

## **ALCOHOL AND ETHNOGRAPHY: A CASE OF PROBLEM DEFLATION?<sup>1</sup>**

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### **ABSTRACT**

The paper proposes that the modern ethnographic literature on alcohol in tribal societies tends to underestimate systematically problems due to drinking. This "problem deflation" probably reflects a concurrence of various assumptions, methods and theoretical orientations in the literature. The functionalist assumptions underlying much of the literature tend to point attention to the gains more than the losses involved in drinking. The pleasures of drinking are more easily observed, in the ethnographer's methodological focus on the everyday and the social, than the private pains and the rarer events -- casualties, chronic disease mortality, etc. -- which are the focus for epidemiologists; the ethnographer may also be less likely to notice abstention. English-speaking and North European researchers have dominated the alcohol ethnography literature; anthropologists from these societies who came to maturity in the last half-century -- particularly in North America -- were members of "wet generations" reacting against the perceived narrowmindedness and moralism of the later temperance movement, and committed to an alternate middle-class ideal of "moderate drinking". Using Schaefer's stratified probability sample of well-studied cultures, it is shown that ethnographic studies published before 1930 were more likely than later studies to report "extreme" male insobriety and regular drunken brawling. In a "dialectic of expatriation", ethnographers may also have been particularly concerned to differentiate themselves from the emphasis of missionaries on the immorality of drinking. The ethnographic alcohol literature, mostly produced by non-specialists in alcohol studies, has tended to accept as a transcultural reality and as an adequate characterization of alcohol problems the modern disease concept of alcoholism; since this "culture-bound syndrome" did not match the nature of difficulties related to drinking in many cultures, ethnographers have often concluded that alcohol problems were rare. The immersion in

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concrete data has muted the effect of these various factors on the ethnographic literature, but there is a need to reexamine general conclusions concerning culture and alcohol based on that literature. In the new generation of alcohol ethnographers, often working within their own society, problem amplification may be more of an issue than problem deflation.

## **ALCOHOL AND ETHNOGRAPHY: A CASE OF PROBLEM DEFLATION?**

This paper explores the proposition that the problems associated with drinking alcoholic beverages are systematically underestimated in the ethnographic literature. This proposition first came to mind as a result of my experience as a participant at two meetings, one a conference on "Alcohol Use and Abuse in Papua New Guinea", and the other an "Inter-American Workshop" on "Legislative Approaches to Prevention of Alcohol-Related Problems". The papers from both conferences have been published (Marshall, 1982c; Kaplan, 1982a). But although these proceedings include a wealth of valuable work, neither of them convey the full flavor of the discussions which went on at the meetings. And it was these discussions at least as much as the papers which first set me thinking.

The Papua New Guinea conference was an extraordinary occasion. Mac Marshall and his coworkers on a two-year study of alcohol in Papua New Guinea had brought together in one meeting room a majority of the alcohol control commissioners from the 19 provinces of the country, interested people (indigenous and expatriate) from the health and social service systems, women's groups, church groups, and other community groups -- and about 30 scholars, primarily anthropologists and with North Americans in the majority, who had done fieldwork in Papua New Guinea. I count it as a memorable week in my education that I was able to listen to the excellent research presentations and the rich and multifaceted discussions that resulted.

But as I listened to the discussions, it began to strike me that there was some discrepancy between the interests, agendas and perceptions of many of the anthropologists, on the one side, and of many of the indigenous participants, on the other. One obvious strain was between the topical emphases of the anthropologists' papers, which concentrated on village cultures and celebrated cultural diversities, and the focal concerns of permanent residents in Papua New Guinea, which centered on urban problems and stressed the agendas of nation-building.<sup>2</sup> But there was also a less obvious strain, which was along the dimension of how severe the problems of alcohol in Papua New Guinea really were. On the one hand, those resident in Papua New Guinea -- with the notable exception of some of the alcohol control commissioners, who were presiding at that moment over a very steep increase in availability (Marshall, 1982a) -- tended to emphasize the severity of the problems. On the other hand, the anthropologists -- not so much in their formal papers, but more as the discussions turned to policy implications -- tended to downgrade the severity of the problems; some tended to discount assertions that alcohol problems were a serious issue as merely reflecting the prejudices of missionaries and women's groups. Yet, from my perspective as a total

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<sup>2</sup>In line with these concerns, the field and survey work carried out by Marshall and his coworkers during the two-year project concentrated on urban populations. Much of this work has not yet been published.

outsider, one had only to read the daily paper and to examine the research tallies of the content of newspaper stories -- which showed large numbers of stories on alcohol issues -- to see that concerns about drinking problems were widespread in the society, and that alcohol issues were at a minimum a major rhetorical arena in political discussions about nation-building. One might well argue about whether the concerns were misplaced or overdrawn, but in my view the existence and strength of the concerns had to be recognized and taken into account. Because of this perception, my contribution at the close of the conference (Room, 1982a) had in mind as its audience as much the anthropologists as the PNG residents.

A few months later, I was again the beneficiary of a very substantial education, this time at a conference, including Latin American, Caribbean, Canadian and U.S. participants, which focused on alcohol problems and policies in Latin America. By and large, the Latin American and Caribbean participants were psychiatrists, while the North Americans were trained as social scientists and lawyers. Among the papers presented at this conference was a magisterial review of the ethnographic and historical literature on alcohol in Latin America by Dwight Heath (1982), who was uniquely qualified for this task by his credentials as a Latin Americanist and as the foremost bibliographer of the ethnographic and other literatures on alcohol and culture (see, for example, Heath and Cooper, 1981). In the conclusion of his paper, Heath ventured a few paragraphs about the degree of severity of alcohol problems in Latin America:

One of the most important generalizations that emerges from an overview of my quarter-century of watching, reading, listening, and otherwise paying attention to alcohol use in Latin America is the rarity with which "alcoholism" occurs. To be sure, it is a serious problem that few of the authors define what they mean by "addiction", "alcoholism", or "alcoholics."<sup>3</sup> Nevertheless it is striking that so many investigators -- most of them presumably at least familiar with the major controversies over these terms -- explicitly make the point that these phenomena are absent in the populations studied. . . . I will go a step further: . . . another important generalization from Latin American experience is that alcohol-related problems, like "alcoholics" and "alcoholism", are rare -- at least in comparison with populations in the rest of this hemisphere, and even in comparison with much of the rest of the world. A few nations are exceptions to this, but . . . Latin Americans, by and large, seem to feel that they suffer few serious problems that can be related to alcohol use, either their own or on the part of significant others. This is all the more significant when one considers that there is, in a very real sense, some bias toward finding problems when one is doing research on alcohol use. . . . Another striking contrast with alcohol-related problems in other parts of the world is the rarity of individuals who suffer greatly in terms of mental health. The guilt-ridden solitary drinker who is so

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<sup>3</sup>In a footnote here, added after the conference, Heath explains: "This statement is deliberately phrased in very general terms. The reference to "alcoholism" is intended to encompass a broad range of the several definitions that have been proposed by professional researchers in various fields, as well as "alcohol dependence syndrome". Although reference to "rarity" admittedly reflects a sociological and anthropological emphasis on dominant patterns, it seems not to be significantly discrepant with those other kinds of data which reflect the epidemiological emphasis on incidence-rates of cases, however aberrant the distinguishing patterns may be."

commonplace in Anglo-America is fortunately missing in most of Latin America, where drinking is usually done in a setting that emphasizes sociability (pp. 157-8).

