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A little more than six years ago, over dinner during an annual meeting of the then new Society for the Study of Culture and Psychiatry, I had an opportunity to discuss with Dr. Edward Foulks the series of events that led to the controversy surrounding the Barrow Alcohol Study. His surprise at the nature and degree of criticism that emanated from so many different sources was still evident. I remember other reactions as well - - confusion, frustration, embarrassment, and some anger. Most of us would have felt, indeed some have felt such emotions when facing similar circumstances. I was deeply impressed by Dr. Foulks' sincerity, intentions, and desire to understand what had transpired. He probably also had a need to bring closure to this chapter of his professional work. We talked late into the night, debating a number of the issues raised by the contributors to this special issue. At the close of our discussion, I suggested that the subject deserved a fuller airing, perhaps in a forum like *Current Anthropology's* "Star Commentary", which is characterized by a principal article, followed by critiques from the field, and closed with a rejoinder. Four years later, as we were about to establish *American Indian and Alaska Native Mental Health Research*, I asked Dr. Foulks if he would consider preparing such a manuscript for publication. Indeed, as it turned out, he had already begun this effort, describing the events in question and analyzing the resulting conflict as a consequence of the misalliance of models among the central participants: hence, the title of his article.

Dr. Foulks has written a remarkable piece. One might have expected a great deal more defensiveness, even bitter attack of his original detractors. Instead, Dr. Foulks provides us with a sensitive and detailed account of his view of this controversy. Without dodging responsibility for some of the problems that ensued, he analyzes the factors that contributed to them in a penetrating fashion. Many of the leading scholars in the Indian/Native mental health field have reacted to his manuscript, offering additional perspectives on the Barrow Alcohol Study, with special emphasis on its ethical and procedural aspects. The result is a fascinating polemic devoid of the diatribe that usually accompanies such debates.

As several of the contributors note, some of the people who were involved in or lived through this ordeal prefer that it not be exhumed. However, a decade has passed and a new generation of young scholars is gradually emerging, many of them Indians and Natives. Birthright and heritage are limited insurance against the commission of similar errors,

particularly when one is pressured to succeed in the academic world at the possible expense of that which is valued among kith and kin. Hopefully, this issue of the journal can serve as a valuable reminder of these tensions and can encourage us to continue to struggle with the accommodations that both demand.

Spero M. Manson, Ph.D.
Editor-in-Chief

MISALLIANCES IN THE BARROW ALCOHOL STUDY

EDWARD F. FOULKS, M.D.

ABSTRACT. *The Center for Research on the Acts of Man conducted a survey of the use of alcohol among the Inupiat of Barrow, Alaska, in 1979. The study resulted in grievances among many individuals and institutions associated with the community. In a retrospective analysis of the factors contributing to these misalliances, the author raises important ethical and procedural questions to be considered carefully in future projects of this nature.*

In 1979, the author was a member of a research team that conducted a survey in the Inupiat community of Barrow, Alaska. The results of the survey generated considerable reaction on the part of the scientific community, the popular press, and the local population. During the past decade, we have re-examined the circumstances which contributed to the initiation of the study and to its subsequent reception. Our concern has focused on an attempt to understand the conflicting social and ethical factors involved in the process of conducting research on topics as sensitive as alcohol abuse and ethnic relations. The purpose of this report is to examine in detail the political and ethical dilemmas which we encountered. We will argue that difficulties might have been avoided had we obtained better insight into the community's beliefs regarding the nature of "the problem," and had we been better able to ensure more total community participation in deciding how the results of the study were to be used.

Summary of Methods and Results

A complete report of the research can be found in a book and several scientific articles (Foulks, 1987; Klausner & Foulks, 1982). To summarize, the research team used a 1972 demographic survey of the community of Barrow, Alaska (Katz, 1972), and drew a 10% representative sample of everyone over the age of 15. We interviewed a total of 88 Inupiat regarding attitudes and values about the use of alcohol, and obtained their psychological histories, including drinking behavior. Each subject was also given the Michigan Alcoholism Screening Test (Seltzer, 1971) and was asked to draw a picture of a person.

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The tabulated results of our survey indicated that 41% of the population considered themselves to be excessive drinkers, and 60% felt badly about the consequences of their drinking. More than 50% reported that drinking ultimately created severe problems with family and spouse. Sixty-two percent regularly got into fights when they drank, and 67% experienced frequent blackouts or amnesia from the episodes. Drinking in this community was described primarily as a social event; only five individuals in our sample reported drinking alone.

Our survey indicated that about 40% of the population were members of the culture-broker or acculturated sector, holding full-time salaried jobs and regularly attending church; the remaining 60% were more traditional in occupation and in their connections to outside social networks. Individuals from the culture-broker class used alcohol in much the same manner as Alaska non-Natives. One quarter of them were abstainers. Although those that drank did so as frequently and heavily as those in the traditional sector, their behavior tended not to endanger life or to precipitate detention in the Community's Acute Alcohol Detoxification Center.

Comparing the social characteristics of individuals detained at least once with those never detained, we found that members of the culture-broker sector were less likely to be detained than traditional Inupiat, despite little difference in the level of alcohol consumption and in other alcohol problems. Eleven percent of the salaried workers, 50% of the blue-collar workers, and 56% of the unemployed and housewives ($\chi^2 = 9.3$, $p < .5$) reported having been detained. The part-time blue-collar worker/traditional part-time hunter was significantly more likely to be detained than were members of the culture-broker class.

Church membership significantly affected Inupiat social interactions, as well as drinking behavior. The churches in North Alaska have a clear position on alcohol use. Thirty-three percent of the population were regular attenders of the Presbyterian Church, which strongly discouraged drinking, and 3% were attenders of the Assembly of God Church, for which total abstinence was a requirement for membership. Sixty-four percent were not church attenders. The minister of the Presbyterian Church identified the people who were active in the church and those who were not. Half of the identified membership was rated as active by the minister and only 29% of those were heavy drinkers, compared to 47% of the nominal members and 48% of those not affiliated with churches.

A similar pattern emerged with respect to detentions in the Community's Acute Alcohol Detoxification Center. Of the active church members, 5% had been detained, whereas 53% of the nominal members and 50% of the nonaffiliated had been detained ($\chi^2 = 11.3$, $p < .05$; the nominal and the nonaffiliated did not differ in their level of drinking problems or in the likelihood of their being detained). The role of the church in this phenomenon may have extended beyond their provision of explicit sanctions regarding drinking; the churches may have prepared members for adapting to the

social hierarchies and bureaucracies now found in their changed society. Church members held positions of leadership in their community, thus reflecting their culture-broker status.

Intermarriage of Inupiat with non-Inupiat historically appears further to have facilitated entrance into the culture-broker sector, since it often necessitated contact with outside bureaucratic organization. According to genealogical data, 51% of the Inupiat were of mixed ancestry. Of these, 33% had been detained for alcohol-related problems, in contrast to 50% of those of pure Inupiat extraction.

Classification by surname also identified a difference in the likelihood of being detained. Unlike a literal measure of ancestry, surnames seemed to have become a focus of social attitudes and social orientations. Those bearing European surnames were more likely to be considered by others in this community as belonging to families of mixed ancestry. They more easily identified with one another and developed a social network. Thirty-eight percent of the population had European rather than Inupiat surnames. Twenty-seven percent of them had been detained, contrasting with 51% of those with Inupiat surnames ($\chi^2 = 3.8$, $p < .05$).

Psychological indicators of ethnic identity were identified in our sample's human figure drawings. Drawings obtained from our subjects were blind-sorted as Inupiat or non-Inupiat, according to the style of clothing and posture of the figure. Seventy-eight percent of the sample drew Inupiat-style figures. Thirty-eight percent of these had been detained, compared with 13% of those did not draw Inupiat-style figures. The difference was not statistically significant, but it suggested a pattern similar to that found for the previous indicators of social differentiation. To the extent that drawings indicated identification, the culture-broker class were—once again—more immune to detention.

Inception of the Study

The impetus for the study originated from the North Slope Borough, which offered a contract to Intersect, a consulting firm in Seattle directed by a former minister of the Presbyterian Church in Barrow. He decided that Intersect would subcontract to the Center for Research on the Acts of Man in Philadelphia to study some of the causes of alcohol abuse and to evaluate the Community's detention program for acute alcohol detoxification. The Director of Intersect established a permanent field office in Barrow, headed by a hospital administrator who was to coordinate the study. The Research Center's team was composed of a cultural psychiatrist, a sociologist, and a health planner consultant from the Kennedy School, Harvard University.

An initial planning meeting was held at the Department of Public Safety in Barrow during the month of April, 1979. The Director of Public Safety was extremely concerned about the traumatic deaths associated with alcohol abuse in the community. He emphasized that he was interested in receiving

research results which might enable him to establish more effective programs to reduce morbidity and mortality. He wanted to know what was behind the behaviors which resulted in detention for acute alcohol detoxification.

A division of labor between the Research Center and Intersect was established at these early meetings. The research was to focus on alcohol use, detention, and nature of social change in Barrow. Intersect would establish the organization that would translate this report and its recommendations into action. These mechanisms included a Steering Committee consisting of local Inupiat leaders, and a Technical Advisory Group (constituted primarily of non-Native professionals who represented various agencies) which provided professional advice for Barrow and the North Slope Borough. The subsequent contract negotiated between Intersect and the Research Center stated:

The Center would analyze social and cultural relations of the population of Barrow, Alaska, in light of the wider societal and economic and governmental organizations of the North Slope Region. Particular attention would be given to the distribution and consumption and abuse of alcohol in and around this community... their work would be based on a review of published and unpublished materials, informal interviewing, and a field study in Barrow of approximately two months.

The Director of Intersect asked for a preliminary research report by September, 1979. This report was to include an analysis of the nature of the alcohol problems and recommendations for remedial public policies. He requested that this and a later larger version of the research be made public in the near future.

Research work began in July of 1979. All basic data were gathered during the summer, but the examination of public safety, court, hospital, administrative, and financial reports, and the study of the research of other scholars both preceded and followed the summer in the field. In fact, some of the census data, vital statistics, and financial audit information were collected nearly two years after the initial field study.

Reporting the Study: What Went Wrong

A first draft report of the research, entitled "Alcohol and the Future of Ukpiagvik" (Klausner & Foulks, 1979), was sent to Intersect in late September and was shown to some members of the Barrow Technical Advisory Group. Members of the Technical Advisory Group completed their work on the report in late October, and requested that a public meeting scheduled for two weeks later be postponed and that the research team return to Barrow to discuss the report.

Shortly thereafter, the Research Center received a communication from the Director of the Department of Health. The Department had been cooperative during the field research and had arranged for researchers to meet many Inupiat technical leaders of the community. However, the Director found the report difficult to read, ambiguous, and verbose, and asked for a more "precise and authoritative document." She and other non-Native professionals of the Technical Advisory Group felt that the research report had imposed Western, "lower-48" standards on Inupiat society, and should be rewritten to better reflect the attitudes and values of the community. They objected to what they regarded to be the biomedical bases of the research findings.

Competing Models: The Problem

It was difficult to identify a consensus of community attitudes and values toward alcohol use/abuse. Rather, ambivalence and an inherent conflict of values regarding alcohol had emerged in this dialogue. On the one hand, there was ample testimony to the hardships experienced by families where alcohol was a problem. On the other, drinking was often viewed as an end in itself, and the behavior of the drinker was excused because he was intoxicated and not in his "right mind." We were also aware that while the problems were widespread in the community, reinforcing the unfortunate stereotypes of the American Indian and Alaska Native and alcohol must somehow be avoided.

On November 5, 1979, the authors once again met with the Barrow Technical Advisory Group, who stated their concern that only Natives were studied, and that outsiders in town had not been included. They further expressed their fear that mentioning ancestry would be a controversial subject. A public meeting was planned and scheduled for the following January. The Barrow Technical Advisory Group asked that the media be used to advantage, that professionals in communications be used to prepare for and promote the meeting, and that the meeting be broadcast on television so that people not present could telephone in their questions. All of the Technical Advisory Group members volunteered to serve on the subcommittees. They looked forward to the Inupiat responses to the report that would emerge from the Steering Committee meeting the following day.

The research team then met with the Steering Committee, composed of leaders from the Inupiat community. Using slides and graphs, the research team spelled out the problems of what the recent research revealed regarding alcohol use in Barrow. For those members who did not speak English well, the presentation was translated into Inupiat. The Steering Committee also endorsed the idea of a town meeting. Their model of alcohol abuse was linked to witnessing culture change; they viewed both the problem of alcohol, and help with the solutions, as part of the new ways of life imposed on them from outside their community.

During the meeting, one leader stated that there was too much pressure for one generation to bear as the culture turned suddenly from a traditional culture to a modern cash culture. He said that problems with alcohol began by the end of World War II. He could not understand why the state and federal governments could not help Barrow stay dry when the people voted for it. Laws against selling liquor must be enforced, he said, stating that maybe new laws were needed.

During the Steering Committee meeting, the authors observed that the Inupiat leaders present were generally from the older generation who had fought over land claims, established a regional corporation, and achieved political self-government for North Slope Borough. They were knowledgeable about political give and take with governments at the state and federal levels. The younger group of leaders in the community, however, were for the most part absent from the Steering Committee meeting. This younger group came to maturity after the resolution of these issues and were proud of their self-reliance. In retrospect, if the research team had learned more about the attitudes of this younger segment of the population, the pitfalls that were to follow might have been avoided.

Following the meetings in Barrow, Intersect proposed that a practical report be prepared. The Research Center was to provide a specific client report simply summarizing the alcohol information and making policy recommendations. The official report, on the other hand, was to be a technical scientific document, dwelling on the more fundamental issue of social change in light of the increased economic activity in the community. The Summary Report was submitted several weeks later (Klausner & Foulks, 1980).

Systems in Conflict

Soon thereafter, a non-Native faculty member of the Inupiat University of the Arctic in Barrow received a copy of the summary report from an Inupiat friend. The faculty member issued a public attack, calling the short report "ethnocentric and parochial, demeaning and denigrating to the Inupiat people." He questioned the "entire methodological orientation of the research project, based on only a superficial understanding by the researchers, none of whom are conversant in Inupiat." The sentiments were formalized in a letter sent to Intersect over the signatures of individuals from the Health and Social Service Agency of North Slope Borough. They "publicly disavowed any participation in the study." and described the project as another version of "cultural imperialism."

Nevertheless, Intersect and the Barrow Department of Public Safety began to plan for the town meeting which was scheduled for January 24, 1980. Both agencies wanted a press release to proceed that meeting. Intersect saw the publicity as having the political impact necessary to galvanize solutions to the alcohol problem in Barrow. They hoped that public

awareness would shock the Inupiat into action to control the alcohol problem. Belief in this approach was also ironically shared by the Barrow Director of Public Health who, through a letter, asked the researchers to provide some numbers on alcohol use that would startle the community. She reported that another group had estimated that 50% of the adult population suffered alcohol problems and our higher estimate seemed useful to her. The researchers were asked to participate in procedures of reporting the results of the research that were to have far-reaching ethical and political implications.

The Startled Community and the Errors of Research Reporting

In mid-January, 1980, the results of the survey that had been presented to the Inupiat Steering Committee and Technical Advisory Group in Barrow in November were released to the press by Intersect and the Research Center. The report referred to increasing use of alcohol and social problems related to cultural changes concomitant with increasing wealth associated with North Slope energy development. The report indicated the degree of the problem, and made recommendations for modifying the Detention Program, for increasing surveillance of the flow of alcohol into Inupiat villages, for adding alcohol education programs, and for establishing a local alcohol agency. Long-range suggestions included slowing the flow of cash through the community, capitalizing it as a current asset, and investing in projects not necessarily in the North Slope Borough.

The January 22, 1980, *New York Times* press release was headlined *Alcohol Plagues Eskimos*. The Associated Press release said that alcoholism and violence had overtaken Eskimo society after the sudden development of Alaska's North Slope oil fields. The United Press International Wire Service wrote its story under the headline, "Sudden Wealth Sparks Epidemic of Alcoholism", with the subhead, "What We Have Here is a Society of Alcoholics." That same day, the North Slope Borough public information officer released a report that stated the community was stunned and angered by the content of the study, and that the manner of its release had done irreparable harm to the community. The Director of Public Safety was fired.

In reporting the study, the press confirmed the stereotype of the drunken Alaska Native, whose traditional culture had been plundered. The public exposure had brought shame on the community, and the people were now angry and defensive.

One of the researchers who had traveled to Barrow to present the report's findings at the two day public meeting found 300 Inupiat crowded into the church. Their serious faces showed little emotion, but they stayed 7 hours—until 3:00 A.M.—listening to the reports. On a projection screen they were shown color videotape prepared by a Seattle television station which depicted the many personal tragedies associated with alcohol in

Barrow. Inupiat leaders present in the audience spoke via videotape. The elders spoke in Inupiat, with English translation being offered by a woman who had just experienced a death in her family due to alcohol abuse.

The following day in the town hall, one of the researchers presented the findings of the study and answered questions from the general audience, composed of Inupiat and non-Inupiat.

Academic Critique

Later, the North Slope Borough, guided by requests from the Health Department, engaged a professor from the Center for Alcohol Addiction Studies, at the University of Alaska in Anchorage to prepare an academic critique of the research study. His mission was to "evaluate the study undertaken to determine if the conclusions arrived at are warranted on the basis of the procedures followed."

He believed that the findings used from previous studies on suicide rates (Kraus & Buffler, 1977) were inflated. He found the figures on the quantity of alcohol consumed to be invalid. The estimates of the frequency of intoxication based on association with the probability of being detained were termed "ludicrous, both logically and statistically." After several other criticisms of statistical method, he warned that because of questionable methodology, the recommendations should be taken cautiously. He went on to state that the people of Barrow had a right to be angry over the study because the authors spread misleading facts about the community and its people, both locally and nationally, and that the study had done a disservice to the field of legitimate research that might be of real value to the people of Alaska. He echoed the sentiments of many local scholars, who now recognized that future social research in North Alaska would be jeopardized by the community's angry response.

