American Journal of Community Psychology, Vol. 21, No. 3, 1993

Response to Malgady and Rogler

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We regret the scholarly lapses that resulted (a) in our previous failing to include Dr. Malgady and Dr. Rodriguez in the citation to *Hispanics and Mental Health: A Framework for Research* (Rogler, Malgady, & Rodriguez, 1989), and (b) in our attribution of a hypothesis to Rogler et al., that they do not claim. While we have no excuse for the first error, our second error was due to our impression that Rogler et al. had eloquently applied our study-specific comment to a more general pattern of mixed results that they reviewed. We agree that the hypothesis, however generated, is still compelling and we happily reclaim it.

Of more interest to the reader than our scholarly lapses are the methological issues raised by Malgady and Rogler (1993) regarding our comparisons of Puerto Ricans, Mexican Americans, and non-Hispanic whites (Shrout et al., 1992). They imply that our results are compromised by (a) our use of lifetime mental health measures; (b) the Diagnostic Interview Schedule (DIS) itself, especially its probes; (c) our survey study design; (d) our failure to consider interactions of ethnic group with demographic variables; and (e) our insensitivity to the effects of culture on mental health assessment. These are serious charges indeed, and deserve careful examination.

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0091-0562/93/0600-0389$07.00/0

To gain some perspective on this discussion, let us review our central results. We found important differences between Hispanic groups in levels of affective disorder, alcohol abuse and dependence, phobic disorder, psychotic disorder, and somatization, but the differences were more attributable to immigration status than country of origin. We also found that comparisons with non-Hispanic whites were not greatly affected by the nature of the mental health outcome variable, whether symptom counts or diagnoses. Finally, we reported for the first time comparisons of DIS symptom counts based with and without structured probes. We found that probes could have an important effect for psychotic symptoms.

CRITICISM 1: LIFETIME MENTAL HEALTH MEASURES

Our first mistake, according to Malgady and Rogler, is the use of lifetime measures of mental disorder. They say that fallibility of human memory undermines self-reports of symptoms over a lifetime diagnosis is likely due to error.

We share our critics' concern for the effects of memory on self-report of mental health status. In fact, it is that concern that, in part, led us to use DIS lifetime measures of mental disorder rather than measures of current pathology. As we stated on p. 736 of our article, the year, month, or week estimates of DIS disorder depend on additional questions about symptom dating, whereas the lifetime reports do not. Malgady and Rogler assert that the dating information is especially unreliable, and yet this is what they would have to use to avoid lifetime prevalence rates.

For the purposes of our analyses, we stand by our decision to use lifetime information. Not only do we prefer to avoid using less reliable dating of symptoms in our analyses, the higher base rate in the lifetime rates of disorder makes reliability somewhat easier to obtain from a statistical point of view (Shrout, Spitzer, & Fleiss, 1987). Their assertion that the larger variance due to higher base rates is likely to be error has no empirical or statistical basis. Moreover, we demonstrated empirically that the symptom scales based on lifetime reports have generally acceptable internal consistency reliability in all groups we analyze.

In addition to their charge that lifetime information is too unreliable to analyze, Malgady and Rogler suggest that our results might be invalid because one or two of the three groups may have more memory problems

than the others. They accuse us of blindly assuming that the Hispanic groups and Anglos have equally good memories, but they present no empirical or theoretical reason for the contrary. While we admit to making the assumption of equally good memory in these groups, we do not admit to doing it blindly. Indeed, we have participated in ongoing studies of memory effects on retrospective reports (Rubio-Stipec et al., 1993), and have found empirical evidence for memory differences according to age and education. In part because of our concern for these artifacts, we controlled statistically for these variables. To our knowledge there is no similar empirical evidence to support Malgady and Rogler's position. They certainly have not provided empirical support.

CRITICISM 2: THE DIAGNOSTIC INTERVIEW SCHEDULE

In addition to their questions about the use of lifetime measures of mental disorder, Malgady and Rogler raise more general questions and issues regarding the Diagnostic Interview Schedule. These are concerns that they have also published elsewhere (Malgady, Rogler, & Tryon, 1992). Their polarized position regarding the DIS is well known.

We ask the reader to note that our original submission adopted a critical but pragmatic attitude toward the DIS. We claim on p. 737, "Although it might be appropriate to reserve judgment about the final validity of DIS diagnoses in community surveys, there were no clearly better alternative interviews for measuring DSM-III diagnoses in general populations at the time these data were collected." Malgady and Rogler have not identified a clearly better alternative in their various critiques of the DIS. We know the DIS has some weaknesses, but it has many strengths that have enabled us and others to contribute cross-cultural findings.