These conclusions were in sharp contrast to the views of the Latin American participants in the workshop. Commenting on Heath's presentation, Negrete noted diplomatically that "the social scientists' view of alcoholism is one of deviant behavior, that is, a form of alcohol use which does not conform with established and socially acceptable cultural patterns. However, alcohol dependence as understood in biological terms is unlikely to be seen as noticeably different behavior in a milieu which provides so much opportunity for socially approved inebriation. Professor Heath's remark about the need to complement ethnographic observations of drinking behavior with biomedical studies is very well taken indeed" (Negrete, 1982). The "consensus views" which emerged from small-group discussions at the close of the conference took as their premise the existence of very substantial alcohol-related problems in Latin America.<sup>4</sup>

Another piece of evidence on the issue with which I am concerned can be found in Joy Leland's thoughtful book on Firewater Myths, which draws widely on the ethnographic literature on North American Indians (Leland, 1976). In Leland's view, there are actually two "firewater myths". One is the widely-diffused and longstanding belief that "Indians are constitutionally prone to develop an inordinate craving for liquor and to lose control over their behavior when they drink" (p. 1). The other, more recent belief, which Leland notes "appears most often in the writings of anthropologists", she labels the "reverse-firewater hypothesis": "that alcohol addiction is actually rare" among Indians, although many proponents of this hypothesis would agree that drunkenness is common among Indians (p. 5). Leland notes that the stereotype of inordinate craving for alcohol among Indians might be seen as particularly "damaging" to them; "perhaps for this reason, many observers of Indian drinking have taken particular pains to refute this portion of the myth" (p. 4). Leland collects statements by 23 authors (including 12 anthropologists; pp. 5-8, 116) that alcohol addiction was altogether absent or uncommon in the group they were studying, as against statements by 14 authors (including 5 anthropologists; pp. 116-8) that indicated alcohol addiction was present.

In all three of these cases, concerning widely-separated areas of the world, there is a consistent strand: compared to other observers, anthropologists tend to minimize the seriousness of drinking problems in the tribal and village cultures under discussion. One view of this is that the minimization of problems by anthropologists is simply an appropriate corrective to the overemphasis on the problems associated with drinking in much of the rest of the literature. There is certainly much to commend this viewpoint, which is, for instance, adopted by Harry Levine when he nominates anthropologists to lead the rest of the alcohol studies field out of what he sees as an exclusive preoccupation with the problem side of drinking (Levine, 1981). But I tend towards the view that the disparity between the ethnographic and other alcohol literatures also reflects a systematic bias in the modern ethnographic literature against the full recognition of alcohol problems in the cultures under study.

Before considering how such a bias might occur, the issue of minimization deserves explicit attention. Those of us who have worked on social science and epidemiological studies in the

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<sup>4</sup>Kaplan, 1982a, pp. 273-278. Harry Gene Levine, a U.S. historical sociologist, dissented vigorously from this premise -- see pp. 279-281.

alcohol field have long been disturbed by the systematic discrepancies between the evidence on levels of alcohol-related problems as we saw it and reported it and the way the evidence was presented in policy-oriented documents. In the U.S. in the current era, there is a systematic tendency to dramatize the results in such a way as to magnify the problems. Often the concerns have remained a private anguish; our shorthand jargon for what happened between the research literature and the policy documents was in terms of a process of "problem amplification" or "maximization" or "enhancement." Recently, these issues have become a matter for explicit analysis, as sociologists have turned their attention to the operation of such processes in the alcohol field (Wiener, 1981; Gusfield, 1981).

But although problem amplification has been the dominant mode in recent years, and has been the focus of these sociological analyses of the relation between research and policy, the process can also operate in the opposite direction. For instance, as we looked at the history of U.S. studies and statements about alcohol's relation to casualties and crime, we found it appropriate to talk in two-directional terms -- of "problem obscuration" as well as "problem enhancement" (Aarens et al., 1977, p. 36). In this area, the 20 years or so after Repeal of Prohibition tended to be a period of problem minimization, in part as a reaction against the claims of the temperance movement, and in part because of the desire of the nascent alcoholism movement to present "the alcoholic" in as positive and respectable a light as possible (Room, 1978, particularly Chapter 10). The widespread minimization of consequences of drinking in the scientific literature in this period extended even to physiological sequelae of heavy drinking. Straus comments concerning this that "it is interesting to conjecture whether the need of scientists to dissociate from the temperance ideology and from being labeled as 'drys' may have profoundly influenced the questions that scientists were asking, thus precluding the discovery of answers that were then socially undesirable" (Straus, 1979).

As Straus implies, the process of mediation between research findings and policy documents is not the only possible locus for "problem amplification" or "problem deflation". Such amplification and deflation can also be built into the research process itself -- in where the researcher chooses to look, in what questions the researcher chooses to address, in what methods are used, and in how the data is interpreted as it is reported. The researcher may well not be conscious of the amplification or deflation; often, as in the instance noted by Straus, the bias simply reflects the worldview from a particular cultural perspective at a particular historical moment. As the terms are used here, amplification and deflation cover both methodological artifacts, where a research paradigm results in under- or over-measurement, and interpretative artifacts, where the measurements which are made are "played up" or "down", focused on or ignored. As explored below, both mechanisms can be seen as having been at work in ethnographic studies about alcohol.

The terminology of "amplification" and "deflation" tends to imply that there is some absolute criterion against which measurements or interpretations concerning problems in a culture can be calibrated. There is, of course, no such absolute standard: one culture's problem may be another culture's solution. But neither, I would contend, are the problems associated with drinking entirely relative (see Room, 1978, pp. 11-18). A casualty death or a case of cirrhosis is to some extent unwelcome in most cultures and most circumstances. For that matter, in terms of social consequences, we may suspect that most cultures would find problematic the degree of social disorganization associated with, say, gin drinking in 18th century London. There is no unarguable solution to this issue; in the present discussion, our implicit criterion is simply a matter of judgement -- a judgement with which others may well disagree. It is fully recognized that we are also covering

over with this blanket of personal judgement the vexed issue of alcohol's causative role in problems.

The remainder of this paper, then, explores the contention that there is a systematic tendency in the modern anthropological literature towards "problem deflation". In my view, this deflation probably reflects the concurrence of a number of aspects of the assumptions, methods and theoretical emphases of ethnographic work on alcohol. In suggesting some of these aspects and how they may have operated, the present contribution is intended to open a dialogue.

### **Functionalist Assumptions**

As Heath has noted, the ethnographic literature on alcohol has had a strong functionalist cast throughout the last half-century. Functional perspectives have most often been pitched at the level of the society as a whole -- an approach which "focuses on the society or the culture as a sort of organism, and assays to spell out the functions that various institutions, beliefs, behaviors or other component parts play with respect to facilitating, maintaining, adapting, or adjusting the societal and/or cultural organism as a whole" (Heath, 1975, p. 50).<sup>5</sup> A smaller number of studies, in Heath's estimation, have focused on functionality for the individual -- on the purposes that drinking serves on a personal level (p. 51).

Heath acknowledges that "one of the criticisms occasionally leveled at anthropologists is that, in their effort to discern the presumed logical consistency and functional integration of various traits within sociocultural systems, they sometimes overlook inconsistencies or dysfunctions" (p. 51). In my view, the deemphasis of the problematic side of drinking is not only a matter of oversight, but rather tends to be inherent in a functionalist perspective. There are always gains and losses from every behavior, if only in the sense that the behavior is a choice among competing alternatives. Sometimes both the gains and the losses accrue to the same individual or collectivity, while many times someone gains while someone else loses. A functionalist perspective is biased towards a concentration on the "gains" side of the equation. From Durkheim onward, in fact, the emphasis tends to have been on the hidden gains behind apparent losses, where what seems to a casual observer to be peculiar, pointless, or cruel behavior to an individual or subgroup is argued to be functional for the maintenance of the group as a whole. In alcohol studies, as Heath notes, the function of drinking perhaps most often cited is in the maintenance of social cohesion or conviviality. In true Durkheimian fashion, the boundaries of the drinking group are often, however, as much a matter of exclusion as of inclusion (Heath, 1975, p. 51; see also Cavan, 1966, pp. 216-233). The gain in prestige or reaffirmation of solidarity for those included may be matched by a prestige loss or alienation for those excluded.