A non-Native faculty member of the Inupiat University of the Arctic stated:

We feel we have proven ourselves in the eyes of the community...the North Slope Borough Assembly has been the victim of a sophisticated hoax aimed at destroying the credibility and integrity of the Inupiat people. The research on alcohol abuse and the news coverage was the most demeaning and reprehensible sham. Instead of using Winchester and Remington rifles to destroy a people and a culture, as with the Indians in the 1880s, they bent words, numbers, and statistics to accomplish what was in effect a social and cultural genocide. These con artists hiding behind the guise of professionalism and religiosity, and acting as consultants to the North Slope Borough have

dealt a devastating blow to the Inupiat people and their cultural heritage.

In August of 1981, the Research Center presented a paper based on the Barrow Study at the Fifth International Symposium on Circumpolar Health in Copenhagen, Sweden. Following presentation of the paper, the Director of Public Health from Barrow rose to enter these comments into the proceeding:

The release of the questionable results of the study to a nationwide news source prior to informing the studied community is a classic example of researchers utilizing indigenous people as so many laboratory specimens. If we within the North Slope Borough are to work at solving our major health problem, alcohol abuse, we cannot sit by and let researchers publish erroneous sensational statistics....

The Dilemma of Reaching a Consensus

It was difficult for the Research Team to struggle through polemics and locate the voice of the Inupiat. Most of the speakers for the community had been non-Native. Contrary to the above quotation, the Steering Committee, composed of Inupiat elders, had heard our research report several months before its release to news sources. Their reactions at the time were, on the surface at least, in agreement with the presentation. They suspected that the problems were widespread, and understood them in terms of the rapid social changes thrust upon the culture during the past several decades.

Later, during the telethon on Alcohol Problems in Barrow, other Inupiat residents expressed the following statements:

How many detentions were there from 1968 to 1976? Do you watch for suicide? How many repeaters are in detention? Is divorce caused by drinking? What does an old person do who has to live with a drunk young person? Why doesn't the Arctic Regional Corporation ban alcohol from the hotel which they own? It is our fault if we get drunk and commit suicide. I am divorced, but my ex-husband harasses me when he is drunk; what should I do? Why don't parents see what their drinking does to kids at school? My husband drinks and then drives; what should I do? Someone gave me their child because they were out drinking too much; what should I do? My parents drink and I get scared; what should I do? Why wasn't something done long ago about this? We drink, not because of our personality, but because

of the political climate of the Arctic Regional Corporation, the Borough government, and the unrest around the formation of the Inupiat people.

The Inupiat understood their problems with alcohol in terms of stresses created by cultural and political change. Their perspectives were not unlike those of the Departments of Health and Public Safety, Intersect, the Technical Advisory Committee, and the project researchers.

The consequent misalliances of this research and its handling in the mass media have resulted in negative attitudes toward research in the area; have brought up ethical and human subjects questions regarding community studies; have called into question the use, dissemination, and ownership of knowledge; and have generated theoretical skepticism regarding the interface between medical diseases, social illness, and deviant behavior. The scientific merits of this study are a matter of public record and can be evaluated elsewhere by the reader. However, we believe that the mistakes experienced in this research study were less those of scientific methodology than of social and political naivete regarding the people of the community studied.

The community of Barrow was complex and multifaceted, containing many opinions and factions—non-Native and Inupiat alike. Our research team was sponsored by only a few of these factions, including Intersect and the Department of Public Safety. Methods were not developed to ensure more community participation because we wrongly believed that the Steering Committee and the Technical Advisory Committee reflected general public opinion. Finally, we became part of a process that allowed reports usually reserved for relatively private scientific books and articles to be used in the mass media for public information purposes. Such a process becomes a complex scientific-professional ethics issue warranting carefully constructed formal procedures and policies to ensure that the integrity of the people studied in research projects such as this is not violated. Contract research projects are perhaps more vulnerable to exploitation in this regard than government or foundation-funded research, in that the results of the latter usually first appear in referred scientific publications. They are therefore subjected to scientific review before the public media has the opportunity to report controversial findings.

What Can Be Done

The question of how to use research results which may ultimately be detrimental or denigrating to a community under study requires adherence to established professional ethical standards, considerable soul searching, and negotiation with subjects, sponsors, and third parties who may be involved. Research involving issues implicitly reflecting the moral values of a community demands particular sensitivity in this regard. Mental illness,

suicide, homicide, venereal disease, acquired immune deficiency syndrome (AIDS), and drug and alcohol abuse are among many sensitive problems which require more social and biological research.

However, successfully carrying out population studies of these often life-threatening conditions is predicated upon establishing cooperative and informed relations, wherein the benefits of the research clearly outweigh the risks to the subjects, their families, and their communities. We hope that our experience will provide a lesson demonstrating the degree to which the questions and methods of science are rooted in ethical social, and ethnical political issues of the times, and of how scientists must self-consciously include these sometimes intangible, value-laden factors into their research design and planning.

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WHO HAS THE RESPONSIBILITY? AN EVOLVING MODEL TO RESOLVE ETHICAL PROBLEMS IN INTERCULTURAL RESEARCH

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Dr. Edward Foulks should be commended for his courage and candor in reporting on the "misalliances" and the turmoil that followed from the 1979 study of alcoholism in Barrow, Alaska. This is particularly true since the condensation presented here in many ways does not reflect the complexity that shows in other longer and more sensitive versions of the actual research findings. Unfortunately, on one hand, most of this original material may not be available to the readers of this *Journal*. On the other hand, the skeletonized research data presented here are too limited to discuss their statistical and methodological aspects. However, they do provide a background against which the ethical issues can be addressed.

It would seem that the various questions raised can be divided into two categories: First, what are the responsibilities of the researcher whose subjects are a complex, living community? Second, what are the responsibilities of the community itself? Both researcher and community are faced with additional issues created by elements outside outside their control, which may be a third category.

Responsibilities of the Researcher

In considering the responsibilities of research personnel, it is striking how parallel these issues are to those that evolved over the past decades in relation to concern for research and scientific publications truly respecting the rights of the individual participating in medical or psychological research. I can recall in my own graduate school days, when it was an accepted practice to deceive subjects either by withholding information or by outright deception—whether this was to test for placebo effects, or study any of a wide variety of phenomena in perception, social interaction, conditions of and limits of learning, emotional reactions, etc. The zeitgeist seemed to accept the idea that scientific research always had a goal of contributing to human knowledge and thus was an ultimate "good". Few research-oriented scholars were concerned with the impact of their activities beyond the limited scope of their particular experimental design.

Contrast this with today's acceptance of the necessity for a review of every research proposal originating on an academic campus, or funded by

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governmental auspices (and most foundations as well). These reviews ensure that informed consent is given by individual research subjects, that any deceptions that are a necessary part of the design be disclosed at an appropriate point in the proceedings, and that if any damage—physical or psychological—can be traced to participation, this be treated. The expense for “undoing” the harm caused even inadvertently is usually born by the employing institution of the researcher, but could ultimately and legally be assessed against the researcher if no such guarantees were available.

These conditions, or something very like them are also now a part of most clinical practice with individual clients or patients, both in medicine and psychotherapy. It might also be noted for later discussion that in many if not most publications of clinical or psychotherapeutic research and practice, if an individual case is used, the author must take pains to camouflage the identity of such a person. In addition, the consent of the individual to the use of his/her own case history is generally obtained in the same manner that informed consent is secured for research participation. It is worth noting that the goals of professional activity in these cases is almost always to change behavior of individuals, and licensing, degree requirements, and reviews of other kinds are accepted as safeguards for certifying the competency and assigning the responsibility for the therapies to the individual researchers, therapists, and authors.

These concerns for the well-being of the individuals serving as research subjects can now be seen extending in several directions. Perhaps the most familiar example is the current controversy over extending rights to ‘humane’ treatment to animals used in research. More slowly, but distinctly emerging, is a convergence toward the concept of cultures and communities as living systems, having similar rights.

The idea of cultures as living, evolving entities is not new. To some extent the field of anthropology has been dominated by it in at least two ways. Anthropologists have traditionally searched for isolated “pure” (and often primitive) societies to avoid the contamination of cultural contact and mixing that is otherwise prevalent. Almost simultaneously, they have debated whether tools, concepts, and behaviors shared by widely separated cultures are independent discoveries or the result of cultural diffusion and unrecorded contact between social groups.

More imaginative development of the potential of societies to evolve is found in science fiction. The idea lies behind a work as early as the 1920s by Olaf Stapledon entitled *First and Last Men*, and as recently as 1988 in *Alien Blood* by Joan Vinge. *Alien Blood* is particularly an apt illustration, since it deals with the problems of mixed ethnicity, as well as the potential extinction of one ethnic group and the evolution of another. Ironically the problem of the ascendancy of the individual or of the collective group is present, in both the failed and the victorious societies. All of these characteristics seem reflected in today's world, but as works of fiction rather than science, they need not be taken literally.

What does need to be taken seriously is whether social research, particularly that which is acknowledged to be needed in order to attempt social change, should in some way be subject to a review of the rights of the group involved. Should the community be given an opportunity to give informed consent to the research and to publication? Are there cultural or community rights which should be respected and protected, just as in other situations the rights of the individual are supported? Should research competence be reviewed to include ethical and moral judgements about the necessity of spending time and energy to get acquainted with the Culture itself and the interactions of all the parts of the Community? Finally, if the answers to the preceding questions are "yes", whose responsibility is it to require compliance?

Foulks suggests that to some extent he and his colleagues were naive in accepting the views of the contracting agency, Intersect, and the advisors it selected as representative of the total community. It would appear that they were also a bit naive in expecting that their skills in research and scientific reporting were a sufficient safeguard if they kept within their required tasks, and left the utilization of the results to Intersect and others. When, perhaps inevitably, they were drawn into defending their research to a distressed and disturbed community, they found themselves to be scapegoats. Since the days of ancient Greece those in power have had a tendency to kill the messenger who brought unpleasant news. Communities as well as individuals often find it much safer to attack outsiders than the politicized factions which had supported the research originally. Given the circumstances of their subcontract, it is not clear that Foulks could have changed the way community rights to the research results were not safeguarded. The question remains however: Should Foulks and his colleagues have tried to do so?

Responsibilities of the Community

In the 1970s, perhaps beginning even in the late 1960s, a "buzz word" among social scientists who were also American Indians was, "We ought to have a Green River Ordinance for researchers." The town of Green River, Oregon, had effectively controlled the nuisance and demoralizing impact of solicitors and peddlers going from door-to-door by requiring a local license of anyone undertaking such activity. Approval of applicants took into account the number and kind of persons active in the community at any given time, as well as the perceived integrity of the solicitation or the value of goods offered for sale.

Anthropologists and psychologists in particular, but to some extent sociologists and other social scientists, have been traditionally allowed to study and record experimental and clinical data on American Indian and Alaska Native populations quite freely, often in the name of "academic freedom." However, the impact of the results of their studies often had fallout

similar to that described by Foulks of jeopardizing future cooperation or welcome of fellow professionals. Many tribes recall anecdotally the exploitation of research data in ways that they had not expected—often to their detriment. Since no outside agency seemed interested or willing to intervene in this process the idea arose that the Tribal community itself should take control under its self-governing powers.

Only a few American Indian Communities were able to take advantage of the concept, but one example illustrates the positive results. The Zuni in Arizona have for at least a decade had such authority. Periodically the whole Zuni population is polled for its concerns and their selection of the major problems that the Tribal Government should address. The result should be humbling for social scientists. Road development, housing, resource management, and other problems of like nature often head the list. Nevertheless, concerns about education, mental and physical health, and social problems are often included.

As these topics are prioritized, they are consulted whenever a social scientist wishes independently to utilize the Zuni population in any research. If the topic fits within the priorities of the community, discussion and negotiation is opened to see if the proposed project will help find answers to their needs. Without a clear relationship to the priorities and a willingness to include the Zuni communities in the planning and execution of the research, it is not approved. In this situation it is exceedingly difficult for any social scientist to find appropriate samples of people to participate in any research project. The effect is to control the number and kinds of research according to the Zuni views of their own needs and advantages.

In the case of the Zuni, such policies have worked out well. There are not only helpful community/researcher collaborations, but also there is a mutual exchange between cultures. Social scientists learn about the Zuni people from working with them rather than from the position of a detached outsider. Moreover, members of the Zuni community are given an opportunity to participate in the planning and execution of research studies, and are therefore better able to understand the implications and limitations of the results, whether positive or negative in relation to their goals. A fair number of Zuni people have continued into research training, and are developing a resource pool of experts indigenous to the community.

There are probably other American Indian and Native groups that have successfully worked out a method of "licensing" research on themselves and within their geographic territory. However some units of tribal government are so rife with power struggles between various factions that the necessary stability for assuming this kind of responsibility cannot be achieved. The imposition of the Anglo American model of one-person-one-vote, and majority rule on a culture that customarily depended on consensus for major decision making has had a variety of impacts on tribal communities and their ability to assume control of and responsibility over their own affairs.

Many benefits flow from a real assumption by the community of the responsibility for utilizing appropriate skills and tools from the mainstream culture to help solve problems in a minority culture. One of these advantages is to place ownership of the research data and utilization squarely and clearly with the community itself. If research is subsidized by grant funding sources, this needs to be negotiated when developing the original contract to avoid later conflicts and misunderstandings.

A closely related matter to be clarified in contracts between a community and research scientists is the manner and form of reporting results to the public domain. How the information is to be presented at professional meetings, published in professional literature, and released to other media can be clearly understood by all parties. The use and dissemination of the results within the community itself should also be provided for in the basic negotiations, including advice from the researchers and community leaders on the probable impact of such dissemination in various forms.

One advantage of such reviews by the cultural community is the resulting sensitization of the social scientist to reaction to professional jargon and unexplained cultural usages. For example, in the Barrow project, "detention" by the Department of Public Safety implies a police arrest to non-members of the Inupiat. Actually it was a community devised plan to respond with a preventive intervention when family or friends requested help with a person who was dangerously out of control due to heavy drinking (Folks, 1987, p.350). Conversely, the term "culture broker" used by anthropologists and social scientists has a specific technical meaning. However, it implies to others that there is an exchange of cultural information for personal gain. Interestingly enough similar problems of communication arise in developing the protection of individuals.

When comparing individual and community as entities capable of and requiring the respect and "informed consent" provisions, there are perhaps somewhat different responsibilities required of communities and cultural groups than of individuals. Individuals have a mutually agreed upon and recognized identity. The community must define itself and its members, and must find ways to ensure that all subgroups of which it is composed are adequately represented. The community must, in other words, answer a number of questions, including the following: "Who compose our community?" How are they adequately brought into participation in the decision making and research participation? If the geographic boundaries of the cultural community include non-members of the ethnic group, is interaction between the two cultural groups significant as an element in the phenomena to be studied? How will this second cultural group be included in the protections of confidentiality? To what extent do the resident but non-members of the cultural community share in the planning of the research and in the dissemination of the results?

The acceptance of the responsibility by the ethnic community to provide answers to these questions is not yet well developed. However it seems to

be a predictable trend of the future. Shared responsibilities of this sort would allow mutual projects of research and evaluation of social changes in ways which are not now possible, or only beginning to emerge. They involve role changes for both researchers and for cultures and social units that may not always be comfortable. Clearly defining acceptable boundaries of both social science research and its application could facilitate the resolution of some of the ethical problems involved.

Other Considerations

There are many elements of any community's life that are outside both its own control and that of any social scientist conducting research. Obvious elements are cataclysmic events of natural origin including storms, weather changes, earthquakes, meteor falls, etc. Another such set of events with a high impact are due to human activities such as wars, missionary activities, technological changes, and in this case the decision by distant corporations to exploit natural resources hitherto ignored or unknown to the cultural group involved.

Whenever social scientists note the impact of such events, it is necessary to probe for the way they are understood and interpreted by various parts of the community, and it is often helpful to seek historical data. For instance, it is interesting that the older members of the Inupiat community attributed the stress and misuse of alcohol to the events of the 1940s which involved World War II. The younger adults, coming to maturity after that event, attributed the present problems to the increase in money and availability of alcohol associated with the development of the Alaska Pipeline. Neither the community nor researchers thought to trace any historical records to the periods of time before the living memory of those now resident, but it is predictable that distilled spirits were introduced with similar effects with the first European contacts — probably the Russians and most certainly the Anglo-American traders. The lack of historical perspective by the community may be a large factor in their discomfort with the data describing a new cultural element—social stratification—which seems to have introduced differential behaviors under the influence of alcohol.

Almost certainly among uncontrolled forces one must put the sensation-hungry elements of the public media, both in the press and on television and radio. Foulks and his team of researchers may regret their participation in making their research information public on a national (and possibly global) scale. Once they had cooperated in the preparation of the press release, passively if not actively, headline writers and others seeking to gratify their own interests could not be restrained by the Inupiat community itself.

Indeed it may be that some sensationalism was desired, if not cultivated, by the major contractors as a way to "galvanize" community action (Foulks, this issue). Intersect may have wished merely to push away what it

perceived as apathy as if it were a small boulder on the trail, but what resulted was more like an avalanche.

With hindsight it seems clear that the researchers should have delayed any premature announcement until the community leaders and the population as a whole had had an opportunity to process the information and to give the research team feedback about it. This might have revealed the missing young adults' opinions and secured their involvement. Whether, under the terms of their contract and the division of activities between themselves and the major contractor, Foulks and his colleagues had the ability and power to do this is, of course, another question. Certainly neither they nor Intersect had the power in any case to prevent the 'leaking' of the research report to the non-Native faculty members of local colleges, whose emotions were also raised and who fed the fires of sensationalism with their polemic attacks.

It is clear that Foulks is a sensitive social scientist who found himself in a fairly common but nevertheless undesirable position of having his research misunderstood and misused without being in any position to mitigate or undo the damage to his personal reputation and that of the professions involved in social research in general. By bringing the incident to professional attention without seeking self-justification, he offers an opportunity for colleagues to wrestle with the ethical problems involved.

