is probably impossible to ask a truly unbiased set of questions. The best way to deal with this problem is to record the questions as well as the responses.

Accuracy is the third area to pay attention to when assessing informant bias. To a significant extent accuracy can be determined only with reference to other sources, but certain internal factors are amenable to evaluation. For example, what is the informant’s general reputation for accuracy—given local standards for determining same? Is he giving answers he thinks you want to hear, or is he trying to present an accurate picture regardless of your opinion? Is the information provided by this informant internally consistent (without regard for its relationship to information acquired from other sources)? Perhaps most important of all, how aware is the informant of the limitations in his knowledge, and how candid is he in admitting them to you? Finally, it should be noted that accuracy should be distinguished from honesty, since even honest and well-intentioned informants can provide erroneous information (Buckhout 1974).

The fourth area to consider is the attitude of the informant toward the subject matter of the research. How does the informant feel about what he is describing to you—ashamed? proud? indifferent? Has he a vested interest in reporting things in a certain way? Is he aware of his biases, and can he characterize them for you? All of these considerations affect the information that will be acquired, but all are capable of description and control to at least some extent.

Finally, to what extent does an informant emphasize ideal patterns over actual ones? What people think they *should* do and what they do in fact are by no means the same, and failure to differentiate between the two can seriously distort the outcome of a reconstructive study (Lee and DeVore 1968:148). For example, it was reported for years that the average Chinese family was very large in traditional times, while in fact it tended to be extremely small except among the gentry (Hsu 1943). This was a simple case of confusion of the ideal state of affairs for the actual one, yet the difference between the two was great

enough to distort the perceptions of several generations of scholars about one of the most thoroughly studied societies in world history. Finally, it is important to keep the actual/ideal problem separated from that of accuracy, since an informant can present an accurate account of the *ideal* system to an investigator who thinks he is collecting data on the actual state of affairs.

BIAS DIVERSIFICATION

An extremely useful way to control both informant and researcher bias is through what has been called “bias diversification” (LeVine 1966) or “triangulation” (Cook and Campbell 1979). This involves the use of sources or techniques having different biases, ideally ones that offset one another. Reliability increases to the extent that the sources or techniques having different biases produce similar results.

Bias diversification does not necessarily result from a simple multiplication of sources, although increasing the number of sources may be a start in the right direction (Hickerson 1970:121). But if, for example, one has time for in-depth interviews with only six of twelve qualified informants, including both men and women, the results will be more reliable if there are three women and three men in the sample than if the six are chosen on a completely random basis. Other types of bias that one can control effectively are age, place of origin, wealth, family (clan, association, etc.) membership, education, religious affiliation, occupation, and social status; the list is theoretically endless.

Just as there is no way to determine just how many sources constitute an adequate number (Pitt 1972:54), there is no way to know just where to stop in the diversification of bias. There probably are no ultimate limits in either case since complete knowledge will always remain an elusive goal. But one can make an effort to determine the biases of the members of the resource population and then include a diversity of same in the people recruited to be informants. All other things being equal, a study in which bias has been systematically diversified will probably be more accurate than one in which it has not.

TESTS OF RELIABILITY

Having gone through the effort of locating and recruiting informants and of obtaining a more or less complete body of data from them, there remains the crucial problem of

reliability. Just how much confidence can one have in a description of phenomena that one has not personally seen or experienced? Unfortunately, no objective measure exists to assess the reliability of reconstructive studies in social science. Even research replication, so essential in most of the physical sciences, is rarely possible in this area, and it is often severely biased even when feasible.

Despite the many problems inherent in ethnographic reconstruction, the reliability of retrospective studies is amenable to at least qualitative assessment. These are the tests of consistency and corroboration.

TESTS OF CONSISTENCY

Tests of consistency are extremely important in assessing reliability in reconstructive studies, and they are the only tests that can be applied if one's field data are the only data available on the study population (Lee and DeVore 1968:5–6). The basic assumption is that reliability increases to the extent that the various elements in the database are consistent with one another.

Tests of consistency are simple in concept although often demanding in practice. The first test is that of assessing the *internal consistency* of the information provided by each informant. Then one checks the information obtained from each informant against that provided by every other informant; this is the test of *mutual consistency*. To the extent that all of the bits of information fit together in a coherent picture, the more reliable the result is assumed to be.

One can take a number of steps to increase the value of tests of consistency, although they all involve the multiplication and diversification of items that must be assessed in this respect. In other words, all the steps involve bracketing. For example, with respect to the information provided by a single informant, one can maximize the *variety* of facts which must be consistent with one another by attempting to reconstruct as much of the system and its setting as possible. Secondly, one can maximize the number and especially the diversity of informants whose accounts must be consistent, both internally and with one another. Third, one can broaden the spatial coverage of one's investigation so that the reconstruction of the study population is consistent with the reconstruction of both neighboring populations and the nonhuman environment. Finally, one can increase the temporal coverage of the reconstruction to periods before and after the one of primary interest. The recon-

struction must be consistent in space, through time, and over a comprehensive set of subjects.

Obviously there is no limit to the process of increasing and diversifying information, reconstructive or otherwise. All that can be said is that the further one proceeds in this direction, the more reliable one's results are likely to be.

CORROBORATION

Corroboration of one's findings by a completely independent researcher is the second means of determining reliability in ethnographic reconstruction. The "independent" investigator may be another ethnographer (who acquired similar information from different informants), an archaeologist, or a contemporary observer who produced what constitutes an ethnohistoric source in this particular context. Consistent with the method of bias diversification, I suggest that confirmation by an entirely different *type* of data—i.e., either archaeological or ethnohistorical, or, ideally, both—produces more reliable results than confirmation by independently collected ethnographic data (cf. Pitt 1972:54; Sturtevant 1968:476).