Implicitly viewing the culture or society as an organism, functionalism has "focused attention on the stabilizing, the pattern-maintaining, or the boundary-defining processes in social life" (Bock, 1963). With respect to drinking norms, the anthropological literature on tribal societies has emphasized agreement on drinking norms, and the harmony of these norms with the culture's overall normative patterns; in Heath's words, "in terms of the global ethnographic picture, . . . consensus and consistency is the rule" (p. 50). Where obvious problems related to drinking are

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<sup>5</sup>It should be noted that while I am relying heavily here on Heath's characterizations of findings from the ethnographic literature, he interprets the characterizations quite differently from the present argument.

acknowledged, a functionalist perspective will tend to ascribe them to causes external to the society itself: "the interpretations that are offered tend, in most instances, to emphasize the political and/or economic dominance of an alien society, and the disruptive impact that it has on native systems of belief and behavior" (Heath, p. 41). Implicitly, the functionalist's organic image of society sets up a neat equation where behavior that is seen as produced from within the society is "good", while behavior derived from outside is "bad": "customary patterns of drinking normally are integrated with other patterns of belief and behavior to a significant degree, and in ways that generally 'fit'; . . . habitual drinking in other than indigenous ways is often disruptive, both of the family and of other levels of social organization" (p. 41, emphases in original).

In line with general trends in anthropological thought, an explicitly functionalist analytical framework has been replaced in recent ethnographic alcohol studies by other explicit frames. But the interpretations offered often remain implicitly functional, or share with a functionalist perspective an image of tribal cultures as organic and autonomous wholes, and a focus on the immanent positive supports for the status quo.

### **Methodological Focus**

In recent years it has become evident that there is often a sharp contrast in views between the ethnographic evidence, on the one hand, and the epidemiological conclusions, on the other, concerning the seriousness of alcohol-related problems in a culture or society (see Heath, 1975, p. 47). This disjunction was sharply expressed in the contributions to the conference on alcohol problems in Latin America, referred to above. It is a fair summary of one aspect of Raul Caetano's comprehensive review of epidemiological studies in Latin America to say that he found "evidence in many countries of significant health damage suffered by individuals as a result of alcohol consumption (liver disease and psychiatric hospitalization), as well as studies indicating a link between consumption and such problems as accident and violence" (Kaplan, 1982b). On the other hand, we have already quoted Heath's conclusion, based on the ethnographic literature, that alcohol-related problems are relatively rare in Latin America.

There are several possible explanations for the disjunction. With respect to the specific case of Latin America, Caetano pointed out that, while ethnographic studies are focused on the village societies of the native peoples of the region, epidemiological studies have focused on urban working-class populations (Caetano, 1982). On the other hand, Negrete has suggested that, at least in Argentina and Chile, "alcohol abuse is a problem of greater importance in the rural areas. Urbanization is likely to have a beneficial influence on the behavior of individuals from rural areas in Latin America, who may find greater support in the cities" (Negrete, 1981, p. 166). So it is not clear that the different study populations of the two disciplinary orientations help explain the discrepancy in the literatures for Latin America.

A more general factor in the disjunction between ethnographic and epidemiological literatures is the historical and methodological tendency towards "problem amplification" of medical epidemiology, as a policy-oriented research discipline. As the discipline's name implies, its paradigm and methods were developed for use in epidemic diseases -- where the need for action, and the need to develop the popular will for action, were both urgent. As I have previously noted, "epidemiological ideology tends to be avowedly activist and oriented to the heroic: every epidemiologist carries in his or her knapsack the handle to the Broadstreet pump" (Room, 1978, p. 70). The tendency towards amplification is built into such common epidemiological statistics as

Relative Risk; such a statistic is totally insensitive to the absolute prevalence of the disease or problem to which it is applied. In general, one might expect a disjunction between the two literatures to be at least as much a matter of epidemiological amplification as of ethnographic deflation.

In the case of the Latin American literature, however, the particularities of the epidemiological literature do not seem to imply much amplification. In fact, one could argue the opposite: as Caetano shows, the literature has been oriented around a North American-style disease concept of alcoholism, and has paid little attention to alcohol's role in such substantial societal problems as accidents and violence. The 16,800 injuries and 240 deaths of the 1982 Rio de Janeiro carnaval, for instance, are manifested in press reports<sup>6</sup> rather than in epidemiological studies.

Perhaps the most powerful explanation of the disjunction, both in the particular case of Latin America and more generally, lies in the very different frames of data collection and analysis of ethnography and of epidemiology. By design, ethnography is oriented to the study of the everyday, while epidemiology is oriented to the study of rare events. In Heath's words, "traditional anthropological techniques include a combination of participant-observation and interviewing, with greater emphasis often on nondirected, rather than directed, interviewing and with little attention to any kind of systematic sampling procedures" (Heath, 1980a). While fieldwork in some studies may extend over many years, in others it is a matter of months; the geographic range of fieldwork is often restricted.

In this framework of data collection, the ethnographer is likely to witness all or most of the pleasures of drinking, but to miss some of the problems -- particularly the life-threatening problems which are the focus of attention of the epidemiologist. The pleasures of drinking are quotidian, and easily visible in a village society. Some of the most serious pains, on the other hand, are far from an everyday occurrence: some are long delayed, while others are relatively infrequent events. Thus to die of alcoholic cirrhosis may require 15 years of steady heavy drinking, while, in a village society, drownings, falls over cliffs, homicides and other alcohol-related casualties may still be relatively frequent even if they occur only every few years. Unlike ethnographers, epidemiologists are attuned in their methods to relative rates of occurrences, and to studying relatively rare events. Their focus, in fact, is on explaining happenings -- like death from a particular cause -- that are not everyday events in a village society. Some of the differences that have been noted between the epidemiological perspective and the ethnographic perspective on alcohol problems in village societies may thus derive from the disciplines' very different frames of data collection.

Levy and Kunitz make the related point that some of the events of interest from an epidemiological perspective may not be accessible to the method of participant observation:

The method is remarkably effective when used to measure normative behaviors and attitudes. It allows us to see how drinking is defined in the society, to relate normative drinking behavior to other behaviors, and to place it in a larger structural context. On the

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<sup>6</sup>San Francisco Chronicle Feb. 25, 1982, p. 28; United Press report. The tactic of purposive inattention in applying the potentially problem-amplifying epidemiological paradigm can also be observed with respect to the alcohol consumption-cirrhosis mortality connection in the U.S.; see Room, 1978, pp. 122-123.



other hand, it is difficult to learn about covert and deviant forms of drinking. Homicide, suicide, alcoholic psychosis and, perhaps, the withdrawal syndrome are difficult to observe directly in such small groups and the anthropologist must make inferences concerning them based upon general statements made about these acts by his informants. As such statements may be normative explanations themselves, they frequently do not describe the reality with any degree of accuracy (Levy and Kunitz, 1973, p. 223).

Ethnographic methods, in short, may underestimate the problems related to drinking because they are better attuned to measuring the pleasures than the problems of drinking.