In the same spirit, perhaps these comments suggest ways of developing a more generic answer to the issues, rather than any effort to assign blame. It may be embedded in human nature to attack the bearer of information rather than the problem the messenger is reporting. Perhaps by participating in the evolving identification of cultures and communities as requiring protection of their rights in the same manner as individuals participating in research and scientific publication, and by developing the appropriate responsibilities to be assumed by both the communities and those engaged in social science research, there will be a mitigation of such devastating effects in the future.

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LIMITED NOTIONS OF CULTURE ENSURE RESEARCH FAILURE

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The candor of Dr. Edward Foulks in discussing the problems encountered in this piece of research is highly appreciated. There are few of us that have not made major mistakes in the conduct of our work, yet it is not often that we are willing to have others scrutinize and comment on research that has misfired. Foulks' openness provides the opportunity to rectify some of the misguided approaches that have been all too prevalent in cross-cultural research.

Discussion of the aftermath of this research can take place at several levels, but I would like to focus on an underlying issue that may explain a great deal of the controversy that was generated. Cultural change is most often seen as occurring along a single continuum from "traditional" to "modern." Anytime a traditional culture comes in contact with a more modern one, it is assumed that people will gradually move from the "old" to the "new" and that while in transit, they are confused, are experiencing "acculturation stress," and are generally unable to function competently. Furthermore, there is usually an implicit assumption that traditional is inferior and modern is superior. This latter point is often denied, yet there are ordinarily subtle, and not so subtle, indicators that this basic value position is held. Another element of this model holds that since movement is on a single continuum, something of the old culture is always lost when one embraces parts of the new culture — it is basically a zero-sum game. This is a very narrow and simplified view of how cultural change operates and has serious limitations from a research perspective. This view can also lead to a devaluing, either actual or perceived, of the culture of the people under study. When this occurs those of the minority culture are likely to become actively resentful or passively resistant.

There is ample evidence that the Point Barrow research was guided by this one-dimensional model and that this led to many of the problems encountered. The perceived inadequacy of the Inupiat people is clearly demonstrated in the nearly total absence of their participation in the research. The project did include a steering committee comprised of Native people, but their participation in the research was minimal until the latter stages of the project when the research was completed and the recommendations formulated. At that point, they could only endorse the

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findings with a faint hope that some good would come of them. In fact, the entire project was conceived and directed by non-Natives. Even at the point where emotions were high and the researchers feared physical reprisals, recourse was to another group of outside experts to resolve the controversy (Klausner & Foulks, 1982).

A lack of confidence in the Inupiat people was clearly revealed in the attempt to "shock" them into action by reporting the most extreme results available. Underlying this act is a belief that the Inupiat were incapable of seeing the destruction being brought about due to alcoholism and that only by "scolding them" would they see the extent of the problem. Indeed the anecdotal information from the elders at the community meeting shows that this assumption is false; the elders were very aware of what was happening among their people and they were asking for assistance with the solution.

With some careful listening to the steering committee, events may have taken a different course. These community elders were expressing a strong belief that the problem of alcoholism was one of rapid cultural change. The research team might have done well to stop at this point and ask some searching questions about what they meant, and how things could be made different. The elders were harkening back to a time when things were better, when they had a sense of control over their lives, and when a sense of cultural integrity allowed them to ward off social problems. I realize that there is a universal tendency to look backwards to the "golden age." However, while life in the past for the Inupiat may have had its own problems, community leaders still felt that the problems were their own to solve. Instead of listening and trying to work constructively with the elders to find new paths and solutions, the researchers proceeded to "demonstrate" that the most traditional people were those with the greatest alcohol problems. This type of finding, when presented out of meaningful context, further devalued Inupiat culture and, instead of moving toward answers, only added to the problem.

Procedural Issues

An extremely short time period (three months) was allowed for the initial report to be issued. In addition to being an inadequate length of time to gain an understanding of a problem of this magnitude, the short time period could generate the suspicion that only preconceived ideas would be registered in the report—or, that there may have been a "political" agenda involved. Indeed, after decades of research, we only have a minimal understanding of the causes of alcoholism in general, let alone what might be leading to alcoholism in this unique socio-cultural environment.

As alluded to previously, the steering committee should have been much more fully involved from the beginning. A general principle of community development is that people have an investment only in those things they have had a hand in creating. Even if the report was fully accurate

in its findings and the recommendations highly appropriate, the Inupiat people would not have a sense of ownership nor would they feel responsibility for the problem or potential solutions.

Furthermore, there was no mechanism in place for utilization of the results. Simply calling the community together to hear the results does not ensure that change can or will be made. Accurate data are a prerequisite to change, but do not lead to change without further intervention, especially in a community that has become immobilized by the effects of alcohol and the rapid erosion of traditional methods for dealing with social problems. In the past several decades, much has been learned about community organization and community change, and these principles could have been adapted for use in Barrow. Structures for effecting change should have been in place even before the data collection took place. Meetings should have taken place to examine and explicate community values and beliefs, and these discussions could have led to the formulation of research questions that had real meaning for the people of Barrow. When the research results were available, working groups could have proceeded to work on problem areas that were pertinent and of immediate importance to the people of the community. With this type of preparation and the assignment of responsibility for the results, a cooperative, rather than competitive, atmosphere could have been established.

The issuing of a press release is puzzling. The intent of the research was to help resolve a problem in Barrow, yet the research team felt it necessary to give the problem widespread attention. Was this another attempt to chastise the people of Barrow and force them to change through embarrassment? Apparently so. In another publication Foulks writes, "They (the research sponsors) saw the publicity as having the political force necessary to galvanize an alcohol program in North Borough. White and Moeller had engaged researchers of some prominence precisely because their report would receive national press attention" (Klausner & Foulks, 1982, p. 306). A further rationalization for the press release was the "scientific" standard of not withholding data from the public. In an attempt to project this work as a true scientific enterprise, the press conference itself was held at the University of Philadelphia. All things taken together, it appears that the press release had more to do with politics and publicity than it did with science. If community change was the scientific goal, the way the results were disseminated was certainly counter to that goal. As noted above, it seems that the information could have been disseminated within the community and served as a basis for rational change. The community meetings were indeed an attempt to do this; however, the press release vitiated any effect that the meetings may have had.

The press was also misled into believing that something inherent in the Native culture was accounting for the alcoholism problem, which spread the myth even further. There is considerable research showing that more traditional Native people (using the restricted model) have greater problems.

This "finding" often sends researchers on a quest for those traditional values or beliefs that account for higher levels of alcohol use, for example. Thus we see the search for Indian alcohol abuse turning to such explanations as emulation of the vision quest, or for drug abuse as resulting from a tradition of peyote use — ignoring, of course, that peyote has a much shorter history among most Indian people than alcohol. Generally, these types of explanations are not empirically based, but they have such common appeal that they take on an aura of "truth."

The search for unique, cultural answers also obscures the fact that, in terms of problem behaviors, cultures are usually more alike than different. In losing sight of these similarities, the opportunity for effective interventions is lost. We do know some things about prevention and treatment of alcohol abuse and should not be reluctant to use this knowledge in culturally different populations. Furthermore, we know that socioeconomic stress can lead to increased substance abuse, and is undoubtedly a better predictor in a population than any exotic cultural explanation.

Most of the problems Foulks outlined in his paper stem from the inadequate conceptualization of the cultural identification process. The Inupiat people undoubtedly felt that their culture was under attack and responded accordingly.

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MISCONTINENCE AND THE BARROW ALCOHOL STUDY

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Dr. Edward Foulks is to be commended for sharing his agony and experience with researchers who often face similar dilemmas by the nature of their participation in "a community of scholars" concerned with American Indian and Alaska Native issues. Those researchers at the Center for Research on the Acts of Man might have foreseen the problems that Foulks encountered, but they did not for reasons that are embedded in the very nature of behavioral science research itself. It is not that the necessary understanding is extremely difficult to acquire, it is more to the point that in research, as in many other arenas of action, the taken-for-granted assumptions are seldom examined. This is true in part because philosophical analysis is often separated from the technique of understanding through the use of behavioral science. Foulks leaves us with the heart-felt imperative that the Barrow Alcohol Study is an example of how "...scientists must self-consciously include these sometimes intangible, value laden factors into their research design and planning." I want to examine "these intangible, value laden factors" more closely with the hope that we can identify more precisely their nature.

Behavioral scientists often fail to be sensitive to the fact that a technique created for some end (or aim, purpose, or goal) is often imbued with values that are quite distinct from the process of human valuing which seeks to answer such questions as the following: Does this technique promote "good?" "Ought" this technique have been used in this way for this end? Who benefits from this technique? Who is harmed?

Behavioral science has thoroughly incorporated one philosophical orientation: the position of Francis Bacon which aims knowledge at power over nature (including human nature), and uses this power for the improvement of the human condition. Access to this power over nature through scientific technology is made possible by reliance on sense impressions, rational thinking, and scientific method, as opposed to the understanding of nature through contemplation. Implicit in a strict adherence to this "objective science" orientation is the notion that the scientific process is value-free; that knowledge and understanding of a phenomenon for its own sake, and sometimes for the purpose of "objective" control over nature and human nature, are the goals of science.

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Consequently, scientists are often troubled by the fact-value distinction. The study of value is frequently separated from and often considered irrelevant to the study of fact. The fallacy in this traditional perspective—and I use the term “traditional” purposely—is that technique (in our case behavioral science research technique) both displays value effects by its very existence and is conducted in value fields or systems which shape the ends to which it is used.

Research Techniques and Latent Values

Consider the first issue that the value effects technique displays by its very existence. The idea that technique creates imperatives and institutions that reinforce its existence in an accumulative and selective way is not a new one (Ellul, 1964; Winner, 1977). Nevertheless, a precise understanding of these effects of behavioral science technique among American Indians and Alaska Natives is not always clear, or when clear, is not often expressed. Researchers hold positions in academic research institutes, private consulting firms, government research, organization social service agencies, and university and college schools and departments. These institutions often share a firm commitment both to the “advancement” of knowledge of and for its own sake, and also for “objective” knowledge application for perceived social good. This shared orientation demands that research conclusions be justified on the basis of accepted scientific procedures, and that these conclusions be shared with a community of scholars and those who apply their knowledge through publication. Positions are obtained and careers are maintained to a large part on the basis of grants or contracts received and publications accepted in refereed journals, monographs, and books. The very existence of research techniques has created priorities and institutions which predispose the examination of human behavior, in our case the object of study being the “problem drinking” of a group of Alaskan Eskimo people.

Science above all is to be progressive and cumulative, a subset of the Western ideology of liberalism which Grant (1969) has defined as “...a set of beliefs which proceeds from the central assumption that man's essence is his freedom and therefore that what chiefly concerns man in this life is to shape the world as we want it” (p. 114). Contrast this Western orientation with that shared by American Indian and Alaska Native people which defines humankind's role as being one of minimal interference with the other elements of nature. If animals are to be taken for survival, they must be talked to in the right way so as not to anger their spiritual essence. In this sense, scientific research displays the potential of being an entirely ethnocentric enterprise when imposed without community understanding, consensus, and consent within American Indian and Alaska Native cultures. There may be radical differences between Western science and ethnoscience as practiced by American Indian and Alaska Native people. The issues of

science for whom and as requested by whom, and for what ends becomes the all important concerns.

The Process of Human Valuing

This leads to a discussion of the second issue with respect to technique and values: the process of human valuing within the context of value systems. In the current case, we must consider both the value system of the community and that of the outside and dominant society as expressed through behavioral scientists. Who bears responsibility for deciding the good, the ought, and the nature of being (Jonas, 1984)? Placing value on events or phenomena occurs in the context of larger metaphysical systems which, through the definition of the nature of existence or being, define meaning for individuals and collectivities. Stated in another way, if we agree with Jonas that metaphysics is a doctrine of being and ethics is a doctrine of action, ethical imperatives for action occur relative to metaphysical statements of being. Surely we know enough about the difference between Western metaphysics and the various American Indian and Alaska Native expressions of the nature of being (world views, if you wish) to be extremely careful about placing values, or notions of what ought to be, on American Indian or Alaska Native communities. The value of behavioral science research is for each community to decide. As professional researchers representing the dominant society, we are constantly in the danger of implicitly and explicitly defining ethnocentrically not only what is good research methodology, but what is the good end or purpose of the research process.

The good or valuable end of the research process in the Barrow Alcohol Study could have been defined as using scientific method to understand the legitimacy of the common claim by members of the community that the cause of the "alcohol problem," as defined by the community, was rapid cultural, economic, and political change. But, this research should have been conducted only if it was desired and sanctioned by the community. Foulks seems sensitive to the fact that many community people blame the alcohol problem on the "stresses created by culture and political change." Yet, the initial design phases of the research seemed not to adequately address the issue of the impact of culture contact, thereby speaking to the perceptions and feelings of the community. The issue of why the community displayed alcohol "problems" was not fully integrated into the research design. Rather, the design chosen attempted to identify alcohol use by "social characteristic", as if there were good and bad characteristics leading to lesser or more drinking. I wonder if there is any coincidence that degree of church attendance was a key variable, given the fact that the former minister of the Presbyterian Church in Barrow was director of the consulting firm that subcontracted the research of the Center for Research on the Acts of Man?

If it was the will of the community, the valuable goal of the study could have been defined as identifying positive and effective steps the community could initiate or had initiated to solve the alcohol problem on their own through a process of local action and self-determination. Instead, the community was simply dissected by type of behavior and social characteristic, leading some community members to feel they were the objects of research for its own sake instead of effective initiators of positive action. The dependency feelings and fears many may hold from years of colonial exploitation and economic subservience seemed to have been exacerbated by the research.

Foulks concedes that mistakes were made in the research process, particularly with respect to the role of the press and the procedures through which the community at large were consulted and informed. The "shocking effect" of the press release (purposely arranged by two agencies—Intersect, directed by the former minister, and the Barrow Department of Public Safety) certainly shows that both factors needed to be considered much more carefully. I wish Foulks had also mentioned such problematic issues as: a) the need for anonymity of individuals and the community in the research and publication process (especially with regard to what he concedes to be a "sensitive" problem); b) the potentially biased role of the former minister throughout the research process; and c) the fact that two key representatives of local agencies of social control (the Barrow Director of Public Safety and the Barrow Director of Public Health) were advocates for this research project, perhaps against the consensus of the community itself.

An Ethical Paradigm

Elsewhere, I and my colleague have examined some of the ethical issues involved in the introduction of biomedical technology (including technique, knowledge, and devices) among the Puyallup Indians, a Coast Salish community of the southern Puget Sound in Washington State (Guilmet & Whited, 1987). We created an ethical paradigm which may be effectively generalized to any situation in which the introduction of technology is occurring in a multicultural or multinational culture-contact situation. In such contexts, the nature of the good with respect to the introduction of technology is not always clear. What may benefit one social group may be detrimental to another. This is especially true when the culture-contact situation is dominated by a technologically more powerful culture. We distinguished between the Good, that which contains that action which would result in the best for all social groups (the common or universal Good), and the culture-bound good, that defined by each social group. We also defined several types of ethical action by culture brokers, including: a) continence, action consistent with the common Good by those who know the common Good; b) miscontinence, action in accordance with a culture-bound good that does not conform to the common Good; and c) incontinence,

deliberate actions inconsistent with the common Good by those who know the common Good.

I would classify the ethical action displayed by individuals involved in the Barrow Alcohol Study as miscontinence. The research team knew the good in their own cultural sphere, but these actions, consistent with their own perceptions, did not result in the common Good. In retrospect we can see that their perceived good blinded them to the injury their work would have on the integrity of the people studied. Let us not forget that we social scientists are cultural brokers who bring to our communities various disciplinary techniques which can be very ethnocentric and can cause injury to the people studied. If we are to act responsibly, we must know the end or goal toward which technological activity should be directed, must know the consequences of technological actions prior to the actual performance of such actions, and must act on the basis of both types of knowledge (Mitcham, 1986).

We must break free of our own personal cultural filters which define good in a way that may not be consistent with the common Good. We must understand that the common Good can be defined; it can and must be defined by the shadow that is cast by nonrecognition and noncompliance. The shadow that was cast by the Barrow Alcohol Study is the subject of Foulks' article. The question becomes our willingness, interest, and desire to visualize the shadow before it appears. More community participation, complex scientific-professional ethics with carefully constructed formal procedures and policies to ensure that the integrity of the people studied is not jeopardized by access of mass media to relatively private scientific books and articles, and scientific review before the media has the opportunity to report controversial findings, all are excellent suggestions by Foulks for addressing the misuse of behavioral science research.

I have added that we must consider the ethical and metaphysical issues which arise when we introduce behavioral science techniques into American Indian and Alaska Native communities. While I am optimistic that the critical and reflexive perspective encouraged by the Foulks article will help develop more socially conscious practices, I still believe somewhat pessimistically that many research projects will continue to be conducted in ethically insensitive ways given the effect of self-interest on the research process and the imperfect understanding of the philosophical context of behavioral science research which is shared by many researchers. On the other hand, research ethics may benefit from the fact that communities will demand to set research priorities and methods as they become more aware of the downside of disadvantageous research designs. I have elsewhere discussed the effect of traditional Eskimo patterns of cognition on the acceptance or rejection of introduced technology (Guilmet, 1985).

In any case, significant change in our practices must occur or future research opportunities among American Indian and Alaska Native communities will become increasingly scarce. This might have a devastating

effect on the alleviation of serious social problems. My feelings are best expressed by the last sentence from the previously-discussed article (Guilmet & Whited, 1987) concerning the introduction of technology among Puyallup Indians: "Though we eagerly await the appearance of those who know the Good and somehow do the Good consistently, we recognize that most of the saints are dead" (p. 197).

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THE BARROW STUDIES: AN ALASKAN'S PERSPECTIVE

ARON S. WOLF, M.D.

Dr. Edward Foulks and his colleagues had been asked in 1979 to be a subcontractor on a study commissioned by The City of Barrow Department of Public Safety relating to their concern about traumatic deaths associated with alcohol abuse in their community. The goal as outlined by the Department was "to receive research results which might enable (the Department) to establish more effective programs to reduce morbidity and mortality." In this present paper, Foulks presents a retrospective look at this study.