The independence of field investigators may be a problem since few conduct field research without first familiarizing themselves with other ethnographic, historical, and archaeological research on the area. Could it not be that similar results are the result of biases unconsciously acquired while reading this material? The answer must be in the affirmative: familiarity with previous results will inevitably result in biased questions, and biased questions invariably prejudice the answers (Buckhout 1974:27).

The other side of the coin of independence is the problem of "passing by" (McCall 1964:146–148; Vansina 1970:170). Passing by is what happens when there is so little overlap between two sets of data that they cannot be used to cross-check each other. By maintaining complete independence from other investigators' research, one runs the risk of not collecting *any* data amenable to independent confirmation by other sources.

The combined problems of independence and passing by pose a dilemma. To preserve independence one should not read anything at all, yet to keep from bypassing the work of others one should read a great deal about the group concerned. There is no obvious way to resolve this problem. In my own research it was handled by default. Before I began my field research I was familiar with the general anthropological literature on the areas concerned, but quite ignorant of the contents of most of the ethno-

historical sources. However, I was unaware of the extent of my ignorance at the time because I had seriously underestimated the extent of both the published and particularly the unpublished sources on each of the two study populations. As a consequence, my field research filled some major gaps in the anthropological literature yet it maintained enough overlap with sources then unfamiliar to me to make possible independent confirmation of many of my results.

On the basis of my experience, I feel that the most effective way to cope with the conflicting demands of independence and passing by is through the use of a middle-of-the-road type of approach. Prior to the field work one should familiarize oneself with the major *topics* that have been dealt with by previous investigators, and ascertain their general findings in each area. One should not, however, achieve command of the details at this point. This approach enables the ethnographer deliberately to collect enough material for partial confirmation without completely sacrificing independence. The more advanced the state of knowledge about a particular area happens to be, however, the less feasible this approach will be. At the other extreme, when one is working in a virgin area, one should emphasize the collection and presentation of data which can be confirmed or refuted by subsequent research (Vansina 1970:167).

A final problem with independent confirmation concerns the priority of evidence from different fields. What if the ethnohistorical data contradict the ethnographic findings? Which should receive priority in reconstructive studies—ethnographic, ethnohistorical, or archaeological data? Herskovits (1959:230; see also Vansina 1970:167) has flatly stated that ethnohistorical data are intrinsically more reliable than ethnographic data. A more balanced approach has been recommended by Vansina (1970:168), however. He argued that no *a priori* statement can be made with respect to the relative value of data from different fields. In his view, *all* the evidence that can be brought to bear on an issue should be. The reliability of all of the data from every source must be assessed separately, then evaluated in the light of the emerging synthesis.

My own experience supports Vansina's position on the futility of making *a priori* statements about the relative value of ethnohistorical and ethnographic data, and, by extension, archaeological data as well. For example, in the Caribou Inuit area, Albert P. Low (1906:135) claimed that the Sauniquirmiut tribe was located on the Dubawnt River. My informants, however, indicated that their territory was

at least 300 km east of the Dubawnt. Who was to be believed, a few old people interviewed seventy years after the study period, or a reputable, scientifically trained observer who was in the region at the time? Fortunately, my informants' statements were corroborated by several other ethnohistorical sources, so I never really had a problem in coming to a decision. But if the other sources had not been available, I might have had a very difficult time convincing other anthropologists that the ethnographic findings were more reliable than the ethnohistorical ones.

In North Alaska I had exactly the opposite experience. Over the course of interviews carried out during the fall and winter of 1969–1970, I had been able to determine the major social units and boundaries that had existed along the coast between Bering Strait and Point Barrow, but I had not been able to discover the names of two of the major social units. Knowledgeable informants interviewed on the topic stated flatly that the units in question had never had any names. However, a few years later, I discovered that a Russian expedition, which included a multilingual interpreter, had explored the pertinent section of the coast in 1838 (VanStone 1977). The journal of that expedition reported precisely the same social units and boundaries that I had been able to reconstruct on the basis of informant data in 1970, but it also included the missing names of the two social units. My informants were wrong in this case. The societies had had names, but they had been forgotten during the rapid social change and demographic dislocations that occurred during more than 120 years between the research and the study periods.

CONCLUDING REMARKS

The method of ethnographic reconstruction outlined here has a number of important elements. First, of course, there is the very careful selection, recruitment, and interviewing of informants. In this respect, reconstructive research is indistinguishable from any other ethnographic study, although greater care may be needed here because observation and participation cannot be used to supplement data provided by informants. The second element is what I call the “expanding horizons approach.” This involves working from the known to the unknown, in time, space, and subject matter. The third element is bracketing—going beyond what one is specifically interested in, in time, in space, and in subject matter. This enables one to establish trends in both time and space, knowledge of which can be extremely useful in understanding what happened at par-

ticular points in time and/or space. Bias control, accomplished primarily by means of bias diversification, is next, followed by the application of tests of reliability. If applied systematically, this approach will enable the investigator to reconstruct social and demographic patterns as far back in time as the fund of historical knowledge held by members of the resource population will permit. This conclusion is every bit as justified in the case of hunting and gathering peoples such as the Inuit as it is in chiefdoms or states, such as those in Africa and Polynesia.

As a final point, it is worth noting that ethnographic reconstruction requires a great deal of time and effort, much more than ordinary field research does. This is because, in most cases, a comprehensive study of the present is a prerequisite to a systematic reconstruction of the past. The standard “year” of field work is just the beginning. But it is worth doing because it is very often the most informative way to learn about the past of a great many of the world's peoples.

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