

COMMENTARY
BY
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Louise Erdrich in her 1984 novel, *Love Medicine*, describes a poignant gathering of three generations of a Northern Plains Indian family following the death of a young aunt. (This woman, inebriated, dies when she tries to walk home in sub zero winter weather.) The expression of this family's grief gets intermixed with accumulated family and racial tensions as the evening proceeds. Alcohol accelerates these processes which then culminate in an outbreak of family violence. One of the younger men, King, drunkenly tries to drown his wife in dishwater and is beaten off by another in the family. The scene ends with King "confused. . .wide-eyed" trying to run off in his pickup.

Such an episode, all too familiar to clinicians and researchers alike working with American Indians, remains at its core a mystery. Is this intergenerational outburst of rage and alcohol consumption an historical/cultural phenomenon? Is it the expression of two century's endurance of oppression and exploitation of the American Indian by the White majority culture? Does the use of alcohol provide a return of the Indian male to his former warrior status (Mohatt, 1972)? Or is it a developmental phenomena which gets passed down across generations as learned behavior; that is, as children identify with their parents' drinking behaviors. Or is it a sociological phenomena of family breakdown secondary to unemployment, isolation, poverty, and despair? Or, finally, is it a biological/genetic phenomenon related to a particular, as yet unidentified, vulnerability to the metabolism of ethanol?

Dale Walker and his colleagues in Seattle have undertaken an ambitious longitudinal study of alcohol and substance abuse across two generations of an urban Indian sample by *not* specifically trying to answer any of the above questions as posed in their traditional theoretical garb. Rather, their approach has been a vigorously empirical attempt to bring state-of-the-art quantitative methods to bear on these enormously important issues. Their conceptual foundation doesn't ignore the traditional hypothesized causes of alcohol and drug abuse, but rather reframes them into risk factors that can be specifically measured by specific instruments. They posit that an accumulation of risk factors, taken from any or all of the above domains, will provide predictive power to better understand why an American Indian drinks at all, drinks moderately, or drinks to excess.

Such an additive risk factor model has worked well for other empirical researchers. Lewinsohn, Hoberman, Teri, and Hautzinger (1985), for instance, conceptualized depression as a final pathway, resulting from a variety of genetic and/or environmental factors that intermingle

in particular ways for each individual so affected. Rutter (1980) was one of the first to show the cumulative effects of psychosocial stressors in predicting children's psychopathology. He showed the greatly increased effects (more than simply additive) of accumulation of these stressors on outcome. Thus, Walker and his colleagues are on solid ground in conceptualizing their study in this fashion.

Another strength of their longitudinal design allows for measuring the *onset* of alcohol and drug abuse, rather than relying on a retrospective report by the research subject. The hazards of the latter are all too well known to behavioral scientists, but constitute the bulk of our current knowledge.

There are too many noteworthy features of this study design for me to underscore them all. Thus, I will select two: (a) sample selection, and (b) attrition.

First, the care with which this research team has constructed their several samples is an example of sophisticated thoughtfulness whose real beauty may not be at first apparent. Noting that an exclusive school based sample would likely miss important youth and families not in that particular system, they have wisely chosen (with an enormous extra effort) to also include 66 subjects from a non-school source (local Indian Health Board). This allows them to detect potential biases inherent in the former group. (Luckily, so far the two samples appear very much alike in most demographic parameters, except family income.)

The other impressive design feature of their sampling technique is the additional use of cross-sectional cohorts. This will allow Walker's team to compare data from the longitudinal groups with cross sectional groups at the same ages, but at different times. Why is this important? It will allow them to spot "cohort effects"; that data peculiar to that particular sample in time but not representative of more inherent properties of the sample itself. If the longitudinal and cross sectional samples bear the same fruit, Walker and colleagues will be able to convincingly argue that their findings are not just the result of a particular idiosyncratic cohort but are more generic to the sample itself. One does not often see this design feature in most longitudinal work.

The other more obvious, but extremely impressive, feature of this report is the phenomenally low attrition rate for the first five years of effort. Attrition is the great threat to the meaning of any longitudinal research, for every time a subject drops out, the remaining sample has been altered. Often, the more symptomatic or less functional individuals are more apt to drop out. What is left can be an artificially better appearing sample than is really the case. This in turn limits the generalizability of any of the results. Readers who are not familiar with what goes into maintaining over 90% of a sample such as this may not freely appreciate this significant achievement. Getting the initial sample, then keeping the same sample over time is the challenge! This paper clearly describes in some detail how the research team has achieved this success. The authors show that it is not merely money, magic, or good luck that kept these several hundred individuals

from over 50 different tribal affiliations in their study, but rather an extraordinary attention to "little things"; snacks at the time of the interview, a newsletter, and most of all, a personal relationship with a known research staff person. Our experience of recruiting and maintaining a Cambodian sample of research subjects completely resonates with this description. We found our subjects had to trust the research assistant and be in agreement with the overall aims of the project before anything else, including payment, would be meaningful (Sack et al., 1993). Money is not as important an incentive to participate as one might think.

When does working hard to maintain a research cohort become some form of intervention in itself? The authors raise this question themselves in their discussion of these issues of follow up. I know of no clear answer to this dilemma. But one possible solution is to create a modest "process" measure. Questions such as, "Did your involvement in our research project cause you to seek help that you might otherwise not have undertaken?" is an example of this kind of item. The rule is, if you can't control it in your design, then try to measure it!

This paper should be of particular help to young researchers wishing to design their own projects. It is the kind of "how to" paper that rarely appears in the literature. I applaud the authors for having taken the time and trouble to write it (another reflection of their attention to detail), and for their strong commitment to the generation of a new knowledge by having the patience and persistence to pursue their answers in the right way.

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