Foulks denotes the differentiation of the scientific merits of his research from the political naivete of his group as well as the political, social, and ethical uproar that ensued as a result of this naivete when the data was released. I certainly would agree that the baseline data that Foulks gathered was valid as a core sample. I would also find, however, that although the study included the Kraus and Buffler (1977) statewide data on suicide and alcohol, it did not address the enormity and pervasiveness of the "alcohol problem" in all of Alaska.

A Statewide Alcohol Problem

The major problem from an Alaskan's viewpoint is that Foulks' work focused on Barrow in 1979 as though the alcohol use pattern were a new phenomenon and as though Barrow somehow has a problem that is not endemic throughout every community in Alaska.

In a subsequent publication *Eskimo Capitalists*, Foulks does look at historical perspectives, but only as they relate to Barrow (Klausner & Foulks, 1982). That retrospective, however, was not a part of the original report that caused the uproar and consternation. The original reports, as written, did not seem to take into consideration long-term, statewide issues, such as the effects of alcohol in Alaska on both Natives and non-Natives, the sociological pressures of change emerging in all of Alaska, or the biological differences in the physiology of handling alcohol by Indians and Natives.

The response of Alaskan Natives to alcohol has been well documented since the time of "contact" with American and European sailors. The historical effects of alcohol on North Slope communities are elucidated by

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James Michener in *Alaska* (1988) when he describes contact by New England sailing ships to the North in 1875. "The (captain) taught two of the older men how to transform molasses into rum and when the first heady distillate appears, the islanders were lost. In the season when they should have been at sea catching seals and storing both the pelts and the meat, they were reveling on the beach. In the more arduous months when they should have been tracking the walrus for its ivory tusks and again for meat which would sustain them for the coming winter, they were drunk and happy and heedless of the passing season."

The above description of events almost 100 years before Foulks' study has clearly nothing to do with oil, 20th century change or the city of Barrow, but it is a recitation of the long standing serious problem of alcohol in the Native populations of Alaska.

Statewide Socio-Political Changes

Foulks' study also did not look at Barrow in the context of the major socio-political changes that were happening in and around it in the 1970s. These included Statehood in 1958; the formation of the Alaska Federation of Natives as a viable lobbying entity; the Alaska Native Land Claims Act of 1971; the formation of Borough City government; the Molly Hootch school decision case; and the placement in Alaska of a great number of Vista workers, to name only a few. These changes involved land, strange corporate and governmental structures, and relatively great wealth throughout the state. They caused profound changes to everyone in every nook and cranny of Alaska. Life in every village had also changed radically with the introduction of electricity, television, freezers, plumbing, telephones, and satellite dishes. The fabric of village life in 1979 was startlingly different from that in 1969, and indeed has made another quantum leap in 1989.

Barrow was only different from any of the other predominantly Native centers because of its proximity to Prudhoe Bay, but all of the other changes were extant in every other village and town in Northern and Western Alaska. The alcohol use patterns are also nearly identical in all of these communities. Because Barrow did have access to greater wealth in a shorter period of time, there was indeed an accelerated wrenching of all of its socio-political structures. The disorganization, fanned by certain non-Native "advisors" during that period, is currently making headlines in Alaska as the federal government is prosecuting several of these "advisors" in a corruption trial. These stresses, as they existed then or as they are acknowledged now, have not played a part in Foulks' writings. The alcohol and violence statistics for the other communities are very similar to those in Barrow, even though Barrow may have been subjected to a greater socio-economic upheaval at the time.

Shortcomings in the Barrow Study

If one is going to look at the socio-political stresses on Alaskan drinking patterns, it would be important to assess these patterns for both the Native and non-Native populations. It is also appropriate to compare the patterns in several communities of the same size such as Bethel, Kotzebue, and Dillingham, as well as at least one Alaskan urban community. As further control populations, one might also wish to look at the Inuit communities in Canada and the Navajo reservations in the Southwest. Even though Foulks and his colleagues were employed to look at Barrow, these comparisons would be important both in a pragmatic sense and in a scientific sense.

Although large scale sociological and political factors indeed played a role in the drinking behavior in any and all of these communities, I believe the Barrows study focused much too heavily on particular sociological phenomena. These foci include looking at having Caucasian surnames or belonging to a specific church group as the sole cause of the alcohol problem and not simply as important factors.

In addition to assessing one or more control communities, the alcohol and violence statistics from the Alaskan court system and the Alaska Department of Corrections should have been assessed. Ninety percent of the violent crimes in Alaska are alcohol-related, by both Natives and non-Native Alaskans. The Native population is overrepresented in the correctional system (35% of this population as compared to 15% of the statewide population,) and 95% of the Natives convicted of a felony had an alcohol involvement in their case (Department of Corrections, 1987). These are statewide figures with no preponderance of Barrow residents or even North Slope residents.

There is also a growing literature (Phillips, Coons, & Wolf, 1984; Phillips, Coons, & Wolf, 1988) both in Alaska and in other communities that Natives physiologically process alcohol differently from their non-Native counterparts. This "alcohol blackout" literature denotes that Indians and Natives are prone to "blackouts" early in their drinking behavior and that both physical and sexual aggression seem to be an integral part of this phenomenon for a small proportion of the effected population. This difference appears to be genetic as to the effects of alcohol on different parts of the brain. The potential for violence by the activation of the limbic system may account for the large number of Indians and Native in custody for both felonies and misdemeanors who have no memory of the event. These individuals have a very different drinking pattern from the non-Native alcoholic. They need not drink every day, but they drink large amounts when it is available. The "blackout phenomenon" is a midbrain occurrence and as such seems to spare these Indians and Natives from either such frontal lobe phenomenon as delirium tremens or such hepatic problems as cirrhosis. If one is looking at the alcohol and violence issues in a predominantly Alaskan Native community,

one cannot avoid understanding the role that the physiology of "blackout" plays in the perpetuation of this violence, as well as its patterns of occurrence.

Use of the MAST

Foulks' group used the Michigan Alcohol Screening Test (MAST) as their prime objective classification. This test, a 25-item questionnaire, had been developed in the Midwest by Seltzer in 1971 and was intended to be a self-reporting screening device for alcohol use. Seltzer's validative studies established that a score of zero to three is associated with normal social drinking, four is borderline, and a score of five or more indicates established alcoholism. In *Eskimo Capitalists* Foulks surveyed the world literature on the use of the MAST. His study, however, was the first use of the MAST on an Alaskan Native population. Thus, although the test had been normed for other populations as diverse as Ireland and Mexico, it had never been used for this present population. Foulks and colleagues compared their Barrow findings to these prior norms. They found that 72% of the population had a score of five or more. This had been achieved in only two prior studies where the subjects were drunk drivers. When compared to the prior populations, this measure certainly indicated a major skew in the direction of both alcohol and violence. Although these figures correlated in many ways with much of the behavior in Barrow, the figures lent themselves to overinterpretation by the press.

Despite this initial overinterpretation, the MAST data from the Barrow study have proved invaluable to provide baselines for further Alaskan studies. For example, our clinic has been gathering MAST data from diverse groups for the last five years. We have used data from Natives and non-Natives, rural screening of another village, student populations, and correction populations including 100 individuals (82 Native and 18 non-Native individuals) who have committed their crimes during an alcohol blackout. Foulks' data have held up on an item by item basis for each of the 25 questions, with the Native samples being very skewed from the "lower 48" norm and with the Native samples being almost identical to the "Barrow data."

The actual data from the MAST portion of the "Barrow Study" were but the first step in what might indeed prove to be both very solid research as well, as perhaps providing a specific device for predicting drinking patterns and violence in the "at risk" populations. The test would need to be renormed for this population, and viewed in relation to what is known about the physiology of blackout in the Native drinker.

Ramifications for Research

Foulks and his colleagues stepped into a quagmire that had very little to do with the specifics of their study. The release of the data to the press

in order to "shock the Inupiat into action" was clearly the undoing of this study. Indeed this exposure may have set back research on alcoholism in rural Alaska rather substantially; currently there is almost no scientific research on alcohol and Alaskan Natives being conducted. Our clinic's research has been conducted primarily through the criminal justice system, and it is only now—a decade later—that we are making some inroads into having access to non-correctional populations.

The alcohol consumption problems of Alaska, both rural and urban, have continued for another decade. The drinking has continued as has the violence both to self and others. It is only in the last two to three years that a number of communities are becoming ready to address this endemic public health issue. A number of communities including Barrow have utilized Alaska's "local option" law to vote themselves "damp," while a few others have voted themselves totally "dry." The communities that have engaged either option under the law have noted a marked decrease in violence and drinking. As a part of this movement, in communities where there has been some success in addressing the alcohol problem, there has been a resurgence of cultural integrity by both the elders and the young people. There is, however, still a great ambivalence about "going dry," as evidenced by the fact that in the last year, Kotzebue has had two public referenda on this subject and Bethel has had four such referenda.

Alaska as a "third world equivalent" has been catapulted into the 20th century with the same kind of disequilibrium that happens with any type of rapid social change. Alaska's cities have used these resources to develop a modern "lower 48" infrastructure, while its villages and their peoples suddenly have computers, cable TV, telephones, freezers, and transistor tape players, while desperately holding on to a culturally-relevant subsistence lifestyle. Free time, occupations, cash economies, and subsistence lifestyles impose themselves on the life of everyone in rural Alaska. This is coupled with a generally idiosyncratic response to alcohol by many of its Eskimo and Indian inhabitants.

Foulks' baseline research and even the attendant negative publicity may have inadvertently helped in starting the process of heightened awareness that is only now beginning to take form. Thus, a decade later, awareness at the state and local level clearly mandates a multi-pronged approach. The first prong deals specifically with the tolerance for alcohol and/or drunken behavior in a community, while the second prong deals with the longer term issues of recreating a meaningful and culturally-relevant lifestyle in village Alaska in the 1990s.

The evolution of the progress of these changes can indeed be aided and understood by careful research, but I would highlight Foulks' final caution that any researcher must not only "self-consciously" include sometimes intangible, value-laden factors into their research design and planning, but they must make them part of the central core of the process.

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RESEARCH OF STIGMATIZED CONDITIONS: DILEMMA FOR THE SOCIOCULTURAL PSYCHIATRIST

JOSEPH WESTERMEYER, M.D.

Over the last 23 years, I have had the privilege of researching several stigmatizing conditions in sociocultural context. These conditions have ranged from alcohol abuse and alcoholism, opium addiction and other drug abuse, to incest, depression, amok and other homicide, and psychosis in village settings. Groups in which these conditions have been studied have included the Hmong, Lao, and other ethnic groups in Asia, Chippewa and other tribal peoples in Minnesota, refugees, medical students, and physicians. Roles in these studies have at times varied from the "pure" research role, to various applied roles such as consultant, planner, evaluator, and administrator. Although some work has been done with my own resources or with research funding, other work has been under the auspices of numerous funding organizations; these include the U.S. Agency for International Development, the World Health Organization, the White House (Office of Drug Abuse Prevention), Congress of American Indian Affairs, United Sioux Tribes, the Indian Health Board of Minneapolis, and others (Westermeyer, 1971, 1972a, 1972b 1973, 1975, 1976, 1978, 1979, 1980, 1988; Westermeyer & Walzer, 1975; Westermeyer & Kroll, 1978; Westermeyer & Wintrob, 1979; Westermeyer & Peake, 1983; Westermeyer, Neider, & Vang, 1984; Westermeyer, Phaobtong, & Neider, 1988; Westermeyer & Hausman, 1974a, 1974b).

Against this background, it is especially gratifying to read and comment on Dr. Edward Foulks' paper. His experience in one case study mirrors familiar situations and problems from many of my efforts. His courage in relating these events, and his lucid analysis, serve the useful purpose of examining the social, political, and personal issues associated with this type of work.

Studying Stigmatized Conditions

Disorders of interest to sociocultural psychiatrist-researchers necessarily touch on matters of cultural pride, personal bias, and racial fear. Persons threatened by such work are not limited to those who profit from others' pain (e.g., bootleggers on reservations, corrupt officials), but often include

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"natural allies" as in this study (e.g., those in the same or allied disciplines). One never knows whence these personal and professional attacks will come, but come they certainly will if the matter being researched is of any consequence at all. The best defense is to maintain a high standard of scientific endeavor and ethics.

The charged political arena associated with these efforts is no reason to abandon them. Child rearing, community security, morbidity, mortality, and cultural survival can be (and often are) at stake. Ignoring these problems serves to perpetuate them. If researchers trained in psychopathology and social science do not address these problems, in most cases they will not be addressed.

He Who Pays the Piper Calls the Tune

In conducting pure research, we as researchers originate our hypotheses and methods. Then we proceed on our own time or with funds from a research foundation, fund, or institute. Political issues are limited to the funding sources, our research subjects, and ourselves. Confidentiality can be reasonably assured. The research question is the focal point, not the particulars of individual subjects or communities. It is this situation with which we as researchers are familiar from our training. Although this is not a simple situation, it is readily bounded and supported by our professional traditions and ethics.

Applied research not funded as pure research is not so readily guided by our traditions and ethics. The researcher must know what the funding source wants to accomplish, and how (or whether) research can contribute to that end. Acquisition of new knowledge alone is seldom the goal of the funding source, although it is often the researcher's central goal (or at least one of the major goals). Funding sources may not explicate their goals, or even be clearly self-aware regarding their goals or expectations for a research activity. Consequently, it is easy for the researcher to be duped—as this author once was by a well-meaning U.S. Senator wanting to benefit his constituency through a treatment project (Westermeyer & Hausman, 1974a).

Although the situation is difficult and full of pitfalls, it is not impossible. My own modest successes in applied research (after some bitter failures) have been based on following guiding principles:

- (1) Full prior knowledge of the goals being promulgated by the funding source; this may involve helping the source to clarify and write down their goals.
- (2) Negotiation and specification of the researcher's task and goals—which must necessarily differ in certain regards from those of administrators, agencies, local officials, and others.

- (3) Meetings with subjects, their association or leaders, and other interested parties to describe and discuss the proposed research; this can aid in fine-tuning the methodology as well as heading off future political problems.
- (4) Extensive background information and experience with the community, the individual players in the project, local factions that may polarize around the project, the culture or group being studied, and the topic or problem being researched.

These prerequisites—previously described in two papers (Westermeyer & Hausman, 1974a, 1974b)—not only facilitate a better research project, but also aid in avoiding political pitfalls.

Enraged Academicians, Administrators, and Advocates

As indicated above, if the project is important and the researcher has something important to say about the problem, someone (or more likely, several "ones") will be disturbed or even enraged. Someone's favorite ox is apt to be gored. If the arguments or objections focus on facts, alternate interpretations of findings, or publication of differing data, we as researchers must attend to them; such assaults concern the validity or integrity of our research work. More often these arguments and objections resemble the personal attacks encountered by Foulks: *ad hominem* arguments, libelous accusations against his credentials, and slanderous alterations of facts. When these occur, the researcher can be comforted by the fact that entrenched attitudes are being called into question by the research findings.

Rage at research findings may have personal origins. An administrator or academic may perceive the group under study to be "my Natives" or "my patients" and resent the intrusion; may have antipsychiatry biases due to personal or family problems; or may feel professionally threatened by the findings. If present, these personal matters are often widely and publicly known. Researchers under attack may feel a natural inclination to counterattack on a personal level. This must be avoided at all costs—as Foulks has correctly done in this report. Researchers under attack must stand with their research questions, methods, findings and interpretations. If this stance is exchanged for personal counterattacks, the researcher has lost sight of the task. In the heat of dispute, this focus on the research may seem to leave the "high ground" to those slinging accusations and personal affronts. With time, however, the correctness of proper scientific argumentation will become apparent—as it has in the Barrow affair.

Recommendation

We cannot eliminate these inherent sociopolitical problems from applied social psychiatric research. However, we can anticipate most of them, avert

some of them, and minimize others. Trainees should be alerted to these dilemmas and, during their early experiences, guided and supervised in dealing with them. Colleagues must be able to discuss, support, and critique each other's efforts in such projects—as does occur in the Society for the Study of Psychiatry and Culture, the Society for Applied Anthropology, and similar organizations. This article and its commentaries are part of such a process.

The misalliance of the well-known Barrow affair certainly has implications for American Indian and Alaska Native communities. However, the basic themes in such misalliances are relevant for numerous other populations: citizens of developing countries, tribal peoples in other countries, refugees, occupational groups, and demographic subgroups. This special issue of the *Journal* will no doubt benefit them and other peoples in addressing their stigmatized psychiatric conditions.

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TRANSCULTURAL RESEARCH RUN AMOK OR ARCTIC HYSTERIA?

JAMES H. SHORE, M.D.

I would like to begin my commentary with thanks to Dr. Edward Foulks, for his effort at self-disclosure and critique in this journal. From one perspective, Foulks presents us with an anatomy of a failure. It is my impression that he has written this review to inform American Indian and Alaska Native communities as well as mental health researchers in their future collaborations. He allows us to benefit from mistakes in the Barrow Alcohol Study that led to the serious consequences of misalliances. Obviously, this post-mortem examination is reported from the author's bias; this does not detract from the uniqueness of its disclosure.

First, a personal word about Foulks. He received his medical degree from McGill University with advanced training in child psychiatry and post-doctoral studies in anthropology. He has served as a faculty member at Hahnemann Medical College, the University of Alaska in Fairbanks, the University of Pennsylvania, and currently at Tulane University. His subspecialty interests have included administrative psychiatry, consultation-liaison psychiatry, individual psychotherapy, psychopharmacology, as well as transcultural psychiatry. Prior to the Barrow Alcohol Study, he had worked at the University of Fairbanks with Alaska Natives and written with sensitivity on clinical issues in transcultural psychiatry.

The design, implementation, and outcome of the Barrow Alcohol Study are reported in detail in his paper. Critics of the study have labeled it as a product of cultural imperialism. Ironically, one of the most vociferous critiques was a non-Native faculty member of the Inupiat University of the Arctic. This critic said,

The North Slope Borough assembly has been the victim of a sophisticated hoax aimed at destroying the credibility and integrity of the Inupiat people. The research on alcohol abuse and the news coverage was the most demeaning and reprehensible sham. Instead of using Winchester and Remington rifles to destroy your people in the culture as with the Indians in the 1880s, they bent words, numbers, and statistics to accomplish what was in effect a social and cultural genocide. These con artists hiding behind the

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guise of professionalism and religiosity, and acting as consultants to the North Slope Barrow have dealt a devastating blow to the Inupiat people and their cultural heritage.