At the other end of the drinking spectrum, we may suspect that ethnographic accounts may also tend to underestimate abstention from drinking and negative attitudes to drinking, particularly when abstention or negative attitudes are private matters rather than public and symbolic statements. In this connection Leland notices lacunae at both ends of the drinking spectrum in the ethnographic record on North American Indian drinking; the studies "deal for the most part with normative behavior, and do not attempt to describe the total range of behavior surrounding alcohol use. The extremes of abstinence and 'excessive drinking' (however that term is defined in the group concerned) usually receive less attention than behavior between these extremes" (Leland, 1976, pp. 22-23). The absence of a behavior is always harder to notice than its presence: abstention or near-abstention as a private, unmarked behavior would be easily missed unless specifically looked for. The tendency for ethnographic methods to focus attention on public and collective behavior in the culture directs the fieldworker toward the pleasures of the drinking group, usually male, and away from the private agonies which the men's drinking may involve for women and children. Here the clash of perceptions is not so much with medical epidemiology as with survey research. With a perspective rooted in the ethnographic literature, Heath (1982, pp. 155, 175-6) is frankly incredulous at the "cultural negativity toward alcohol" found in a survey study in a Mexican village and urban neighborhood; Negrete (1982, p. 170) adds that "anyone who has had the opportunity to observe the same population behaving spontaneously in natural settings must doubt the correctness" of survey findings that one-third of adults interviewed had not had a drink of alcoholic beverages in the year prior to interview.<sup>7</sup> The difference in findings may reflect more than the ethnographic focus

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<sup>7</sup>Despite his strictures on the study's "methodological shortcomings", Heath cites it in support of another conclusion in his paper (p. 154). The study in question is Calderon et al., 1981. Heath and Negrete were referring to the preliminary cross-cultural analysis of the same study (Roizen, 1981). An independent study using the "Jellinek informant method" in the same Mexican village, whereby community drinking practices and attitudes were described by summing results from 30 five-person groups from a cross-section of occupational groups in the community, also found a considerable "cultural negativity" towards alcohol: concerning how a man getting drunk is considered by a woman in the community, 20% of the groups said it was "completely intolerable", and another 50% that it was "undesirable but something one has to tolerate". Concerning the proportions of the community population who do not drink, 40% of the groups thought at least half of the males abstained, and 67% at least half of the females (these opinions, if taken literally, would imply a higher abstention rate than found in the survey). A majority of the groups thought both men and women in the community perceived a woman who abstained with approval; 20% of the groups thought men and 40% thought women also perceived with approval a man who didn't drink. Such a method, involving declarations in front of one's everyday neighbors, invites the display of "public

on public and collective behavior. As usually applied in practice, the "emic" perspective of the ethnographer may imply a greater attention to the "opinions which count" in the society under observation, whereas survey methodology tends to start from the "one person one vote" egalitarian assumptions of Western post-Enlightenment thought. In this sense, the two methods can be useful correctives to each other: surveys can pick up cultural perspectives which are disenfranchised but nevertheless present in the society, while ethnography may better reflect the society's internal hegemonic structure in the setting of norms.

### **The "Wet Generations" and Alcohol Ethnography**

However much he or she strives to understand and present the culture under study from the inside, the ethnographer brings to the field perceptions and values formed in his or her own culture. Much of the relevant ethnographic literature has been provided by ethnographers from Northern Europe and English-speaking countries -- countries which, in the very broadest of terms, have shared a common history of radical shifts in the last century in the cultural position of alcohol. As ethnographers have belonged primarily to the middle-class liberal intelligentsia, a consideration of possible "home" influences on their perception and presentation of alcohol issues must start from the history of alcohol as an issue for middle-class liberal intellectuals.

There are, indeed, variations in the national histories, but we can say very roughly that for European and North American literary authors who came to maturity in the late 19th century, the cause of temperance was a progressive reform, and alcohol was a serious problem for attention.<sup>8</sup> For those who came to maturity after about 1910, on the other hand, the alcohol issue had disappeared from the roster of progressive issues. In North America, particularly, by the end of the 1920s abstention from drinking or concern about alcohol problems became for young progressives an outdated cultural style associated with rural conservative know-nothings. In the United States, the alcohol issue became, indeed, the marker of a great cultural divide (Sinclair, 1962). First among Bohemian youth, and then after about 1927 among middle-class youth in general, drinking became the "symbol of a sacred cause. . . . For us it was a self-righteous pleasure", A.J. Liebling wrote in his reminiscences; "drinking, we proved to ourselves our freedom as individuals and flouted Congress" (Liebling, 1981, p. 667; Room, 1982b; Warner, 1970).<sup>9</sup> While Pekka Sulkunen (1979, pp. 71-72) has identified a "wet generation" in the general population in Finland that came to maturity in the 1960s, it might be said that the "wet generations" among North American liberal

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attitudes" rather than what Genevieve Knupfer has termed "covert norms". In a situation of cultural negativity about alcohol, this invites overestimation of problematic drinking behaviors and of the disruption they cause. But the method does offer striking support that cultural negativity as a public attitude to alcohol is not a "curious" finding in present-day Mexico. See: Natera et al., in press, Tables 10, 28, 29; Smart et al., 1980.

<sup>8</sup>For a view of "progressive" writers in this period from a modern Eastern European official perspective, see Bratanov, 1975.

<sup>9</sup>The impact of this "wetting" of the American middle class can be seen in the decline in the class position of temperance movement leadership. See Gusfield, 1955.

intellectuals lasted for about half a century; only in recent years are there equivocal signs of a "drying" tendency.

Associated with this politicocultural stance on alcohol, eventually, was a specific theory of the cultural genesis of alcoholism, a theory which became known -- though not by anthropologists<sup>10</sup> -- as "the sociocultural model". Drawing on general functionalist perspectives that we have already noted, the theory viewed drinking that was normative, integrated in the culture, and traditional as unproblematic; it was new drinking styles imported from the outside, or the breakdown of traditional norms on drinking, that produced drinking problems. Where there had been an active temperance movement, as in North America, the normative conflicts between the traditional drinking culture and superimposed abstinence norms had produced a cultural "ambivalence" about drinking and resulted in a high rate of alcoholism.<sup>11</sup>

While this line of argument did receive a formal exposition in the alcohol literature, it reflected a set of presumptions and views that were widespread in general "progressive thought". In this wider arena, at least in North America, "moderate drinking" or "responsible drinking" became the cultural ideal, abstinence was viewed as a sectarian peculiarity, and problems associated with drinking were depreciated and confined within a tightly-defined "alcoholism". In considering the anthropological literature on alcohol, it is appropriate to consider this wider cultural frame, rather than the special frame of the alcohol literature. For a salient feature of the ethnographic work on alcohol, noted by Heath, is that most of it has been done by researchers who had no previous history in alcohol studies, and who may not have been attuned to the alcohol dimension of their data while they were in the field. In Heath's words,

one important factor that has shaped ethnographic studies of alcohol to date is their almost uniformly incidental or casual conception. At a recent international conference on alcohol studies and anthropology, I got unequivocal confirmation of a long-term hunch -- not a single one of the anthropologists in attendance who had published on drinking patterns had set out originally with that in mind. By that I do not mean that they had changed their focus of research during field work, but rather that they had studied something else . . . and found, when analyzing their data later, that the relations between people and alcohol were important enough to deserve special discussion (Heath, 1975, p. 60, emphasis in original).

While there are a variety of ways in which this characteristic of the literature may have shaped the

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<sup>10</sup>Anthropologists have tended in fact to be puzzled by the application to a specific set of hypotheses of a name which for them signifies the whole paradigm of cultural anthropology; see Heath, 1980a.

<sup>11</sup>For a critique of the logic of this position, see Room, 1976. The theory received its most explicit statement by the sociologist Albert Ullman:

In any group or society in which the drinking customs, values and sanctions -- together with the attitudes of all segments of the group or society -- are well established, known to and agreed upon by all, and are consistent with the rest of the culture, the rate of alcoholism will be low (Ullman, 1958).

For a review of the literature on the explicit "sociocultural model", see Frankel and Whitehead, 1981.

ethnographic record, it seems clearly to imply that most ethnographic accounts are written by authors who are essentially laymen with respect to the alcohol literature. Their conceptualization of alcohol issues is thus likely to be roughly that of the average liberal intellectual of their own society and time.

At least in the case of North American ethnographers of the last 40 years, then, this conceptualization would have included a tendency to view drinking as the "natural" state of man, and abstinence as an unnatural goal which results in a problematic "ambivalence" about drinking (Room, 1976). It seems, on the basis of my observations at the Papua New Guinea conference, that there may have been a tendency toward this view even among anthropologists working in places where there was no alcohol aboriginally.

