

COMMENTARY
BY
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Research on Indian mental health and substance abuse issues has long been problematic. From the early days of anthropologic research which was ethnocentric and intrusive, to some present day research that has served largely to harden stereotypes about Indians (e.g., on alcoholism, suicide, and depression), there have been few studies that have successfully achieved a balance between cultural sensitivity, appropriateness of content, and methodologic rigor. For this reason, it is a pleasure to see a study like Walker et al., which does strike that balance.

Perhaps in part because Walker et al. are themselves Indian, they approached this research in a thoroughly culturally sensitive manner. For example, they were able to avoid the common temptation to “hunt for train wrecks” among Indian people. Rather, they carefully defined alcohol use so as to avoid inclusion of false positives, chose cases carefully to exclude individuals who were unusual in some way, and relied on structured instruments which they examined for cultural sensitivity, rather than the more common practice of asking overly simplified and overly broad questions which do not engage the subject in their own cultural context.

In choosing instruments, however, it should be noted that these researchers found themselves in a dilemma which every culturally sensitive researcher confronts. If one uses instruments which have wide acceptance in the field, they may not be culturally sensitive. But if you develop a culturally sensitive instrument for a particular group, the results may not be comparable with the literature at large. Certainly, researchers should continue to develop culturally sensitive instruments. But it is also important to apply both those instruments and similar mainstream instruments to the same subjects, and try in some way to make a translation between the results. The subjects should be both from the cultural group under study and from a comparison group (e.g., Caucasians). Only then will we be able to truly compare results across cultures. Because Walker et al. used a wide variety of instruments, some of which are mainstream and some of which were developed by their group, perhaps they will be able to make a start on this task.

Appropriateness of content is a second area in which this study is strong. That is, these researchers have chosen to study risk factors for pathology, rather than jump into the amorphous “prevention” field. It is *au courant* in Indian country to fund “health promotion” and “prevention” activities as though they were of proven effectiveness, although often there is no real focus to these efforts, and often no evaluation of effectiveness. The

theory seems to be that if people eat right, sleep well, and treat one another respectfully, they will be happier and healthier, and if they are happier and healthier they will not get sick. In some limited instances, this may be true, although little data supports this notion in mental health or substance abuse. However, risk factor research is one area of inquiry which can lead to focused prevention activities that lend themselves to evaluation for effectiveness. Put simply, in most instances we do not know where to put programmatic dollars to decrease the incidence of mental/substance abuse disorders. Only by identifying specific risk factors which are associated with these conditions will be able to approach true prevention.

The present research is a good example of the best of risk factor research. It is prospective, longitudinal, and focused. As noted above, the pathological conditions being studied are well defined, and the subjects carefully chosen. Those who run clinical programs which are conceived of as preventing substance abuse or other mental disorders would do well to read the methodology of this study, and having done so think carefully about what risk factors they are trying to target and upon which they will base evaluations of their programs.

The methodologic rigor in Walker et al. is also impressive. Yet, there are always trade-offs in research, some of which the investigators discuss. A few other trade-offs should be mentioned, as well.

First, there are many instruments used in this study, yielding a lot of data points. This makes a great deal of sense, as it is clear that in research on potentially stigmatizing issues such as substance abuse, multiple measures of the same variables are necessary. Yet, there are two problems which arise from this strategy. First, which of the multiple measures does one accept as "truth"? A strategy must be worked out in detail to deal with this problem. Second, even though the number of cases in this study is high as Indian studies go, still, there are not enough cases to effectively make use of all the data collected. Even in multivariate analyses, the numbers of cases will support only a limited number of variables. There are statistical strategies to reduce the data, of course, which should be utilized. But I also would encourage the researchers to focus much of their efforts on a few well chosen data points on which they can do time-series analysis. One of the strengths of this study is that it is longitudinal. Such an analytic strategy would allow them to make the most of these unique longitudinal data.

There is one more issue which bears mentioning. This is the problem of comparing rates between Indians and Caucasians. Clearly, this is a descriptive epidemiologic study, rather than a hypothesis testing study, so there is no need for a control group, per se. However, it is also clear that others will use these data to compare with data collected in studies of the majority population. This is problematic for several reasons. The first is discussed above — there may be difficulties in comparing these data (collected with culturally sensitive instruments) and data

collected with other instruments. Second, there is the real possibility that even using the same instruments, Indian people (or any specific ethnic group) may exhibit a systematic difference from Caucasians in patterns of response. If these patterns do not relate either to risk factors or psychopathology, they represent systematic response bias. Part of this potential for response bias is related to cultural differences and the cultural sensitivity of instruments noted above, but it is also related to the care with which the ethnic specific data are collected by rigorous, persistent, and culturally sensitive researchers such as the Walker et al. group. For example, Indian adolescents and families may have been more candid in answering sensitive questions than were persons in majority studies precisely *because* the researchers had a good relationship with the families and because the families saw the study as helpful to the Indian community. If so, rates for non-Indian people might be lower than those found in Indians because the Indian respondents are being more truthful.

One (not very serious) answer to this dilemma would be for these researchers not to work as hard at relating to the subjects and following them over time. Another, more reasonable, answer is to do studies of potential comparison groups with the same rigor as is found in Walker et al. That is, researchers working with other populations need to be encouraged to spend a similar amount of effort relating to and following subjects. Perhaps in some existing studies of Caucasians this has been done. If so, the choice of studies for comparison with the Walker et al. study should be based on whether those studies have achieved a similar degree of rigor with regard to basic methodology, relationship building, and followup.

The above caveats notwithstanding, this research promises to provide us with enormously useful information about risk factors in Indian adolescents and women. It is becoming harder than ever to do longitudinal research, so it is important that the few longitudinal studies which do get funded are designed with the degree of care found in Walker et al., and that future studies make attempts to solve some of the dilemmas noted above.

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