A bitter criticism indeed. In fact, echoes of this accusation are common to non-Indian researchers. Vine Deloria (1969) classically attacked white cultural bias in *Custer Died For Your Sins, an Indian Manifesto*. Deloria carried the old adage of "an anthropologist in every hogan" to a bitter criticism of the blind application of white culture and its scientific/technological assault on the fabric of traditional native cultures.

Into each life, it is said, some rain must fall. Some people have had horoscopes, others take tips on the stock market. McNamara created the TFX and the Edsel. But Indians have been cursed above all other people. Indians have anthropologists. The origin of the anthropologist is hidden in the historical mists. Indians are certain all societies of the Near East had anthropologists at one time because all those societies are now defunct (p. 83).

Another criticism of the application of Western technology and transcultural behavioral science research is made by Marano in his 1982 article entitled "Windigo Psychosis: The Anatomy of an Etic-Etic Confusion." Marano states, "Windigo psychosis has been one of the most celebrated cultural bound syndromes of North American native people for almost half a century" (pp. 385). This classic example of 'Culture bound psychopathology' involved delusional cannibalism or murder to satisfy an obsessional craving for human flesh. The capacity of this syndrome to inspire theorization in anthropology and the related disciplines seems inexhaustible. Marano reviewed the voluminous Windigo literature and concluded that, although aspects of the Windigo belief complex may have had components in some individual's psychological dysfunction, there probably never were any Windigo psychotics. He hypothesized that Windigo psychosis is an etic/behavioral form of anthropophagy and is an artifact of the search conducted with an emic/mental bias. The Barrow Alcohol Study examined a common and prevalent psychiatric disorder, alcohol abuse and dependence. It is not a "culture bound syndrome." However, Marano's criticism of researchers with etic/emic confusion was also leveled at Foulks and his colleagues.

Foulks reports on a tragic misalliance between the researchers and various community and Native groups. A list of those groups include the Center for Research on the Acts of Man, the Inupiat of Barrow, the community's Alcohol Detoxification Center, community churches, the North Slope Borough, a Seattle consulting firm, the Kennedy School at Harvard

University, the Barrow Department of Public Safety, a community steering committee, a technical advisory group, coordinators of records (from public safety, court, hospital, administrative, and fiscal reports), the Department of Health, a faculty member of the Inupiat University of the Arctic, the University of Pennsylvania, the *New York Times*, and the Fifth International Symposium on Circumpolar Health in Copenhagen, Sweden. Any researcher who began a field research collaboration with this number and diversity of cultural groups would immediately anticipate significant problems and unavoidable conflicts. This diversity became a significant difficulty for a sensitive adaptation of epidemiology field research methods. It would have been a problem in any setting, especially Indian and Native communities that have not only traditional but multiple non-Indian agencies and Indian groups at various levels of acculturation change.

Perhaps some simplistic guidelines would be of practical value to future researchers. "Keep it simple." "Don't attempt to be all things to all people." Advances in knowledge often do not have immediate practical benefit for a local community. Sometimes research data are unusable or negative. This may be unacceptable in certain cultural-political contexts. The goals of Foulks and his colleagues included the application of a precise research methodology, a useful scientific outcome, education of community groups, consultation with selected agencies, dissemination of the information, consensus building, and participation with the community for effective data utilization. The breadth of these goals, while laudable, certainly contributed to the misalliances, to the negative outcome, and the destructiveness of the process.

In his academic critique of the study, Foulks has identified major critical variables. A basic discontinuity was the inherent conflict between the perspective of the non-Indian technical advisory group and the Native steering committee. A major goal of the research team was to reach a consensus between the Inupiat and the non-Native agency personnel of the community. Foulks refers to this goal in his paper saying, "It was difficult for the research team through the polemic to locate the voice of the Inupiat." While I share the goal as an idealistic one, the complexities of communicating technical epidemiological information to service providers, community administrators, and community members is rarely if ever accomplished in the same effort. In fact, the generation of resources for new programs and community change usually follows some years later, with the mobilization of a broader political network that diffuses focus on a particular community.

Guidelines for Release of Information to the Press

The Achilles heel of this project appears to be in the complex decision-making process that led to a press release. What were the motivating factors? Were they researcher ego, community education, national education, program development, resource development, and/or a spontaneous

outcome with the open community meetings? For transcultural researchers working with Indian and Alaska Native communities, a press release is usually the kiss of death. Nevertheless, there are times when it will occur. In those instances certain guidelines may be helpful.

- (1) The release always should be supported and initiated by the tribal or village government.
- (2) Research findings should be reported as generalizations.
- (3) The identity of small local communities should remain confidential.
- (4) The emphasis should focus on positive aspects of the finding and decrease the taboo traditionally associated with high reported prevalence rates for mental illness, especially alcoholism and/or suicide.

There is no doubt that our government works by creating special pressures on and by vested interests for a short-term increase of public funds. The classical example in Indian country is the press release in the 1960s of the Indian adolescent suicide epidemics from the Northern Plains to justify funding for the National Institute of Mental Health Suicide Prevention Center. Indian adolescent suicides are a serious public health issue. Some of those new funds benefited Indian communities. However, the publicity also contributed to the public's negative stereotype of "the suicidal Indian," a self-image incorporated by many Indian tribes regardless of their suicide rates.

Essentials for Psychiatric Research

In 1977 I published a chapter on "Psychiatric Research Issues with American Indians" (Shore, 1977). Based on my review of the Barrow Alcohol Study, I have expanded my original six criteria to ten essentials. The ten essentials for psychiatric research with American Indians and Alaska Natives are as follows:

- (1) Planning for mental health projects should begin with collaboration between the social scientist and the Indian community.
- (2) The focus for particular research projects should be compatible with local priorities of the tribal council or community health committee.
- (3) In the research design and selection of a particular methodology, consideration should be given to the relevance of the outcome for use by the Indian group.
- (4) The methodology should be realistically conceived, and limited in its focus and project goals. It should be a practical method for field application in a transcultural setting.

- (5) The project should be implemented in a local community partnership with an attempt to employ Indian staff whenever possible.
- (6) An agreement should involve sharing the research findings with the local community in a manner that would maximize relevance for program planning.
- (7) If human subjects are involved, patient rights must be protected.
- (8) Community confidentiality must be preserved, just as individual patient confidentiality is protected.
- (9) A press release should be avoided.
- (10) Be realistic about the short-term, practical application of research data.

Foulks ends his paper with the following statement. "Hopefully, our experience will provide an example and a lesson demonstrating the degree to which the questions and methods of the times, and how scientists must self-consciously include the sometimes intangible, value laden factors into their research design and planning." His openness is educational for us, and he is to be commended. The critical factors affecting Indian and Native health, morbidity, and mortality, are significantly influenced if not caused by behavioral factors. To advance public health goals, applied field research must continue with full awareness of and sensitivity to the risks of imposed value systems, cultural imperialism, scapegoating, distortions, and misapplication of research findings. Perhaps, we can go forward with greater insight from the Barrow Alcohol Study to create effective alliances across cultural boundaries to study and promote the mental health and well-being of American Indians and Alaska Natives.

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A COMMUNITY SYSTEMS APPROACH TO RESEARCH STRATEGIES

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There are a number of conflicting conceptual models dealing with possible causes of alcohol problems, and with what interventions might be helpful (Bany, 1988). These include among others: medical models, with concepts such as "the sick role," genetic predispositions, metabolic factors, structural defects, and medical treatments for symptom relief; behavioral models, with concepts such as reinforcement, learning, modeling, and interventions based on aversion, extinction, education, skill training; psychological models, with concepts such as unconscious problems, denial, character disorder, the "self-medication" hypothesis, "dual diagnosis," and interventions such as psychotherapy, group therapy; socio-cultural models, with concepts such as enabling, co-dependence, acculturation stress, and interventions such as family therapy, Adult Children of Alcoholics programs, environmental change, social detoxification, cultural treatments; recovery models, with concepts of compulsion, loss of control, progression, spirituality, and treatment based on self-help, 12-step programs, and sponsorship.

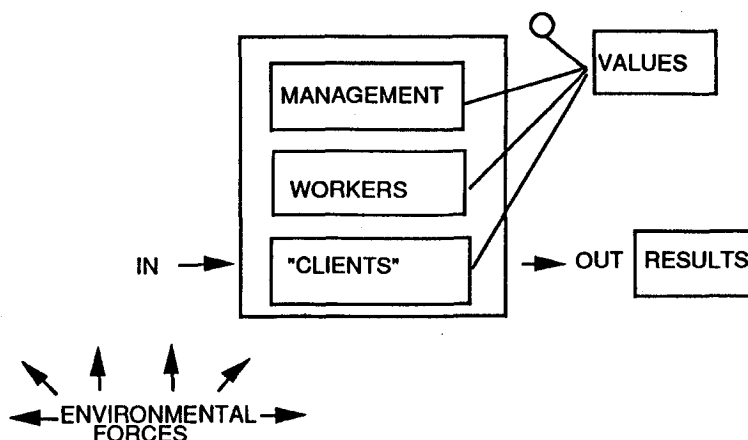
Because of this proliferation of competing theories and approaches, "misalliance of models" is not that uncommon in alcohol studies. When one then tries to go a step further and carry out an alcohol study in a small rural community like Barrow, problems are multiplied. Rural communities in many parts of Alaska, as well as other developing areas, are having to cope with extremely rapid change and multi-system problems, and there may be no clear consensus coming from within the community about what to do. Traditional elders may have one approach, law enforcement officials another, ministers another, public health officials another, and local Alcoholics Anonymous groups still another. A researcher beginning with what seems a simple question can rapidly come to feel "enmeshed" in an overwhelming set of interlocking and complex social, economic, political, and legal problems, with strongly held and conflicting feelings and opinion on all sides.

It is so common to have "misalliances" under these conditions that I would say a good portion of my job involves "shuttle diplomacy" to help resolve conflicts between "outsiders" and local people trying to work on alcohol/mental health projects together.

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A Systems Viewpoint

A general method to analyze these sorts of situations and identify potential intervention points can be helpful. The approach I use takes a systems viewpoint. It emphasizes careful definition of what the problem is, who is going to carry out the project, and what the end results are supposed to be. Sensitivity to possible alternative "world views" of participants, management issues, and identification of forces outside the immediate scope of the project that may impact on it are also considered. Being a visual thinker, I might use a diagram like the following to help keep track of these various aspects.



As can be seen, in this case we have a "client" coming into a system, having a problem of some type assessed and handled by a "worker," and then leaving with—hopefully—some measurable results. The system has a management. The participants both inside and outside the system have "world views." Environmental forces outside the system exist which may act as constraints or impact on system operations.

Diagrams of this type can, of course, be modified depending on the particular system being analyzed. "Exploded views" to graphically represent the processes at work in various "subsystems" can also be used. Diagrams of one's own and other "conceptual models" can also be informative. Like a football coach charting a play for the team, researchers may find "a picture is worth a thousand words" in describing interactions and clarifying dynamics.

Helpful discussions of this style of systems analysis and strategic thinking are given by Chuckland (1981) and by Wilson (1984).

When I attempt to develop a flow chart to help understand exactly what happened in the Barrow situation, I come up with some questions. These include:

1. *Were the "clients" selected for the study the best ones?*

The "identified patient" may not always prove to be the real problem. In the case of alcohol studies, it can be extremely difficult to know exactly how to define the problem, or even who has the problem.

One could consider the problem to be individuals with alcoholism, dysfunctional family systems, community breakdown, interactions with outside cultural groups, or various other alternatives.

Special care is needed if, as in the Barrow case, there are potentially significant differences between the subjects being studied (the "clients"), the persons carrying out the study (the "workers"), and the individuals funding the study ("management"). Differing perceptions of what the problem is, who has the problem, and what should be done about it can be anticipated.

It seems there could have been more involvement of the various community factions at the "front end" of the project in deciding exactly who or what was the problem needing study.

2. *Were the "workers" actually carrying out the project the right ones?*

In the Barrow case, some of the "agents" involved in carrying out the project were in Seattle and Philadelphia. Although there was a local "Steering Committee" and "Technical Advisory Group" built into the research design, it appears that the Center for Research in Philadelphia actually did quite a bit of the work, which the local groups then "reacted" to.

Especially on a project like this, where there are language and cultural differences, factions within the community, intergenerational differences, and a value-laden problem such as alcohol abuse, my approach would be to push for local people to do most of the work. Steering Committees and Advisory Groups might need training about possible ways to proceed, and local research associates might need to be developed. This can take time, but seems preferable to the researcher doing the project without sufficient community involvement, and then having the community "shoot the bearer of bad news" or accuse one of being "imperialistic," etc.

3. *What were the end results really supposed to be?*

Targets in the case of the Barrow project appear to have been unclear. Try, for example to draw a diagram of what will be happening once the Inupiat are "shocked into action." Assume the project will be a smashing success, and you are now five to 10 years into the future. Can you represent what has happened to the clients? What percentage have stopped drinking and are out of jail, and what other benefits have accrued? If you are unclear, other participants in the project are probably also confused, and may not perceive the value of the project or be very cooperative.

My own approach is to involve the community in extensive dialogue about desired results, as well as the format of final reports, and how they will be used, at the beginning of a project. I might ask them for diagrams as well as develop my own, to get very concrete about exactly what they want to achieve and what "success indicators," they will use. I assume people want to know "what's in it for them," and that data for them to measure progress towards those results will need to be collected as part of the project.

When one wants to "involve the community," there are often problems as to who really speaks for the community. When there is lack of consensus, I attempt to give special weight to what the traditional elders recommend. They may be behind the scenes and not in formal leadership positions and can sometimes be difficult for "outsiders" to engage in the type of dialogue needed for project planning. However, they can be extremely influential in determining whether other community members will really support the project. Ministers, public safety officers, health department officials, etc., may also have valuable input, but if this was not matching what I was hearing directly from the elders, I would be concerned as to why, and try for even more in-depth discussions with the elders to find out what they thought was going on.

I would even go so far as to suggest, if the elders were not apparently much of a presence in the community, that the researcher consider very careful explorations as to why this had happened, and clarification as to how important, "value-laden" decisions formerly made by elders were now made in the community. As explained in the next section, I operate from a "values model," and a problem like alcoholic behavior, being a value-laden problem, is expected to generate conflict over values. To avoid getting "caught in the middle" between various value positions, if elders are no longer actively carrying out their traditional roles as decision-makers, research projects on this topic need to figure out what decision criteria will be used by the community to determine whether a proposed project will be supported and acted on.

4. What were the world views/value orientations and perceived values of the participants?

World view deals with very basic ways of perceiving time, space, and knowledge. These can be quite different in various individuals as well as in various cultural groups. Value orientations stemming from these differences have been described (Spiegel, 1982). Perceived values are closer to surface awareness, but likewise can be different among individuals and in different cultural groups.

Researchers need awareness of their own world views and value orientations, to the degree possible, and how these might differ from participants in the project. For example, the approach outlined above of working with community groups to determine end results, and then helping them measure whether they are getting there, reflects certain basic

orientations of my own. It is very much "future-directed," and action-oriented ("doing"), but values group participation rather than individual solutions.

I am well aware that other participants in a project may not share these basic orientations. They may well be more present or past-oriented, "be-ers" rather than "doers," and favor individualistic rather than group solutions. I am therefore prepared to spend time with potential participants in the project to try to learn their own orientations and determine whether we will be able to work together. I am willing to take a pretty active role in "marketing" my own value orientations, and do not consider myself at all a "blank screen" or completely objective reporter. I might take another approach with other types of "research," but here we are talking about community-based, "action research," which is not a neutral or value-free endeavor. The key to not triggering resistance with this active type of approach is to be very sensitive to the orientations of the other participants in the project, respect their positions, and include them as "co-investigators" in designing the strategy and structure for the research.

To influence project participants to work together effectively, I also find a "values model" to be helpful (Dwyer, 1988). This model proposes that if you are to successfully influence others, what is critical is their perceptions of how the behavior you want them to do relates to their personal values. They will be weighing five factors. They will be assessing whether they can do what you want done, whether there is possible value satisfaction in it for them, how probable it is they will receive that satisfaction, what the costs are, and what the risks are that other value satisfaction currently held might be threatened or lost.

The implication of this model is that when research projects meet resistance, the researcher needs sensitivity to the influencee's perceived values, and needs to consider how these perceptions might be altered.

5. Who was managing the project?

The paper does not describe in much detail who was really calling the shots on this project, or how the management processes were carried out. How the project will be represented to the media is to me a management issue that needs to be carefully planned out. It appears that the State Office of Alcohol and Drug Abuse was involved as a funding source, with a grant to the North Slope Borough, who then contracted to a group in Seattle, who then subcontracted to the group in Philadelphia.

Funding agencies often set the rules or constraints on how monies can be spent. Each intermediary the funding passes through tends to layer on their own agendas and requirements. Sometimes these are negotiable and sometimes not. But if not given careful attention, it can soon be very unclear who the researchers are really working for. Even a project that has done the initial steps of getting the community to define the problem and the desired results precisely can be sabotaged by people at higher levels.

My own approach on this project would have been to encourage funds going directly to the local Inupiat elders, who might well have participated

more actively in the project if they felt more "ownership" by having more direct control over every aspect of the project, including the funding. Training needs, language barriers, distrust on the part of funding agencies that the elders could do the project, or other problems resulting from this approach would have had to be anticipated and dealt with.

6. *What was happening in the environment that might have impacted on the project?*

It appears that factors outside of the immediate concerns of the project might well have been important. The community of Barrow was involved around the time of this study in obtaining financing from "Wall Street" for various community projects, and in negotiating with oil companies on "local hire" provisions in labor contracts. Studies documenting a high level of alcohol problems and appearing in the *New York Times* and other newspapers, could well cost interest points on financing offered for community projects, and mean decreased ability to negotiate labor contracts. People might say, "Why should we lend money to or hire people who seem to have a lot of alcohol problems?" The community could be badly hurt economically by release of information creating a negative impression, and non-drinking community members could be penalized in the job market.