If the ethnographic work on drinking is indeed affected by the cultural definitions of drinking in the ethnographer's own culture, we would expect to find the perceptions of the alcohol dimension in the study cultures shifting between the early years of the century and the decades after the 1920s. There are, as we shall discuss below, alternative possible explanations for such a shift, but there should at least be a shift. As a first effort to test this question, use was made of the sample of the ethnographic literature compiled by James Schaefer for the "hologeistic study" in his dissertation (Schaefer, 1973). Schaefer used a sample of 57 tribal societies with adequate ethnographic data for his purposes.<sup>12</sup> He provides the actual codes for the 57 societies for two alcohol variables (pp. 378-9): the degree of male insobriety in the society ("extreme", "moderate" and "rare"), and the presence or absence of

**Table 1**  
**Alcohol Codings of 57 Societies by Publicatrion Years of References**

<u>Male insobriety<sup>a</sup></u>			<u>Drunken brawling<sup>b</sup></u>		<u>Total</u>
Extreme	Moderate	Rare	Absent	Present	

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<sup>12</sup>The sample was based on a stratified probability sample of 60 tribal societies (stratified by cultural area, with one society chosen from each stratum) drawn by Naroll and associates, applied to the Human Relations Area Files (HRAF) Quality Control Sample Universe. Forty societies from Naroll's sample were included, with substitution of societies from the same cultural area in 17 strata where there was inadequate alcohol data for the original selection. Three areas, all apparently with relatively little aboriginal alcohol use, were excluded altogether, since there was no culture with qualifying data. In Schaefer's final sample, all but four of the cultures met "level B" of the HRAF's standards of quality control for the ethnographic literature: "1200 pages of cultural data, 1000 pages of which had to be ethnographic data. Additional requirements were: coverage by more than one authority; at least one monograph; no serious questions regarding basic ethnographic description; well rounded coverage of economic, social, political organization, as well as life cycle data; aboriginal conditions or nearly so; and data depth of 100 years prior to field time" (Schaefer, 1973, pp. 68-69).

**Date of earliest reference used:**

Before 1930	10	6	5	9	10	22
1930-1949	6	9	4	8	8	19
1950 & later	3	7	6	9	6	16

**Date of latest reference used:**

Before 1930	5	2	0	1	5	7
1930-1949	4	7	5	8	5	16
1950 and later	10	13	10	17	14	34

**Average of mean publication years for societies in category:**

	1918	1944	1934	1938	1933	1933
<b>No. of societies:</b>	19	22	15	26	24	57

<sup>a</sup>Excludes one society with no data on male insobriety.

<sup>b</sup>Excludes 7 societies with no data on drunken brawling.

regular drunken brawling in the society.<sup>13</sup> He also provides "the complete bibliography of all

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<sup>13</sup>Schaefer describes the coding categories in several ways, not quite consistent with each other. On the one hand, "'drunkenness' has been used in this and other studies as a gloss for any drinking behavior" (p. 117). On the other hand, the "male insobriety" variable is coded under an overall rubric of "functional impairment" (p. 118), with these categories on the final coding protocols: degree of male insobriety strong -- "excessive, long, unconsciousness" -- moderate -- "intoxication, non-regular unconsciousness" -- and rare -- "'they never become drunk', restraint" (p. 337; the definitions on p. 118 vary somewhat). "Drunken brawling" is defined as "the regular occurrence of physical assault among members of a single community while intoxicated" (pp. 117, 336).

Schaefer's "male insobriety" categories appear to be derived from those of Horton (1943). Horton is explicit that his "moderate insobriety" category can include relatively frequent drunkenness: "drinking usually ends in intoxication but does not continue for days. Unconsciousness not regular or frequent" (p. 266). He comments that "'moderate' drinking, in the European sense of only slight motor disturbances, is relatively rare in the sample" (p. 251). Schaefer's raters appear to be about as likely as Bacon et al.'s (1965) but less likely than Horton to code a society toward the "severe" end of the trichotomy. For the 16 societies with "male insobriety" codes from Schaefer and "male frequency of drunkenness" numeric codes from Bacon et al. (1965, pp. 89, 108-111), 4 were assigned a higher code by Bacon et al. and 4 by Schaefer (taking Bacon et al.'s scores 5-9 as equivalent to "moderate"). For the 13 societies assigned a male insobriety code by both Horton and Schaefer, 5 are coded more severely by Horton and 2 more severely by Schaefer; inspection of latest publication dates of the former 5 suggests the difference is at least partly due to coding

references used in the ethnographic analyses" for the 57 societies (pp. 361-373). Table 1 shows the sample's distribution on the alcohol codes for the publication year of the oldest reference used for the society, and for that of the most recent reference used. A mean year of publication was also computed for each society's references, and the average of these mean publication years is shown for societies falling in each coding category. Whichever way of dating the ethnographic sources is used, it can be seen that "extreme" or "strong" male insobriety is more likely to have been coded for societies with earlier ethnographic references than for societies with later references.<sup>14</sup> There is also a weaker tendency for the presence of drunken brawling to be more often reported in societies with earlier reference sources.

We can thus tentatively conclude that, with respect to the general ethnographic literature, "extreme" or "strong" insobriety is likelier to have been reported for a society by ethnographers working before 1930 than by ethnographers working in more recent decades. We have offered an interpretation of this relation in terms of the preconceptions ethnographers of the "wet generations" may have carried into the field. The obverse of this explanation, of course, would be that earlier ethnographers viewed the world with an especial sensitivity to the problematic side of drinking. This is certainly a viable hypothesis: in earlier years, ethnography, colonial administration, and missionary activities were not necessarily differentiated pursuits, and the missionary perspective, at least, often tended towards alcohol problem amplification. Against this argument must be set the fact that the materials included in Schaefer's study have passed through multiple winnowing processes attuned to modern standards and sensitivities: only 3 of the sources listed by Schaefer were published by religious publishing houses, and two of these refer to societies for which drunkenness is coded as "rare".

A third line of explanation of the difference would be in terms of historical change: although hologetic studies like Schaefer's conceive their material to be in the "ethnographic present", perhaps the difference reflects a real diminution in the worldwide occurrence of "extreme" drunkenness in tribal societies in recent decades. While common presumptions tend to point in the opposite direction, it is certainly possible to find instances (e.g., Levy, 1966) where the rate of alcohol problems or of extreme intoxication seems to have diminished since the beginning of the century. Yet another line of explanation would be that ethnographers may have reached and studied societies with extreme drunkenness on the average earlier than other societies. Thus the evidence offered by our reanalysis of Schaefer's data must be regarded only as suggestive rather than conclusive support for the operation of the "wet generations" factor in the ethnographic literature.

### **The Dialectic of Expatriation**

It is possible that the "wet" perspective of modern ethnography owes something not only to

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practices rather than to new findings in the literature.

<sup>14</sup>It is actually the societies coded "moderate" which have the most recent literature. If there was confusion in Schaefer's coding scheme, as seems possible, between "moderate drunkenness" and "moderate drinking", the pattern of dates may reflect the downplaying of abstinence as well as extreme drunkenness in the later literature.

the ethnographer's own cultural background but also to the specific situation and experiences of fieldwork. In their article on "Holy and Unholy Spirits," Mac and Leslie B. Marshall (1976) have traced the conflicts in the middle and late 19th Century between different white expatriate communities in Oceania. In particular, the Marshalls focus on the conflict in eastern Micronesia between the missionaries and the "beach community" -- both groups largely American -- and argue that this "antipathy" can be "explained as a fundamental clash in life styles and values that reflected a similar clash occurring at home in the United States" (p. 135). In this conflict, attitudes to alcohol played an important symbolic role: "right from the beginning, . . . the missionaries began to use the symbols of alcohol and tobacco to set themselves apart from other foreigners in the islanders' eyes. The message was simple and clear: we are different people with a different purpose" (p. 151). Alcohol was also an important symbol in the missionaries' efforts to alter "the very fabric" of the islanders' societies "to bring it into accord with the values of middle class American 19th century Protestantism". In this effort, they "found themselves in competition with local political elites for the allegiance of the common people. Once again the missionaries employed the powerful symbol of abstinence to press their cause. . . They opposed dancing, nakedness, polygamy, long hair and turmeric -- anything that offended their own brand of morality. But while they preached against all of these evils, they reserved their strongest sermons for demon rum" (pp. 165-6).