There is an issue of "academic freedom" here, so that the researchers may understandably have had concerns about local politics influencing how their results were reported. But there is also an issue of how to protect the rights of the local people, who in this case were highly vulnerable because of larger scale politics involving outside oil interests.

Summary

Conflict management skills are needed when dealing with research projects, in multi-cultural settings. Alcohol projects are especially prone to conflict because of the variety of different models of causation and intervention.

There are a number of theories of conflict and type of conflict management approaches. The role of people who are in "cultural broker" or interface roles between conflicting parties has been reviewed elsewhere (Spiegel, 1973).

My own approach—one of a number of possible alternatives—is briefly outlined here. It involves a form of systems analysis, and a role that many researchers might not initially feel comfortable with. The role goes beyond that of the "objective scientific reporter," and even beyond that of "conflict manager" or "culture broker." I am not sure quite what to call it: there are active attempts to get community members to resolve conflicts and carry out projects, but also what I consider a research orientation to try to carefully observe and describe and measure what is done. It is, I suppose, a form of "participant-observation," but the participation involves developing an overall strategic approach to the community's perceived problems.

There is careful attention to who gets defined as the "problem," and who will actually be carrying out the project. Local community factions may need

assistance in developing a very clear vision of the targeted results for the project. One has to also be keenly aware of possible problems that can result from differences in world view, pressures from funding sources or other intermediary groups who may have their own agendas for the research, as well as the overall context or environmental constraints. Key information that will shape the overall research strategic design needs to be gathered in collaboration with the community, but may not even be labeled as "research," even though each project is basically an experiment which attempts to describe and evaluate results.

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The comments in this article are solely those of the author, Dr. William Richards, and not necessarily those of the Alaska Area Native Health Service.

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MALFEASANCE AND FOIBLES OF
THE RESEARCH SPONSOR
damnant quod non intelligent

JOSEPH E. TRIMBLE, Ph.D.

Ten years have elapsed since the preliminary findings of the now infamous and legendary Barrow Alcohol Study were released to the scientific and public communities, respectively. In the course of the past decade, numerous cross-cultural researchers and Native community activists have held up the study directed by Professor Edward Foulks as an example of what happens when community-sponsored research violates certain ethical practices. The study and the ongoing flow of debate also are held up as an example of what happens when the values, beliefs, and lifestyle preferences of a Native community are not given careful and due consideration. Throughout the debates, however, Foulks has displayed a willingness to listen and respond to the heated complaints. Indeed, he appears to recognize that something did go astray, and seems willing to both admit the mistakes of his team and assure others that he will profit from the criticisms. To this end, I applaud his admission of error and willingness to further subject his work to the scrutiny of the researchers represented in this journal. If Foulks had excused himself from the occasion presented by the editor of this journal, I believe it would have only served to reify the many accusations tossed in the direction of the study.

In reviewing Foulks' article and a few of his earlier publications concerning the Barrow Alcohol Study I must admit that I, too, could join the ranks of the hard-line critics. However, others in this journal have addressed the procedural, methodological, and statistical flaws in the study; hence, I will restrain myself.

Responsibilities of Research Sponsors

In his paper, Foulks points out "that the mistakes experienced in this research study were less those of scientific methodology than of social and political *naivete* (my emphasis) regarding the people of the community studied." To an extent, and based on my experiences, observations concerning community-sponsored action research, and my careful reading and assessment of the scenario surrounding the Barrow Alcohol Study, I believe Foulks touches on a lively yet often overlooked point: many community planners and action research sponsors are indeed ignorant of the strict rules

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governing scientific research, standards, and ethical policies we are obliged to follow. More often than not, action research sponsors appear to lie in wait, ready to tear into a draft of a research report armed with little understanding of the general requirements of the scientific method and professional report writing. In short, many sponsors are almost too quick to condemn what they do not understand—*damnatio quod non intelligit*. In the shadow of my contention I would argue that community action research sponsors have a responsibility to understand and appreciate what they will receive when they contract out a scientific venture. Above all else, they should expect a product to be empirically oriented, and conducted and written in a straightforward objective manner. In the main I believe Intersect and Dr. Foulks and his research team delivered such a product to the contractor, the North Slope Borough.

Let me stay with this point for a moment. Several critics, most notably the Director of the Department of Health and "other *non-Native* (my emphasis) professionals of the Technical Advisory Group" found the report to be ambiguous and verbose, difficult to read and lacking in precision and authoritative tone. In my opinion, the report was none of these things. If the reviewers expected a precise and authoritative report, it was their responsibility in the first place to lay out their expectations, standards, and level of sophistication in the initial contractual agreement.

Continuing this point, I was somewhat mystified to learn that the non-Native critics were quick to point out that the report had imposed Western, "lower 48" standards on Inupiat society and should have been written to better reflect Inupiat values and attitudes. I am not sure that I understand what "Western, 'lower 48' standards" really implies. If the non-Native critics are focusing on Foulks' professional scientific report writing style than they are way off base; in effect, they are criticizing the standards established by science itself, and none of them have the credentials nor the expertise to offer that criticism. Second, their criticism is certainly ethnocentric and in no way does justice to the very extraordinary heterocultural makeup of those who reside in the "lower 48." Third, how can a technical report describing the distribution, consumption, and abuse of alcohol in Barrow be written to reflect Inupiat values and attitudes? As I read the contractual agreement between the North Slope Borough and Intercept, nowhere can I find a scope of work statement that calls for an ethnographic analysis of Native values and attitudes. Moreover, and more to the point, a description of summary descriptive statistics can only be presented in the manner described by Foulks.

A related but significant line of criticism was offered by a non-Native (again) faculty member at the Inupiat University of the Arctic. His attack claimed that the report was ethnocentric, parochial, demeaning, and denigrating to the Inupiat; he even charged that the methodology of the study was flawed because the researchers were not conversant in Inupiaq. I find the statement concerning the research staff's lack of fluency in Inupiaq

somewhat absurd and nonsensical. From what I can gather from the requirements of the agreement, Intersect and Foulks were not required to be fluent in Inupiaq nor was any part of their product required to be translated into the Native dialect. Frankly, I seriously doubt if any of the non-Natives responsible for initiating and monitoring the project are themselves totally fluent in Inupiaq and completely understand Inupiaq values and attitudes. In many ways, it sounds as though some of the non-Natives most vocal and active in their criticisms of the Barrow Alcohol Study were terribly patronizing and overly paternalistic. What and whom were they protecting? After all, the non-Natives were largely responsible for setting up the study and providing details concerning the project's scope of work. In addition, I am invariably suspicious of non-Natives who reside as a minority in a Native community and who zealously take pot shots and cast unsubstantiated, inflammatory remarks toward outsiders especially those hired ostensibly to document what Natives already know to be factual. Moreover, I am doubly suspicious and usually intolerant of local non-Native "experts" who find it necessary to come to the rescue of "their people...their informants"—usually without the consent of traditional leaders.

Absence of Native Commentary

In the course of reading through the reports of the Barrow Alcohol Study, I looked for negative criticisms and comments from Inupiaq traditional leaders and Natives with advanced academic degrees. The absence of any Native commentary was either a) a deliberate omission or an oversight by Foulks and his colleagues; b) eliminated by non-Natives because it served no useful purpose or it wasn't strong enough; c) never solicited in any manner whatsoever; d) not documented throughout the course of the public meetings of the advisory committees; or e) due to the efforts of non-Natives to cover the mistakes they made in initiating the study in the first place. Whatever the reason, I would be eager and delighted to read comments prepared by the Inupiaq leaders, community activists, and planners to learn about their insights and concerns. After all, since the study was directed toward them and involved their perceptions of the "alcohol problem," they in fact are the real owners of the data. As such, the Inupiaq people of Barrow should have the final say in this whole scenario.

Use of an "Outside" Research Firm

I have some concerns about the organization of the study and the fact that a contract was offered to Intersect, presumably a non-Native research group based in Seattle. If the North Slope Borough administrators were so adamantly concerned about the cultural integrity of the study, why did not they contract with a Native research firm or at least a cadre of researchers tied into a Native studies program at the University of Alaska in Fairbanks

or Anchorage? Better yet, why did not they solicit the expertise of researchers at the Inupiat University of the Arctic, especially those who seem to know so much about Inupiaq values, attitudes and general lifestyle? Moreover, if the administrators recognized that research expertise was available through the Center for Alcohol Addiction Studies at the University of Alaska at Anchorage, why did not they solicit the Center to assist in the project? It seems to me that if there were any concerns about "cultural imperialism" those concerns could have been easily and almost effortlessly tended to by involving the appropriate Native group and cultural brokers before the study was conducted or when the contract was awarded.

From my reading, and based on my observations of the whole Barrow Alcohol Study, the North Slope Borough administrators must share the responsibility for the accusations and inflammatory remarks directed toward Foulks and Intersect. They initiated the study, signed off on the contract with Intersect, participated in the planning and conduct of the research—and then turned on Intersect and Foulks, apparently to absolve themselves of any wrongdoing or responsibility for the research. Collectively, their ethical standards and judgement should be dutifully challenged and accordingly censored and reprimanded.

Release and Ownership of Data

In addition to the actions taken by the North Slope Borough administrators connected to the study, what troubles me most about the whole scenario is the decision to release the study's findings to the press. I believe the responsibility for this decision rests with Intersect and Foulks' research group. As I understand contract research, the study's products, whatever they may be, are the property of the contractor, the North Slope Borough. The Borough should have reserved the right to decide on the extent to which the study's findings were released to the public. I even take the position that the property of the study, especially the findings, belongs with the respondents, the Inupiaq people of Barrow (Trimble, 1977; Trimble, 1988). They or their representative should have had the last word on the distribution of the findings. This is true even for findings published in professional journals. Research participants have rights of consent. Under the ethical principles which govern research, researchers must, above all else, maintain standards which protect the anonymity and confidentiality of respondents.

The fact that the results of the study were released to the press and the fact that the results were used for Foulks' professional advancement is most disturbing to me, especially when Foulks did not have permission to use the data generated in the study. I repeat, *research respondents own the data*. Let me use this occasion to remind readers of our responsibilities as researchers. Far too many seemingly well-intended cross-cultural researchers have shown little regard for the question concerning data ownership. In response to these concerns, the American Psychological

Association convened a task force to establish Advisory Principles for Ethical Consideration in the Conduct of Cross-Cultural Research (Tapp, Kelman, Triandis, Wrightsman & Coelho, 1974). The principles developed by the task force expanded upon the established ethical principles developed by the American Psychological Association to include: "(1) the inherent ethical acceptability of the actions undertaken in the research; (2) respect for the host culture; (3) open communication; (4) respect for subjects' rights; (5) protection of subjects' welfare and dignity; and (6) benefit to the participants" (Warwick, 1980, p. 361). To this list I would add that the "responsibility for the establishment and maintenance of acceptable ethical practice in research always remains with the individual investigator." The investigator is also responsible for the ethical treatment of research participants by collaborators, assistants, students, and employees, *all of whom incur parallel obligations* (my emphasis) (American Psychological Association, 1973, p. 22). In short, all who in any way were responsible for the conduct of the Barrow Alcohol Study stepped on and violated fundamental ethical principles.

Issues of Confidentiality

The world now knows who was a respondent in the study, not by individual name but by community of origin. Consider that the population of Barrow in 1980 was close to 2,300 people of which some 78% were Native (American Indian and Aleut as well). Consequently it would not take much effort to single out who likely participated in the study.

Given the circumstances surrounding the Barrow Alcohol Study, I wonder why the collective voice of the Inupiaq in Barrow were not more vocal concerning the violation of their rights as human participants. Could it be that the participants were not made aware of those rights? If that is the case, they then as well as now have a recourse which indeed would create an embarrassment to the research team should they opt to pursue it. I leave this disturbing possibility to those who maintain a surveillance of the ethical conduct of psychiatrists and the disciplines represented by those who were a part of the Barrow Alcohol Study.

Community Repercussions

In closing out my comments, I feel compelled to add a few more points. I was not able to obtain all of the correspondence and published materials concerning the activities surrounding the Barrow Alcohol Study. Hence some of my comments may misrepresent the facts. If I am in error and overstated or was inaccurate in my interpretations then I too stand accused. To those who are offended by my inaccuracies, I extend deep apologies. Foulks and members of Intersect and the research team should know that several people I contacted in Alaska were simply not willing to discuss the

project any further and wished that the publicity would die down. Frankly, I do not blame them for that position and would probably take a similar position if I were a victim of such circumstances.

There is one more disturbing point. The study's results were intended to serve as a justification for initiating an intensive alcohol prevention and intervention program in Barrow. According to one of my informants, no such program was implemented even after the controversy settled down. Sadly enough the incidence levels of alcohol use and abuse in Barrow are still at the levels reported by Foulks. Moreover, the incidence of marijuana use and abuse is reportedly running a close second to alcohol abuse in the community. Both findings are tragic and unconscionable considering the intent of the original Barrow Alcohol Study.

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THE COMMUNITY AS INFORMANT OR COLLABORATOR?

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Reflection on the Barrow Alcohol Study is important, given the continuing problems associated with behavioral health among Alaska Natives and the desire among leaders of Native and non-Native communities in Alaska to ameliorate the situation (Hensley, 1987; GICCY, 1988; Mohatt, McDiramid & Montoya, 1988; Booker, 1988). The assumptions are that research can provide valuable insights into etiology, prevention, treatment, and epidemiology of behavioral health problems and that such research can guide policy and practice to improve the quality of life for Alaska Natives. However, research within Alaska Native communities will never influence policy and practice if it cannot take place because of distrust engendered by previous research. Foulks (1980) indicates many local scholars believe "that future social research in North Alaska would be jeopardized" if research produces a widespread lack of credibility within a community.

I will analyze Foulks' reply by applying a set of parameters for community-based research from a collaborative and participatory research framework. These principles will point out deficiencies in the analysis of the original research. My analysis is based on the conviction that research must build the capacity of the studied communities to become the researchers of the needs of their own communities. Social research as process has the power to increase the independence and self-determination of the community: individuals become more conscious of themselves, and the community more clearly can differentiate its problems, their causes, and pathways to amelioration. The community can learn of its strengths and define and face its problems, thus becoming empowered and strengthened. However, this process of empowerment can only occur in a participatory research framework in which the research team includes both the experts who are trained in methodology and members of the community. The team becomes a carefully formed partnership based on mutual respect and collegiality. Through such a process, outside researchers may actually achieve the goals of the research and prevent the alienation between research and the community that Foulks describes so well in his article.

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The Positivist Paradigm

Given these assumptions, I understand the statement of Foulks (1980) when he indicates, "We believe that the mistakes experienced in this research study were less those of scientific methodology than of social and political naivete regarding the people of the community studies." My analysis is that a mistake of scientific methodology caused his team to become and remain naive and resulted in a report with little credibility toward which much hostility was and continues to be felt. The scientific model utilized is based on the assumption of an expert studying the problems of indigenous non-experts in which the latter serves as the object of the research. Additionally, the indigenous people assist in two areas: garnering "better insight into the community's beliefs regarding the nature of 'the problem,' and...[ensuring] more total private community participation in deciding how the results of the study were to be used." (abstract) The scientist is involved in the design and execution of the study as a technical enterprise.

St. Denis (1988) utilizing Smith (1983) and Reason & Rowan (1981) speaks of this type of research as the positivist paradigm or the old paradigm research in contrast to the post-positivist and new paradigm research models. She speaks of the following three aspects of this positivist paradigm that seem to describe the model exemplified in the Foulks' article: 1) "social facts are treated as things external to the individual" (to which I would add "and the community"); 2) "social investigation is a neutral activity;" and 3) social science provides a basis for "social engineering to improve society."

A Participatory Model

In contrast, the participatory model involves the community and the scientists as colleagues in an on-going process beginning with the setting of parameters and concluding with the interpretation of data. It is based on the assumption that social reality is precisely social and, therefore, one cannot separate social facts from context. In terms of research, the contextualizing must involve the people of the community in all phases of the research. All research is value-laden and most particularly social research. Without continual involvement of the community in which the study occurs, the research is more likely to develop a methodology which is ill-suited to its context. Finally, engineering of social reality implies a degree of control which is inconsistent with the self-determination of communities.

I will now indicate some of the key parameters of participatory research and their application to this study. My analysis is based on the original Klausner & Foulks (1980) report, other local historical documents, and the latest retrospective analysis.

Parameter one. The community must set the ground rules for the research, including defining the purpose of the study, who controls the data,

its uses, and the forums in which it will be reported. The setting of research ground roles should occur prior to the inception of the study.

Who is the community, one might ask? In the Foulks' article this logically would have been the representative group from the borough serving from the beginning as a guiding group. It should have been heavily represented by Native residents broadly constituted in terms of age, sex, and political distribution within the community. To have had separate technical and elders groups seems to me to be problematic. It established the hiatus between expert and non-expert and created distance between the community and the research process, thus increasing the inevitability of alienation. The research team was in the difficult position of being a secondary contractor and, therefore, initially should have developed a participatory structure and framework in order for the research to be guided by the community. Foulks (1980) discussed his involvement with the steering committee, their representation from the older generation, and his perceived need to involve other age groups in order to learn "more about their attitudes..." What I am suggesting is that the assumption guiding the research must shift from simply using "committees" to learn more to a process of creating partnerships and collegialships for the research. What Foulks seems to have done is create collective informants among the community with whom the team cross-checked their work.

Although it is clear that the community had great concern about the accuracy of the data and the implications of its interpretation for their reputation and sense of well-being, the researchers reported and continue to report the study in scholarly forums. It is important in collaborative research to insure from the beginning that agreement exists on the rights of the researchers to report their study. The contractors have the right to indicate the forums in which they wish and do not wish the research to be reported as well as who will own the data once the study is completed. If the research team does not agree with the limitations in this area, they do not have to do the research. No discussion was made of this issue in either article, although I am sure it has been a major concern of North Slope leaders during and subsequent to the study. It also raises a serious ethical question when required permissions did not seem to be acquired.