As 20th century anthropologists, in their turn, came into the field, they were likely to find the legacy of these 19th century efforts, with which they were professionally as well as culturally out of sympathy,<sup>15</sup> still determining the rhetoric for discussions of drinking. If abstinence was a self-conscious distinguishing mark of the missionaries, it must have been tempting for ethnographers, in their turn, to distinguish themselves from the missionaries on the alcohol question.<sup>16</sup> At the very least, we might expect ethnographers to wish to avoid any appearance of supporting missionary emphases and attitudes in their own work. This may be an explanation of "why the subject of alcohol and culture has been neglected for so long" (Marshall, 1982b) in the ethnographic literature on Oceania.

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<sup>15</sup>See the discussion of "the basically negative attitude of anthropologists toward missionaries" in Stipe, 1980.

<sup>16</sup>Stipe (1980) cites a reference to an anthropologist "who was in the habit of smoking on the premises of a missionary organization that had strict regulations against the use of tobacco or alcohol within its compound. In fact, he even urged some of the people there to accept free gifts of cigarettes." Apart from the question of relations with missionaries, the ethical questions raised by their own choices about drinking practices while in the field might well serve as a topic for discussion among ethnographers. Referring to the Navaho reservation in the Southwestern U.S., legally "dry" by tribal option, Levy and Kunitz note that "the pattern of heavy drinking by on-reservation Anglos has persisted. In addition to the drinking of traders, there was drinking by federal employees and anthropologists. . . . Older anthropologists have described their own summer drinking patterns while doing archaeological field work on the reservation during the 1930s. In all instances, Navajo employees have had the opportunity to observe the behavior and to compare it with the public statements concerning drinking" (Levy and Kunitz, 1974, p. 70).

To the extent this dialectic of reaction against missionary influence has operated in the ethnographic literature, we might expect it to result in an emphasis on the normative aspects of drinking, and a deflation of the problematic aspects. Presumably such a dialectic would operate most strongly in areas of the world, such as Africa and Oceania, where Protestant missionaries were active in the latter half of the 19th and early part of the 20th centuries, at the zenith of the temperance movements in their home countries.

The clash of perspectives between missionaries and anthropologists on the specific issues of alcohol may be seen as related to the more general movement by anthropologists after 1920 to distance themselves, by adopting a relativist and functionalist stance, from colonial administrative and moralizing perspectives. The characteristic professional standpoint of anthropologists of this period can thus be seen as a dialectical reaction to features of their own culture. Applied to alcohol issues, this stance took the form of leaning against the problem-amplifying views of colonial officials and of the earlier literature:

European observers of drinking behavior in primitive societies judge the degree of intoxication with reference to their customary European standards. . . . Due to the fact that drinking is a social problem in many if not most European countries, the European observer is interested in drinking behavior and he reports drunkenness more frequently than he reports other, less controversial, forms of behavior. He is quick to criticize what to him seems "excessive" drinking, and to praise "sobriety" and "moderation". He is frequently concerned with the practical problems of imperialism, and is sensitive to the effects of drinking on aggressiveness towards Europeans, on crime and poverty, and on trade. . . . His European morality, which makes drunkenness intriguing to him, often lends his comments a moralizing tone. This must be discounted in evaluating his report. (Horton, 1945, p. 250)

Deflationary impulses among anthropologists today, as more generally among social scientists (Levine, 1981), continue to have a dialectical basis. Critical reactions in conversations concerning the arguments presented here have tended to take the form not of denying the deflationary tendencies of the ethnographic literature, but rather of justifying them as a proper corrective to official problem amplification.

### **Ethnographers and the Disease Concept of Alcoholism**

North American ethnographers of the "wet generations" came from a cultural background in which all social and health problems associated with drinking tended to be viewed as symptoms of one underlying condition, a disease called "alcoholism", characterized by the experience of loss of control over one's drinking, and over one's life because of drinking. In the post-Repeal cultural debacle for temperance views, the disease concept of alcoholism served as a culturally acceptable and humanely-oriented governing image (Room, 1978) for discussing alcohol-related problems. The problem was not in "the bottle" but in "the man"; alcoholics had some "predisposing X factor" which made them unable to drink normally, unlike the great majority of citizens. The disease concept thus bifurcated drinking between "normal" or "social" drinking, on the one hand, and "alcoholic" drinking, on the other. The amount or pattern of drinking was seen as having little to do with the whether drinking was "normal" or "alcoholic"; instead, the crucial marker was the conscious experience of loss of control over drinking, with associated guilt feelings and such indications of divergence from normative expectations as gulping drinks and drinking alone. Both in the clinical



literature and in popular thought, this governing image of alcoholism as a disease tended to be viewed as a transcultural Platonic reality, in the same sense that measles and a broken leg have a transcultural reality.<sup>17</sup> The view can be seen continuing to the present as a fundamental assumption in the clinically-oriented literature; abandoning the term "alcoholism" in favor of a more tightly-defined "alcohol dependence syndrome", but retaining "an impaired control over intake of the drug ethyl alcohol" as a "leading symptom", a WHO Group of Investigators nevertheless concluded that "the alcohol dependence syndrome is a psychobiological reality, not an arbitrary social label"; instead of viewing dependence as culturally differentiated into various types, observed "cultural differences . . . could better be interpreted as culturally, environmentally or personally patterned manifestations of the fundamental alcohol-dependence syndrome" (Edwards et al., 1977, pp. 9-10).<sup>18</sup>

The history of sociological thought concerning the disease concept of alcoholism over the last 30 years might be characterized in terms of a gradual emancipation from the assumption that alcoholism (or the alcohol dependence syndrome) is a transculturally valid Platonic reality, and the movement instead to a nominalist view of the disease concept of alcoholism as a social construction of particular societies in particular epochs (Room, 1983). In particular, the modern American disease concept's focus on the experience of a personal loss of control has come to be viewed as a new, post-Repeal twist in an older tradition of thought, a tradition that "first emerged in American popular and medical thought at the end of the 18th and beginning of the 19th century. Around that time a new paradigm was created; or, in Foucault's terms, the 'gaze' of the observer shifted then to a new configuration -- a new gestalt" (Levine, 1978). To conceptualize loss of control because of drinking as the disease of "alcoholism" was a specific application of the medicalization of deviance and the preoccupation with individual self-control which emerged as general concerns in post-Enlightenment capitalist societies. A disease concept centering on loss of self-control, in this view, is not just differentially distributed between cultures, nor just manifested differently in different cultures; instead, it is at its heart a "culture-bound syndrome", a concept which has meaning only in a culture where individual self-control is the normative mode of social control.

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<sup>17</sup>Such a notion of "transcultural reality" of course in no way gainsays that cultures may impart different meanings to the same physical occurrences, and there will often be different rates of the disease in different societies.

<sup>18</sup>It should be noted that the "alcohol dependence syndrome" combines both of what were called, in earlier parlance, "psychological dependence" and "physiological dependence". It is the "psychological dependence" dimension we are focusing on here as "culture-bound". There are indeed transcultural physiological bases -- possibly somewhat differentiated genetically -- for withdrawal symptoms, drinking to relieve withdrawal, and functional tolerance. But even on the physiological side, the syndrome can be seen as culturally conditioned. Cultural expectations influence the recognition of and conceptual aggregation of physiological effects into a syndrome. And Levy and Kunitz (1981, p. 65) report that particular cultural drinking styles -- specifically, the rapid cessation of drinking when the supply runs out or is cut off -- may produce withdrawal symptoms as a consequence of a single drinking bout, without implying a continuing condition.

One might have expected this line of thought to have emerged from the anthropological rather than the sociological literature. Certainly, in my view, ethnographers were in command of materials which lend great strength to the argument. Yet in general, at least in dealing with alcohol issues, ethnographers have not made the crucial shift to a nominalistic perspective on disease categories as defined in their own culture. While alert to the existence and structure of "folk diseases" or "culture-bound syndromes" in tribal societies, ethnographers seem to have had blinkers on which kept them from looking back at the potential "culture-boundness" of disease categories -- and in particular the disease concept of alcoholism -- that they carried in their own cultural baggage.<sup>19</sup> Such a perspective is conspicuously absent in a review by Jack Waddell of the "principle of cultural relativity" as a fundamental premise in "anthropological research in alcohol use and misuse". Reviewing the perspectives of one anthropological conference on alcohol at another such conference, Waddell lists a number of characteristics of the literature and dimensions of the earlier conference that reflect "the ubiquity of the relativist premise", including "the relative effectiveness of different treatment philosophies" -- but without mention, as potentially subject to a relativist perspective, of the conceptualizations which underlie a treatment response (Waddell, 1981, pp. 19, 23, 24).

Some exceptions can be found to this general lack of a cultural relativism concerning North American concepts of alcoholism. A notable exception is some of the work of Edwin Lemert, whose work has straddled and been informed by both ethnography and general sociology. In his 1951 sociological text on Social Pathology, Lemert clearly identifies the culture-boundness of a concept of pathology oriented around loss of self-control:

The chronic alcoholic or drunkard is socially recognized by the compulsive or uncontrolled quality of his drinking, which persists in the face of severe social penalties and countless resolutions of his own to refrain from drinking. . . . In a given society, . . . in order for chronic alcohol addiction or compulsive drinking to develop, there must be strong disapproval of the consequences of drinking or of drinking itself beyond a certain point of intoxication, so that the culture induces guilt and depression over drinking and extreme drunkenness per se. . . . The general theme underlying [American attitudes toward the chronic alcoholic] has to do with lack of self-control on the part of the drinker. This societal symbolism of the deviation as a sign of character weakness is one of the most vivid and isolating distinctions which can be made in a culture which attributes morality, success, and respectability to the power of a disciplined will (pp. 341-341, 348-349, 356).<sup>20</sup>

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<sup>19</sup>One anthropologist, Miriam Rodin, has recently published (1981) a specific analysis of "alcoholism as a folk disease". But, although she draws the concept of "folk disease" from the ethnographic literature, her data and the analytical tradition within which she is working derive from survey research rather than ethnography.

<sup>20</sup>In her review of the ethnographic findings on North American Indian drinking, Leland edges toward a related perception: "if controls over drinking are culturally determined, and if Indians have never socially or culturally internalized such controls, we might be tempted to conclude that their absence in the group should not be interpreted as loss of control, i.e., a symptom of alcohol addiction". Faced with this statement, Mark Keller, a staunch defender of the disease

More generally, however, the ethnographic literature on alcohol reveals little sensitivity to the potential culture-boundness of alcoholism concepts. This does not necessarily mean that ethnographers sweep up the conflicts and problems associated with drinking observed in their study populations into a disease concept interpretation. Many ethnographers, indeed, have recognized very clearly that the patterns they observed did not fit North American or European disease concepts of alcoholism. Instead, the common procedure has been to hold the disease concept stencil up against the data on the society observed, to note that the behavior in the society did not fit the stencil's pattern, and on that basis to offer as a conclusion that there is little or no alcoholism in the society studied. In a literal sense, such a conclusion may indeed be warranted. But it is a conclusion that slides right past the corollary question: if the alcoholism concept has a bad fit with the conflicts and problems involving drinking in the society, what is an appropriate and culturally sensitive way of characterizing these problems and their degree of seriousness in the society? However "emically" the society has been studied, the focus on alcoholism as the criterion in discussing alcohol problems imposes an "etic" perspective on the interpretation. Often, indeed, the disease concept has served in ethnography, as in American and European societies generally, as a vehicle and rhetoric for ignoring or depreciating alcohol-related problems that did not fit the concept.<sup>21</sup> Alcohol-related problems other than alcoholism simply disappear from view, for instance, in Favazza's neat equation: "In discussing alcohol, . . . most psychiatrists, like other

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concept, offered a remarkable concession, suggesting there could be an alcoholism without loss of control: "Keller is willing to entertain the notion that perhaps 'conceptions of loss of control in Jellinek's and Keller's sense simply do not apply. If so, loss of control cannot be for Indians the pathognomic diagnostic sign of addiction to alcohol. One would have to look for other signs'" (Leland, 1976, pp. 53-54, quoting a personal communication from Keller).

<sup>21</sup>For further discussion on this point with respect to industrialized societies, see: WHO Expert Committee, 1980; Mäkelä et al., 1981; Moore and Gerstein, 1981.

Using a combination of ethnographic and epidemiological methods, Levy and Kunitz (1981, particularly pp. 64-68) have largely emancipated their analysis from an alcoholism perspective, and moved to a disaggregated "problems perspective". See also Leland, 1976, pp. 121-2.

However, such a combination of methods has led other researchers in alternative and more problem-amplifying directions. Using a conventional North American conception of "alcoholism" and an off-the-shelf scale (the Michigan Alcoholism Screening Test) drawn from the clinical literature but applied to a general-population community sample, a recent study by a sociologist and an anthropologist/psychiatrist in an Alaskan Eskimo North Slope village concluded that "*53% of the drinkers in the sample scored in the certain alcoholism range. . . . 72% of the population as a whole are possible or certain alcoholics*" (Klausner, Foulks and Moore, 1980, p. 48; emphasis in original). A New York press conference held to announce the results of the study drew headlines across the U.S. such as "Eskimos May Face Extinction as Alcoholism Destroys Society" (Klausner and Foulks, 1982). The aftermath included a lowered rating in the New York market for the study community's municipal bonds. Researchers concerned with alcohol have reportedly not been welcome in that region of Alaska since the report was issued.

physicians, think in etic terms of alcoholism. Emphasis is on individual psychopathology and pathophysiology. In contrast, most anthropologists think in emic terms of cultural drinking patterns. The term integrative drinking, for example, rarely appears in the psychiatric literature". (Favazza, 1981)

The process that we have outlined lies at the heart of the "reverse firewater myth" that Joy Leland outlines and attempts to test empirically from the ethnographic literature on North American Indian drinking. As we have noted, she suggests that there may be an ideological component -- a desire to depreciate Indian alcohol problems -- behind the "reverse myth". Furthermore, she points to the dominance in the literature of researchers without a substantial background in alcohol studies as a complicating factor:

Many observers of Indian drinking apparently are unaware that "alcohol addiction" is not a well-defined phenomenon and that there is little agreement as to what manifestations constitute valid indicators of the diagnostic category. The alcohol literature is very large and scattered. Most people who have not consulted this literature hold the "commonsense" conviction that everybody knows an "alcohol addict" when he sees one. . . . On the other hand, to their credit, I suspect that observers of Indian drinking who refuse to label Indians as "alcohol addicts" are reluctant to do so, not only because of their recognition of differences between Indian and White drinking characteristics, but also because of their largely intuitive recognition that the concept of addiction is unduly vague and arbitrary. If these observers were to scrutinize drinking in the dominant society as carefully as they have in Indian groups they might find themselves as reluctant to use the label "alcohol addict" in the former as in the latter (Leland, 1976, pp. 126-7).<sup>22</sup>

## **Concluding Comments**

### **SOME CAVEATS**

This paper started from the assertion that there has been a systematic tendency in the modern ethnographic literature on alcohol to deflate the prevalence of alcohol-related problems in tribal societies, and proceeded to develop in some detail a number of reasons this may have occurred. One problem with this procedure may be that our attempts at explanation have been too successful. I do not believe that the picture from the ethnographic literature is as distorted as the cumulation of lines of explanation might suggest. The defining characteristic of the literature -- its immersion in concrete data and "thick description" -- may have been its saving grace; while the atheoretical cast of much of the literature has often been decried, this quality may have helped to limit the extent of distortion.