Although one does not know in detail what happened in the initial meetings reported by Foulks (1980) to have occurred in July, they could have initiated a careful analysis of what Intersect meant by "social and cultural relations of the population of Barrow, Alaska...[and]...wider societal and economic and governmental organizations" and whether, in fact, the Borough leadership and community not only had similar interpretations but, in fact, wished the team to define the question in this particular manner. It seems that this did not happen. The structure for the research defined quite independent roles for the research team, technical group, and steering committee. Additionally, it seems to this researcher that the process of only

meeting with both technical group and with the steering committee to modify the report represents critical errors in methodology.

Parameter two. The research team includes technical researchers, a broadly constituted steering committee, and local research colleagues and workers. The team guides the design of the study and methodology, the instruments used, the questions asked, the interviews performed, the analysis of data, and the development of recommendations from the data.

In this model the group would meet throughout the process to determine and monitor the specifics of the research, its implementation within the community, any explanations provided to the community, and the reporting of its results. In this context, the criticism of the local faculty member that the group had only "a superficial understanding" should not occur.

In this case and at this point, such a group would have either included or not included outsiders, Natives, and ancestry as variables to study. The outside research group would not simply hear that concern and make its decision, but would instead reach consensus with the community committee on what and who to include in the study.

Additionally, they would decide when and to whom to release the report. I found the discussion of why Intersect and the team released the report to the press to be indefensible and disturbing. Did the community want to be shocked? Does such shock typically lead to—openness or defensiveness—when it comes from a report of any group, particularly a non-Native group? It is clear that Intersect, the team, and the Director of Public Safety wanted the report to stimulate action. Unfortunately, they seem to have placed themselves in a position in which enough distance existed between them and the community that they could actually believe that press releases would help "galvanize solutions" (Foulks, 1980). "The proof is in the pudding" as the saying goes. I found this article unconscionable.

There is no discussion of the use and training of local research interviewers or data analyzers in the Foulks' article. It would seem that the outside team with Intersect did the work, developed the report, and attempted to begin the process of eliciting comments and recommendations about the data. If, in fact, no one from the indigenous community participated as a researcher and no conflict occurred, the team would have left the community more dependent on "outsiders" than capable of articulating and investigating their own problems. Such a deficit in social research eschews the responsibility of the researchers to build the capacity of the community and reinforces historical dependency relations.

Parameter three. The team itself must check its process against the desires of the original contractor as it moves through the research.

Even with the existence of the committed and joint research team, in order to monitor the pulse of the community the team will need to have ample meetings with the community leadership during the initial research and post-research. I would say that the efforts made after the report to work with and

inform the community were certainly a strong point of the project and a necessity for a collaborative project. However, in a collaborative project, members would not have made the errors of judgment in reporting and would have reached consensus on the precise variables to study.

Parameter four. Research can be discontinued once begun if the team and the contractor (provided this is the community studied) agree that it is not appropriate. However, the power to decide this is ultimately that of the community contractor.

We seem to assume that a contract once made must be completed. However, completing a contract in fact may mean not to complete the research and/or not to report the results. Only the community should decide this. A collaborative process has a reasonable chance of knowing whether the research team should approach the contractor and/or community leadership to determine if the research should continue. It would not have been a failure to discontinue the project or not to report the results if the Borough would have determined that the common good was best served by such means.

Summary

Foulks writes in this issue:

It was difficult for the Research Team through the polemic to locate the voice of the Inupiat. Most of the speakers had been non-Native. [T]he Inupiat understood their problem with alcohol in terms of stresses created by culture and political change. Their perspectives were not unlike those of the Departments of Health and Public Safety, Intersect, the Technical Advisory Committee, and the researchers.

Foulks implies in this latter part of his article that in some ways his group was almost sabotaged or victimized by a group of non-Native professionals who wanted to, at best, paternalistically protect the Inupiat or, at worst, protect their own positions of power as "helpers" and definers of what is right and wrong with the Barrow community. This seems to me to be a convenient scapegoat. From retrospective reports of Inupiat people and examinations of the local press, (*The Pioneer All-Alaska Weekly, Anchorage Daily News, and Barrow School News*) there were in fact a large number of Inupiat people not only upset but also in disagreement with the report. More appropriately, as Foulks (1980) states, "Barrow is a complex and multifaceted community containing many factions—non-Native and Inupiat alike." Additionally, Foulks indicates the research team should have developed methods that insured more and broader community participation.

My conviction is that the team would have needed to change their scientific paradigm to a collaborative approach in which empowering the community, building community capacity, and discovering answers to complex questions unite in the same research process. I do not believe the question is "how to use research results" but rather "how to conduct research in Native communities such that the decisions are clearly theirs." Professional ethical standards and standards of scientific conduct must become contextualized by a paradigm that creates an indigenous research methodology and collaborative research ethics and methods. Only the community should decide when benefits outweigh risks. The design and conduct of the research will determine how likely it is that local communities will have this opportunity. Researchers must not simply become more self-conscious of "intangible, value-laden factors," but instead must employ a systematic method in which research means building capacity. The outside researcher becomes the consultant to the community rather than the community as the informant or consultant to the researcher.

Naivete seems to have little to do with the problem confronted by the research team and intersect. I have argued it has more to do with assumptions about research expertise, ethics, relationships between researchers and community, the paradigm of research, and assumptions about the capacity of Native communities to engage in the process of research. Objectivity is a standard for our research. Unfortunately in conducting research in Native communities, objectivity often seems to imply that outsiders can make statements about such communities based on the data they gather and questions they ask without considering the lack of community involvement in shaping the research. The research assumptions related in the Foulks' article can originate from an institutionally-racist perspective which sees the Western positivist paradigm as helping to reach objectivity when, in fact, it guarantees distance from the reality of the people they wish to understand. We must avoid solely product considerations in order to embrace culturally-based research processes.

I appreciate Foulks attempt to face the failures of his work, however I do not believe he has adequately faced the bias in the methodology and inappropriateness of his continuing to report the data. I also think he should state directly that he does not plan to report the data in any further forums, a clear desire expressed by people from the North Slope. Such a clear, definitive statement would signal the beginning of a needed paradigmatic shift in his thinking.

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THAT WAS YESTERDAY, AND (HOPEFULLY) YESTERDAY IS GONE

PHILIP A. MAY, Ph.D.

In his article, Dr. Edward Foulks describes a series of events which have been acted out, in varying forms, many times on many reservations and in numerous Native villages. Those of us who have worked in the area of Indian mental health have all witnessed and, in some cases, have become involved in such scenarios and debates.

The use of research results, data, and interpretation have presented many challenges for professional and Native alike. The biggest change which I have witnessed over the past 18 years has been the mixing and partial synthesis of these two perspectives—the Native and the professional.

Attitudes in the 1950s and '60s

In the 1950s and 1960s the professional and Native perspectives were far apart; further, they were seldom found within the same human body. There were few Indians or Alaska Natives with professional training, and most of the non-Native professionals who worked on reservations were "short timers" who seldom were able to utilize their non-Native perspectives and reactions to Indian culture in a manner which was constructive and useful for the advancement of Indian mental health solutions. As a result, there were major battles between the polar interpretations of Native and professional paradigms, world views, and ideas on research. The clash of these perspectives took many an interesting turn and resulted in many strange alliances, such as those described by Foulks. The end result of most of these battles was a failure to rationally plan for and carry out proper research studies, and an incapacity to consider and utilize the results of research. The old axiom that "research is a dirty word among (and irrelevant to) Native communities" was often heard, and was used to obstruct and discredit both research initiatives on reservations and the application of relevant results from mainstream research to Native community needs.

An Easing of Tension

Since the late 1960s, change in this area has been slow and localized, but very decided and obvious. Research among most Native groups is less

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an arena of combat and more an area of appropriate debate. Emphasis has been modified among many Indian groups from radical dialogue, which excluded research, to appropriate concern for research as a guide for health, social, and mental health programs. I believe there are a number of reasons for this. First, the doors of higher education were opened more widely to Indians and Alaska Natives in the 1970s, and many Natives have received advanced training. Therefore, many have achieved positions of authority in health, social, and educational programs. Second, even though few Natives have gone specifically into research, their academic and professional training has developed a new understanding and appreciation of the research process and what it can and cannot do for local community needs, interests, and problems. In short, several types of research (e.g. applied and evaluative) have been destigmatized among many groups and separated from some previous research which had little bearing on social and health planning.

Third, today there is a larger number of researchers in the mental health and health areas who have developed and maintained long-term working relationships with tribal groups regarding mutual concerns within the areas of research, planning, and development. Fourth, many federal initiatives, mandated by law, have required research and evaluation of programs (e.g., tribal-specific health plans, evaluation of health status, and education programs), and have provided money for these endeavors. Therefore, tribes wishing to maintain old monies and gain new program monies have had to reach out and establish contacts and relationships with researchers. Fifth, radicalism among Indians and non-Indians alike has declined, allowing for more reasonable debate.

A New Outlook on Research

The above trends, then, set the stage for a new outlook on research. Because of these trends, new procedures and mechanisms have evolved in a number of communities which facilitate research initiatives and the application of research results. Many tribes, most Indian Health Services (IHS) area offices, and many other local concerns now have individuals and committees who are charged with reviewing research proposals, results, and information. These committees are vehicles for formalizing debate, ensuring input from tribes, and for striving towards the proper use of results. They provide a more appropriate arena for the debate and contention of research ideas. Thus, mechanisms are in place in many locales to safeguard against some of the calamities detailed by Foulks. Further, these committees serve an educational purpose for tribal concerns and researchers alike.

I personally have had manuscripts and proposals questioned and even rejected by IHS and tribal committees. In the process of debate, criticism, and revision, these committees and I educated one another. The resulting

projects and manuscripts were greatly improved, and we all were able to gain new knowledge, perspectives, and understanding from the process. One must, however, be patient, generous, and "thick skinned," and must strive to minimize feelings of proprietary claim on the research in these negotiations. Even though these committees can in some instances cause delays and seem obstructive, the forum they provide is vastly desirable to the series of negative experiences described by Foulks regarding the Barrow, Alaska, ordeal.

Once the research and manuscripts have been approved by an official—and hopefully representative—tribal committee, then publication and dissemination of the results are safer, less likely to cause problems, and more likely to be used for its intended purposes. Further, these committees can suggest various ways to present the factual data so as to protect against offending important constituencies, yet to allow for appropriate implementation. When a mutual understanding is attained by a researcher and a committee, a strong social force has been created. If manuscripts are not approved and data cannot be disseminated, it is painful. This has rarely happened in my experience, but when it does, if one is patient, new, and more acceptable approaches to research and application of research results can be solicited. Censorship, a larger issue, can and has been raised in considerations of tribal and agency research committees. But given the fact that small Native communities can suffer tremendous harm at the hand of the media and public opinion, I prefer to accept the power and judgments of the designated research representatives of the IHS and representative tribal groups.

I tend to believe that the experience described by Dr. Foulks in Northern Alaska is an extreme one, and one which is becoming less likely each year because of the factors mentioned above. But it is still possible that these events can be repeated. I would hope that safeguards such as tribal and agency research committees will continue to grow in number and sophistication. I further hope that we researchers will continue to improve our sensitivity to and control over the volatile potential of our results.

The article by Foulks is important in that it documents a history of how research results might be used in a disruptive, unproductive, and calamitous way by a variety of actors and forces. As history, the Foulks article is valuable to all those who aspire to do research on reservations for the improvement of Indian communities. It is appropriate to conclude these comments with two quotes:

"The use of history is to give value to the present hour and its duty."
- Ralph Waldo Emerson

"History, by apprising [men] of the past, will enable them to judge the future."
- Thomas Jefferson

Researchers working in the areas of American Indian and Alaska Native mental health need to know and understand the history of research politics among Natives. And we must strive not only to avoid the bad experience, but to work to ensure effective, culturally-appropriate and sensitive applications of research results. We may fail in these efforts, but the only virtue lies in trying.

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EDUCATING THE RESEARCHERS

CAROL LUJAN, Ph.D.

For many years the tribes and communities of American Indians and Alaska Natives have been the subject of intense study and research in a number of disciplines, particularly anthropology, sociology, psychology, and medicine. Until recently, virtually all research was conducted by non-Indians. Only in the past 20 years have more American Indians begun to enter into the social science professions. Consequently, the majority of research questions, methodologies, and conclusions has been based solely on the interests and cultural background of non-Indians. Furthermore, the types of research funded has been and continues to be controlled by various non-Indian governmental and state agencies.

A Paternalistic Emphasis on Deviance

One of the effects of the western European's positivistic approach to the study of American Indians and Alaska Natives has been to concentrate on studying their weaknesses rather than their strengths. For example, the question of why Indian people have survived as a group despite the odds is not a popular subject for funding. Consequently, few studies examine this topic. The majority of funded research focuses on the deviance of American Indians and Alaska Natives, such as alcoholism, suicide, violence, and mental illness. These are certainly important areas of concern for American Indian/Alaska Native people; however, the manner and method that many researchers have used has not been beneficial to the tribes involved.

Specifically, the researchers' main objective has been to collect the data (usually during the summer), and then publish their findings. Few researchers have encouraged meaningful involvement of the tribe. Even fewer have provided useful feedback to the communities. This paternalistic utilization of knowledge has been exploitive and detrimental to Indian people, especially since it feeds into the negative stereotype of Indians.

Lessons of the Barrow Study

The Foulks article is an example of mistakes that can occur among well-intentioned researchers. It is a useful paper for both Native and non-Native researchers interested in working with tribal groups because it provides

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helpful information for avoiding serious problems. Specifically, it highlights the crucial need for researchers to learn about the culture and the political structure of the tribe prior to entering the community. Foulks experience stresses the importance of encouraging meaningful community involvement in all phases of a research project. Furthermore, this paper demonstrates the growing cultural awareness among some non-Native researchers and practitioners in the Inupiat area. It also reflects the (intentional or unintentional) insensitivity towards the Inupiat community of the researchers and practitioners who were directly involved in the study. Additionally, it indicates that the Inupiat are concerned about the dissemination of information which might have a negative impact on their community.

As a result, it seems that both the Inupiat and the researcher(s) have learned from the experience. The Inupiat are probably more likely to take an active role in all phases of research projects and the researchers are probably less likely to enter a community without first learning the political and cultural structure of the area. In the past, the relationship between the researcher and the Indian tribe/group has been exploitive in favor of the researcher. Researchers entered a community, collected data, and left.

Today, the researcher-community relationship is moving toward one that is more balanced. Ethically, researchers working with Indian tribes/groups should, at the very least, be respectful of the people involved and be willing to share information with the community rather than limit their knowledge to specialized journals and academic colleagues.

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HEALTH AND SOCIAL SCIENCE RESEARCH IN THE ARCTIC: GUIDELINES AND PITFALLS

ROBERT F. KRAUS, M.D.

The opportunity to comment on "Misalliances in the Barrow Alcohol Study" by Dr. Edward Foulks opens the door to the discussion of a wide range of issues in sociocultural research. These comments will focus on issues specific and relevant to the Barrow study. I will confine myself to discussion of the Arctic and the immediate relevance of Arctic Research to the larger scene. Arctic research has attracted increasing official attention in the decade since the Barrow Alcohol Study. A wide range of guidelines and standards have been developed which were not in place in 1979. The Barrow Alcohol Study, with its sequelae, will be viewed as a social phenomenon in and of itself. A brief analysis and discussion will focus on issues relevant to Arctic research.

The Arctic and Subarctic regions comprise some 20% of the earth's land mass. These regions constitute one of last great frontiers and reservoirs of untapped resources and are undergoing intense exploration and development. The importance of the American Arctic to the scientific community should be clear. National defense, resource development, rapid population change, and the intense differential stresses which impinge upon Arctic populations, Native and non-Native alike, are all issues of broad research significance. All indicators suggest that technological and developmental change is having a shattering impact on the traditional cultures and adaptations of the Arctic. It is worth noting that the North American Arctic and Australia are the only areas of the world in which hunting and gathering societies have always lived in isolation from agricultural, pastoral, and industrial systems. In the north, peoples live as they have existed throughout most of human existence (Burch, 1988).

Development of Ethical Guidelines Since 1979

Unfortunately, formal research emphasis in the Arctic has been centered in the natural sciences, with scant attention paid to the wide, fascinating, and significant range of research questions in the social and behavioral sciences. This was the case in 1979 when the Barrow Alcohol Study was conducted. Since then, a variety of national and international initiatives too numerous to review here have taken place with reference to medical, behavioral, and

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social science research. These are listed in Appendix A. Of particular note is the publication of the Association of Canadian Universities for Northern Studies (1982) entitled *Ethical Principles for the Conduct of Research in the North*. A listing of the principles is reproduced in Appendix B. Most recently, the Committee on Arctic Social Science of the National Research Council (1989) has prepared a comprehensive report addressing issues in Arctic Social Science Research with recommendations for further development.

The Study as a Social Phenomenon

This context did not exist at the time of the Barrow Alcohol Study. It is to be hoped that, from the paper under discussion and the associated commentaries, some understanding of the study as a social phenomenon in and of itself will emerge. I would like to offer a few observations in this regard. Concerns about alcohol abuse in Alaska with respect to both Native and non-Native cultures are not unusual. Alcohol abuse in Alaska and the circumpolar region generally is a paramount public health concern and is a much-studied phenomenon. It is of interest to note that DuBay and Kelso (1986) review over 300 publications concerning Alaska Native alcohol abuse. I am not aware of any publication concerning alcohol abuse in Alaska Natives that engendered turmoil and conflict comparable to the Barrow Alcohol Study. A ten-part series in the *Anchorage Daily News* (1988) on Native alcohol abuse and related problems was confrontational, specific, and—according to some—sensational in its approach, listing names and dates and displaying photographs. While it engendered comment pro and con, the public reaction was not similar in quality or degree to that which followed the Barrow study. A follow-up article (*Anchorage Daily News*, 1989) was also received without extreme responses.

How then does one account for the reaction which followed the Barrow Alcohol Study? Space and the editorial limitations placed on a commentary do not allow a detailed analysis of this complex phenomenon. I would, however, like to note certain terms used in the paper as follows: Director of Intersect, a consulting firm located some 3000 miles from Barrow; Director of Public Safety; Director of Public Health; faculty at the Inupiat University (the University no longer exists, having disbanded under a legal cloud several years after the study); hospital administrator; Technical Advisory Group; Advisory Committee; North Slope Borough Health Department; Arctic Regional Corporation; Borough Government; North Slope Bureau Public Information Office; Health and Social Service Agency of North Slope Borough. This was in an Inuit village with a population of a few thousand of whom 60% were "traditional" in their life style. It is worth noting that the first five individuals mentioned in the list above were non-Natives and that the Technical Advisory Group was also predominantly non-Native in composition. To my knowledge, none of the categories listed above have any relevance to traditional Inuit culture. Observers of the Alaskan scene have noticed an

interesting phenomenon in which western corporate, bureaucratic structures staffed in many instances by non-Native transients with marginal professional training and qualifications and agendas having little to do with local culture, have been superimposed on traditional Native groups. In many communities, the results have been problematic.

The unfortunate sequelae of the Barrow study seem to relate to a number of factors. The investigators undertook field research in a situation for which adequate professional guidelines and standards had not been established. They proceeded with their work on the mistaken assumption that they had the informed consent of the Native population and that research findings would be handled in a professionally responsible manner. In fact, it would appear that they were dealing with a network of non-Native institutions and agencies external to the culture and became enmeshed in a complex economic and political struggle. It is possible that if the guidelines listed in Appendix B were in place in 1979, that the research would have been conducted differently, or perhaps may not have been attempted at all.

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Appendix B Ethical Principles for Conduct of Research in the North* (*Association of Canadian Universities for Northern Studies, 1982, pp.3-5)

1. The research must respect the privacy and dignity of the people.
2. The research should take into account the knowledge and experience of the people.
3. The research should respect the language, traditions and standards of the community.
4. The person in charge of the research is accountable for all decisions on the project, including the decisions of subordinates.
5. No research should begin before being fully explained to those who might be affected.
6. No research should begin without the consent of those who might be affected.

7. In seeking informed consent, researchers should clearly identify sponsors, purposes of the research, sources of financial support, and investigators responsible for the research.
8. In seeking informed consent, researchers should explain the potential effects of the research on the community and the environment.
9. Informed consent should be obtained from each participant in research, as well as from the community at large.
10. Participants should be fully informed of any data gathering techniques to be used (tape and video recordings, photos, physiological measures, etc.), and the use to which they will be put.
11. No undue pressure should be applied to get consent for participation in a research project.
12. Research subjects should remain anonymous unless they have agreed to be identified; if anonymity cannot be guaranteed, the subject must be informed of the possible consequences of this before becoming involved in the research.
13. If, during the research, the community decides that the research may be unacceptable to the community, the researcher and the sponsor should suspend the study.
14. On-going explanations for research objectives, methods, findings and their interpretation should be made available to the community, with the opportunity for the people to comment before publication; summaries should also be made available in the local language.
15. Subject to requirements for anonymity, descriptions of the data should be left on file in the communities from which it was gathered, along with descriptions of the methods used and the place of data storage.
16. All research reports should be sent to the communities involved.
17. All research publications should refer to informed consent and community participation.
18. Subject to requirements for anonymity, publications should give appropriate credit to everyone who contributes to the research.

A CASE STUDY OF HOW CROSS-CULTURAL MISUNDERSTANDINGS CAN NEGATE RESEARCH

DWIGHT B. HEATH, PH.D.

Dr. Edward Foulks has provided a valuable service by offering, in this context, a brief summary of an ambitious research project about which the results were primarily in reports of only limited circulation (Klausner & Foulks, 1979, 1980), and a costly book (Klausner & Foulks, 1982). Perhaps even more valuable is his summary discussion of how the project became exceptionally famous—and infamous—for many of the wrong reasons. Researchers, administrators, journalists, and others concerned with mental health and special populations have much to learn ethically and procedurally on the basis of this sad example.

Foulks' summary of findings is accurate as far as it goes, however, interestingly, it omits some of the points that loomed largest when the project was featured news in *The New York Times* and other media for nearly a week.

There is not enough detail in Foulks' present article to serve as a basis for any substantive critique of the research process itself, or of the conclusions. This is not an appropriate context in which to explore in detail the methods and findings of the study that are not reported here. Anyone who is seriously interested, for whatever reason, should go to the earlier and larger sources, which include not only more details about what was done and what was found, but also some fairly extensive discussion of misgivings that were weighed by the researchers as they were selecting their sample, instruments, and so forth. It is noteworthy that those reports are replete with caveats and conditional statements that are the stock-in-trade of academic writers, but that were—predictably—ignored by reporters whose newspaper articles selectively and forcefully publicized the findings in a way that angered many of the people of Barrow.

Foulks' Study: A Cautionary Tale

Presumably no one questions the good intentions of the research team, both with respect to conducting a meaningful study and in terms of phrasing it so that practical implications could easily be discerned and changes implemented to diminish the prevalence and severity of a variety of social problems that people thought of as being associated with alcohol. This

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paper, like Foulks' sad epilogue to the commercially-published version of the full report (Klausner & Foulks, 1982), can well be viewed as a cautionary tale. A Monday morning quarterback whose special skill is 20/20 hindsight might be less than appreciative that Foulks here "raises important ethical and procedural questions to be considered carefully in future projects" (Foulks, this issue), and wonder why they were not considered in the planning of the Barrow study—but such a view may not be fair to those who suffered through it.

One might wonder how much hope there was at the outset that locals would warmly receive a group of researchers from "the lower 48," who had been contracted (by an emigrant) to deal with a quintessentially Alaskan problem. Furthermore, the fact that the diverse constituencies who were interested in the study had very different agendas must have been obvious at an early date, and there should have been judicious weighing of the pros and cons—from the perspective of each of the several groups—at every step in the process of collecting data, analyzing it, presenting the results, and making recommendations.

In principle, the idea of having "a Steering Committee consisting of local Inupiat leaders, and a Technical Advisory Group which was constituted primarily of non-Native professionals" who were also familiar with the area was appropriate, and one might even have expected that such contacts would have helped the investigators to avoid major pitfalls.

But the idea of publicizing the findings before discussing them with local people in a town meeting, so that "public awareness would shock the Inupiat into action to control the alcohol problem", appears, at least in retrospect, grotesquely naive. The fact that the Inupiat were acutely aware of various aspects of "the alcohol problem" had been a dominant theme throughout the report, and the fact that the voters had enacted prohibition at the community level was ample evidence that they had already taken a strong stand. The lack of enforcement of that prohibition presumably has more to do with negligence on the part of the Department of Public Safety (an instigator of the research project) than with the Inupiat themselves.

In summarizing the research and its findings in this issue, Foulks is highly selective, providing little insight into what apparent justification the people of Barrow may have had for complaining about press coverage of the study. For example, it seems to me noteworthy that when the results of the survey "were released to the press by Intersect and the Research Center," the press conference took place in New York City rather than in Barrow. Similarly, it seems ironic that there is no mention in the present paper of the startling "72 percent alcoholism rate" that was featured in *The New York Times* (and presumably other mass media), or about Klausner's having been quoted about "the Eskimos...[as] not a collection of individual alcoholics, but a society which is alcoholic, and therefore facing extinction" (*The New York Times*, 22 Jan. 1980, p. C1).

It is little wonder that members of the community expressed concern over such a characterization. Nor could their ire be dismissed as simple denial of a harsh reality. In a later article, following a marathon public hearing on the subject in Barrow, it was reported that "residents challenged the validity of the report, contending that some of the research instruments and researchers were culturally biased and that some of the findings were erroneous because of false assumptions" (*The New York Times*, 29 Jan. 1980, p. C5). Those same criticisms were important among the points made in the independent "academic critique" that Foulks mentions as having been conducted by a member of the University of Alaska's Center for Alcohol and Addiction Studies.

Without going into great detail, one pertinent example of cultural bias is the questionable use of the Michigan Alcoholism Screening Test (MAST). It is gratifying that the authors recognized transcultural variation in the meaning of the component questions: "...it was necessary sometimes, with the help of a native assistant, to explain the intent of certain items [in the MAST]" (Klausner & Foulks, 1982, p. 165). Although they subjected the responses to elaborate statistical factor-analysis, they appear to have done the scoring without any adjustment for cultural differences, which raises serious questions about the validity of one of their major quantitative indices of alcoholism.

Heavier reliance on qualitative data might have helped the researchers to present a better rounded characterization of the nature and extent of problem drinking and drinking problems in the community. Furthermore, richer ethnographic detail might have provided a better understanding of the norms, values, and meanings that are the context of such behavior.

But there is another level of significance to this study that seems not to have been addressed by the investigators at all. Quite apart from whatever details are specific to this local case-study, the "misalliances" in Barrow are important in terms of some other general principles. However important this case may be as an example of pitfalls in cross-cultural research and the risks that are inherent in outsiders' dealing with sensitive issues among "minority" populations, it would be a mistake to overlook another crucial set of implications.

If we are to derive maximum profit from the sad experience of Foulks and his colleagues, we should also pay attention to what the data from Barrow have to say about various approaches to prevention and treatment of alcohol-related problems in a more general sense. For example, despite the enormous amount of ink that has been spilled about the percentage of the population who might "really" be alcoholic or about other indices of the scale of "the problem," most people seem to have lost sight of the fact that Barrow was officially a "dry community," one in which prohibition had long been the rule under Alaska's local-option law.

The Control Model of Prevention

If the highly-touted "control model" of prevention—espoused and promoted by National Institute of Alcohol Abuse and Alcoholism, the World Health Organization, and the highly vocal "New Temperance Movement" (Heath, in press)—were correct, such severe restrictions on the availability of alcohol should have resulted in virtual elimination of drinking problems. That is the approach the Soviets tried a few years ago, and recently gave up as a deeply flawed policy. That is what the Scandinavian countries have favored for decades while suffering dismally high rates of problems, in striking contrast with circum-Mediterranean countries where cheap and easily available alcohol results in high average per capita consumption, but is accompanied by relatively few problems. That is what Iceland, the United States, India, and various states elsewhere around the world tried briefly, and then repealed when they found that prohibition—except when imbedded in a religious context—seems not only to be ineffective but often also to be counter-productive.

There are many advocates of "the control model", which favor a variety of legal and regulatory controls, who explicitly reject total prohibition, whether on the basis of ideology or realism. They have been increasingly vocal in recent years throughout much of the world, calling for higher prices, fewer sales outlets, shorter sales hours, and a variety of other measures that would make alcoholic beverages more difficult to acquire. Presumably these steps would also diminish consumption (e.g., Calahan, 1987; Grant, 1985; West, 1984). However, there are few places in the world where alcohol is more expensive and outlets more restricted than in Barrow. Evidently the basic premises of "the control model" have little relevance to drinking patterns among the Inupiat.

Another cornerstone of "the control model" is the appealingly simple generalization that alcohol-related problems occur in direct proportion to the legal availability of alcohol, so that legislative and regulatory controls are the best way to curtail such problems. In fairness to some of those who are most often cited in support of such a view, it must be noted that Bruun and his colleagues (1975), Makela et al. (1981), Room (1984), and a few others have been careful to point out exceptions and to qualify carefully most of their statements in scholarly contexts. But it is at least equally apparent that such niceties disappear in policy recommendations, consensus documents, and other briefs that are meant to be forceful among legislators, administrators, and other laypersons (Heath, 1988a). Whatever else it may be, the Barrow case is a dramatic illustration that "the control model" did not work, even among the relatively homogeneous Inupiat population, where it had already been chosen by a majority of the voters.

The Sociocultural Model

This is not to say that there should be no controls on drinking. It does suggest that "the sociocultural model" (Heath, 1988b) might be more apt, emphasizing informal controls (rather than formal ones), expressed by peers and kin (rather than in laws and regulations), enforced by social pressures (rather than by judicial penalties), and within the normal sociocultural ambience (rather than isolated from it).

While there is no quick and easy way to help the many Inupiat who may have been hurt by inappropriate actions as a result of their own excessive drinking or someone else's, the experience of some other Native populations elsewhere suggests that changes in attitudes toward drunken behavior, increasing cultural awareness and related self-esteem, and a concerted community effort to re-integrate people without dependence on excessive drinking can achieve significant changes in the rate and types of problems that occur (e.g., Hall, 1986; Jilek, 1981; May, 1986; Weibel-Orlando, 1985).

It is ironic that this brief paper by Foulks, with the various comments on it, may turn out to be the most beneficial outcome of such an ambitious, expensive, relatively well organized, methodologically varied, and multi-disciplinary research project. We are all in his debt for offering his scientific colleagues and others this positive opportunity to learn from a sad experience.

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REJOINDER TO THE COMMENTS ON THE MISALLIANCES IN THE BARROW ALCOHOL STUDY

EDWARD F. FOULKS, M.D.

The Committee on Arctic Social Sciences of the U.S. National Research Council has just published its mandated recommendations for developing a strategy for future social science research in the Arctic (Kraus, in this issue; Committee on Arctic Social Sciences, 1989). The committee focuses major attention on the issue of research ethics in Arctic communities. It recommends that a statement on ethics should be developed that not only "guides scientist, but also gives people and communities affected by research and their representative institutions a clear idea of what behaviors to expect from scientists in the conduct of research" (p. 58).

Among the questions that the committee feels should be explicitly addressed are: "Should those proposing to conduct social research in the Arctic be required to obtain a permit from local authorities prior to the initiation of their research? If so, what is the appropriate process for making decisions about requests for such permits? Should scientists working in the Arctic be required to disseminate the findings of their research to residents of the communities in which they work? What form should the dissemination of these findings take? In cases where the publication of research findings may negatively portray Arctic residents or Arctic communities, should Arctic residents have some say in the publication or timing of the release of research findings? How can requirements of this sort be made compatible with the traditional value of academic freedom?" (p. 58). The presence of guidelines which would address these questions would have obviously allowed us to design, implement, and report our research in a more congenial manner.

The commentaries from respected colleagues in this issue suggest how our research and the research of others might have been better negotiated with the community. As Westermeyer (in this issue) points out, psychiatric disorders are a stigmatized subject in our society and in most others. Unlike epidemiological studies on heart disease, for example, studies of alcohol dependency, schizophrenia, or other mental disorders are likely to threaten the self-image of a community. Because of stigma, individuals suffering alcohol dependency in our own society are often not willing or able to consider themselves as such. Rather more frequently than not, they are

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offended when family, employer, or physician suggest the possibility. In some cases, the problems created by their alcohol use become persuasive and the individual seeks treatment. In other cases, denial of the problem tragically persists because of the associated social stigma, and individuals, families, and communities are presented from dealing with the condition in a humane and effective manner. Because of this bias, research on mental disorders must be conducted with even more sensitivity than other kinds of medical and social research.

Trimble (in this issue) points out that the levels of alcohol use and abuse in all populations, remain high in rural Alaska. Wolf (in this issue) adds that our study focused too narrowly on the sociological factors associated with alcohol use in only one community, when the pattern is problematic in other rural Alaskan communities as well. He cites new approaches at the state and local levels that mandate an examination of specific tolerance of alcohol-related and/or drunken behavior in a community. Researchers and clinicians must be sensitive to the fact that such tolerance is often based on not wanting to offend victims or their families by labeling alcohol-related behavior a disorder. In contrast, it is important to recognize that a different kind of sensitivity is utilized to identify symptoms of heart disease. Heart disease is not shameful; a mental disorder, like alcohol dependency/abuse unfortunately is.

For these and related reasons, the crux (or the Achilles-heel as Shore puts it in this issue) of our study was the manner in which it was reported. Trimble, Heath, and others (in this issue) are correct in laying the ultimate responsibility for release to the press of the research findings at the feet of the researchers. The directive from Intersect, and the concurrences of the Steering Committee and Technical Addressing Community in Barrow to do so, made sense to us at the time. We believed—as they did—that the problems of alcohol-related morbidity and mortality were not being addressed by community and state institutions, and that the release of the report might bring much needed attention to this formidable but perennially overlooked problem. In retrospect, it has become apparent that this perspective was based on the clinical approaches used with individuals suffering alcohol dependency/abuse with concomitant denial in our own society. As Guilmet and Richards (in this issue) point out it represented the biases of the biomedical model of health/disease. The model follows a format in which experts in medical and psychiatric conditions make a diagnosis based on the empirically derived categories of biomedical technology. Their subjects (patients?) receive the announced diagnosis and are expected to comply with recommended treatments. True collaboration between expert and subject in defining who and what is to be studied, how it will be studied, and for what purposes has not been obtained.

Exciting alternatives to such regrettable approaches are presented in this issue by Mohatt, Trimble, May, Beauvais, Lujan, and others. They argue for research approaches that "build the capacity of the studied communities

to become researchers of the needs of their own communities." Such involved collaboration can help a community "learn of its strengths and define its problems, thus becoming empowered and strengthened" (Mohatt, p. 64). Interestingly, parallel shifts in relationships between medical experts and their clients are also emerging. Consumer advocacy groups such as the National Alliance for the Mentally Ill are forming powerful constituencies, which may result in more negotiated approaches to patient care. The demystification of expert opinion and the enhancement of public information and sophistication has the potential to bring about a more ethical and effective delivery of services needed to manage problems of individuals and communities.

The current status of funded social science research in the Arctic, however, remains appalling. The National Science Foundation funded no projects there during 1985 and 1986, and estimates funding of just \$62,000 in both 1987 and 1988 (Committee on Arctic Social Science, 1989). It is to be hoped that the recommendations of the Committee on Arctic Social Sciences and the approaches outlined by Attneave, Richards, Shore, and others in this issue will clear the way for researchers and communities to move forward with much-needed projects in alcohol-related problems and other important issues affecting the health and welfare of Native American and Arctic peoples.

We are grateful for the opportunity to air the painful events precipitated by our research so that some of the factors and issues which have prevented progress in needed areas of research might be addressed explicitly and solutions found. I believe that the commentaries offered in this journal have provided the concepts and framework for a new beginning in social and medical science research in communities in Alaska and elsewhere.

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