It is also worth adding that the paper is not intended as grist for the mill of any "methods imperialism". Much has been learned and more can be learned from the existing ethnographic literature, which offers us the widest available range of material on the spectrum of possible interactions between human social arrangements and alcohol. Ethnographic methods are and will remain an important part of the armamentarium of alcohol studies.

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<sup>22</sup>Apart from its other virtues, Leland's analysis includes a lively if deadpan gloss on what Jellinek might have meant in his classic description of the symptomatology of alcoholism, on difficulties in its logic and operationalization, and on its potential transcultural applicability.

It should also be clear that, while the present discussion has focused on problem deflation in a particular segment of the alcohol literature, in my view problem amplification is a more pervasive issue in the whole sweep of the current North American alcohol literature. Although it may be unbalanced from a scientific perspective, from a policy perspective the ethnographic literature's emphasis on the positive aspects of drinking may be argued to serve as a useful corrective.

Perhaps the most serious effect of any "problem deflation" in the ethnographic literature is not so much on that literature itself as on cross-cultural analyses and generalizations which are based on that literature. If there are, indeed, systematic and temporally specific distortions in the ethnographic record, cross-cultural analyses that depend on the record may need to be reevaluated. To the other reservations about correlational "hologeistic" alcohol studies (Mäkelä, 1979) must be added the suspicion that the observed correlations may have something to do with the worldview of the ethnographic observer as well as the cultural plan of the observed society. Analyses that focus on correlations between drinking-specific dimensions, such as Frankel and Whitehead's (1981), seem particularly vulnerable.

#### THE INTERPLAY OF GENERATION AND PERSONAL HISTORY: AN EXAMPLE

Our explanations for "problem deflation" in the ethnographic literature have combined explanations at several levels: in terms of generational and historical change in governing images and approaches, in terms of professional paradigms and their dialectical relations with competing professional paradigms, in terms of personal biographies -- notably, the inexperience in the alcohol literature of most alcohol ethnographers. The interplay of these factors in an individual ethnographer's work can be seen in a remarkable reflective memoir by John Honigmann, published posthumously (1980). Honigmann contrasts ethnographic accounts of drinking among native peoples in three different sites in Northern Canada which he and his wife coauthored over a period of 25 years. Looking back at the first account, published in 1945 while he was still a graduate student, Honigmann notes the normalizing and tolerant account it presents of drinking and associated behavior by Indians and non-Indians in "Delio", a hamlet on the newly-opened Alaska Highway. Observing that the article treats "Delio as if it were an independent, primarily isolated, culturally highly distinctive social system", Honigmann outlines

how a variety of data is conceptualized and integrated to make it consistent with the perspective that sees Delio's alcohol behavior to be a normal part of the culture. A good part of the behavior would no doubt be illegal and unconventional in the main-line society, but in Delio -- it is acceptable to the groups we studied and must be accepted by an anthropologist who identifies with those groups' norms and values. This is the attitude I had learned from teachers. (pp. 268, 270-1)

Twenty years later, the Honigmans' account of drinking among the Eskimo of Frobisher Bay took quite a different perspective, presenting "alcohol as a serious personal and social problem with which the Eskimo were striving to cope successfully" (p. 272). Honigmann notes that "we came to Frobisher Bay knowing the town's notoriety as a place where resettled Eskimo had become socially disorganized. Immediately upon our arrival a non-Eskimo administrator briefed us on the widespreadness of native personal and social disturbance promoted by alcohol. Although our research conclusively disproved the stereotype of a socially disintegrated native community unable to deal successfully with alcohol, we reached that conclusion only by adopting, as a working hypothesis, the generally shared view of alcohol as problematic" (p. 272). In the wake of the Frobisher Bay experience, Honigmann published a reinterpretation of the Delio material, taking a

view of drinking as a short-term mode of recreation, see[ing] it as indicative of social disintegration, and frankly recogniz[ing] the illegal, or deviant, aspects of alcohol behavior in the community. . . . No new data led to the new interpretation. The explanation for the shift lies in two interrelated factors solely associated with the ethnographer. First, in reanalyzing Delio's alcohol behavior I abandoned the principle of cultural relativity. . . . I now judged events with the universal, or absolute, concepts of social disintegration and deviant behavior in mind. Second, [the new conceptions marked] changes in me, the ethnographer, as a person. New experiences had . . . include[d] exposure to social psychiatry and deviant-behavior theory, interest in applied anthropology, and greater familiarity with the literature pertaining to alcohol. (pp. 273-4)

In a study of a third community, published in 1970, the Honigmanns shifted ground again: "acknowledging the existence of deviant alcohol behavior, we treat such deviance relativistically, as if it were normal in the context of [the] native sector's frontier culture", as a "positively-sanctioned contracultural feature of frontier culture" (pp. 274, 277). Honigmann shows how this conception, too, could fit the situation he had observed in Delio.<sup>23</sup>

In this memoir, Honigmann records a personal journey which I believe other social scientists of the "wet generations" who have traveled far into alcohol studies will recognize: a strong initial focus on the positive aspects of drinking, a growing and perhaps overcompensating recognition of the problematic side, an eventual synthesis that acknowledges both sides. I believe that many in the minority of contributors to the ethnographic literature who have continued in alcohol studies could report similar journeys. If this is correct, the overall deflationary tendency of the ethnographic literature reflects the fact that most of its contributions have come from neophytes.

#### FUTURE TRENDS

The emphasis in the present analysis has been on "problem deflation", since until recently this has been, I believe, the dominant tendency in ethnographic studies. But in the future, the issue may rather be "problem amplification". There are several trends pointing in this direction. One is the relatively new phenomenon, noted by Heath (1980b), of the full-time engagement of anthropologists in alcohol research funded by mission-oriented agencies. Those of us who have long lived in that world are intimately aware of the attractiveness of problem amplification in these times in such an environment; Levy and Kunitz (1981) note that pressures on them toward problem amplification came from a variety of directions. A second development is the strong trend towards studies by ethnographers of drinking practices and problems in their own society. Frequently these studies are carried out in clinical environments, under clinical auspices, or in collaboration with clinicians --

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<sup>23</sup>Honigmann's conclusions concerning the nature of the ethnographer's work are quite far-reaching: "Were I to adopt the usual model of ethnography as a mirror of cultural reality, I would conclude this chapter saying I have described three styles of alcohol behavior, three different meanings of alcohol discovered in northern Canadian communities.

"But clearly, when the behavior of one community can be described from different perspectives and none of these is false in the sense of being contradicted by another, more is involved in ethnography than just reporting existing phenomena. . . . The freedom I described in ethnographic conceptualization signifies that the culture patterns presented in an ethnographic report are not reflections of reality but to a great extent represent constructs dependent on factors inherent in the ethnographer and independent of what is perceived." (1980, pp. 280, 282).

usually psychiatrists. There is a strong movement in Anglo-American psychiatry towards a resomatization of mental disorders, a movement which carries with it unusually strong claims about the transcultural validity of psychiatric diagnostic categories, even when they are based on a symptomatology of human behaviors. My informal experience is that ethnographers are often not well prepared for a scientific scepticism about these claims, and, more generally, that functionalist and "emic" approaches, when applied to one's own society, have difficulty in distancing themselves from the status quo and envisioning alternative concepts and approaches. This second trend is potentiated by the third: there are many signs that the 50-year respite in broad societal concerns about drinking, a respite which was a break with the previous century of American history, is now over. In gross terms, concerns about one's own drinking and about alcohol problems in the society seem to be shifting their cultural position in North America from a rural, poor, conservative constituency back to an urban, middle-class, progressive constituency. To the extent these trends are mirrored among anthropologists, future American ethnographers may carry a very different cultural baggage concerning alcohol into the field.





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