Criticisms of the DIS that have direct bearing on our findings do deserve comment. Malgady and Rogler are incorrect when they assert that "the DIS yields treated, not true prevalence data." They apparently believe that the DIS probe structure requires consultation with a mental health service before a problem is considered sufficiently severe to be a symptom of disorder. In fact, severity of a reported problem is assessed by asking respondents not only whether they sought help from a physician or other professional but also whether *the symptom significantly affected their daily living.* Malgady and Rogler's claim that DIS diagnoses require mental health treatment is simply misinformed. Moreover, in Puerto Rico we adapted the probe flow chart so that "professionals" included spiritualists, *santeros,* healers, and other informal resources used by Puerto Ricans seeking help for distress.

DIS estimates certainly include both treated and untreated cases. Indeed, there is substantial research on the characteristics of DIS cases who have never sought professional treatment (for a review, see Robins & Regier, 1991).

Malgady and Rogler's polarized critique of the DIS made use of one of our original findings, but they tried to use that finding to question our entire contribution. Our analyses of DIS symptom counts with and without probe information indicated that certain probes for psychotic symptoms introduce ambiguity into the results. For those symptoms, interviewers are allowed to rate problems as "plausible" in the probe section. We found that the rates of plausible problems differed across sites, and thus we advised caution in interpreting the results for psychotic symptoms and disorders. Our critics found it convenient to ignore the fact that this result is disorder-specific and to generalize their DIS-gloom to the entire instrument. We remind them and the reader that for affective disorder, alcohol abuse/dependence, phobic disorder, and somatization, the results with and without probes yielded similar patterns of results. We stand by our results for these disorders.

CRITICISM 3: STUDY DESIGN

Malgady and Rogler correctly note that identical survey designs were not used in the Puerto Rican and Los Angeles surveys. In Los Angeles the entire DIS was used and the household was interviewed only once. In Puerto Rico an abbreviated DIS was contacted twice. Malgady and Rogler raise the concern that the different designs could obscure the comparability of the findings. On one hand, fatigue from the longer DIS in Los Angeles could diminish levels of symptoms toward the end of the survey, and on the other hand the repeated interview design in Puerto Rico could dampen response levels at the second (DIS) interview.

We agree that the design of surveys can have effects on response levels, but not enough is known at this time to say how large those effects are. Incomparable survey designs are especially likely to be a problem in cross-cultural epidemiology, since the culture-specific surveys are usually done independently. Exact comparability of methods is difficult to obtain under these circumstances.

Some clarification of the Puerto Rican design is warranted. Although the DIS survey did indeed follow a governmental health survey, it was not done at the same time, and in many households it was not carried out with the same respondent. The health survey used any available household

informant, while the DIS survey selected a specific respondent. Because different respondents were sometimes interviewed, and because the nature of the two interviews were so different, we do not believe it is likely that the Puerto Rican results were much affected by the drop-off in rates that has been reported in panel studies of mental disorder.

CRITICISM 4: INTERACTIONS WITH DEMOGRAPHIC VARIABLES WERE NOT INVESTIGATED

Malgady and Rogler were critical of our focus on main effects rather than on multiple possible moderating effects. They gave as one example of an additional analysis we could have done, a gender by group interaction for alcohol abuse/dependence. They formed this hypothesis from looking at our previously published data.

Although we have no doubt that there may be additional analyses of interest in the cross-cultural comparisons that we began, the goal of our analyses was to investigate overall Hispanic heterogeneity in mental health status. Our results that immigrant status among Mexican Americans was more prominent than national status would not be affected by additional analyses of interactions.

We may find extensions of our analyses to be interesting, but we do worry about the possibility of Type I errors in the exploration of statistical interactions. Hypotheses formed after looking at the data are especially susceptible to inflated Type I error.

CRITICISM 5: CULTURAL INSENSITIVITY OF OUR FINDINGS

Malgady and Rogler state that the most serious flaw in our article is the neglect of culture in all phases of the diagnostic and analytic process. They cite a previous paper of our group (Canino et al., 1987) as an example of how we should have proceeded. In the 1987 paper we suggested that cultural consideration argued for changes in the diagnostic algorithms of dysthymia, cognitive impairment, psychosexual dysfunction, and obsessive compulsive disorder. The other DIS disorders did not seem to require algorithm changes.

Malgady and Rogler neglect to note that our exemplary paper actually justifies our use of all the variables included in our 1992 analysis, with the possible exception of affective disorder. The possible exception pools

dysthymia with major depression and bipolar disorder. Had we implemented the modification for dysthymia, there might have been a slight increase in the rate of affective diagnoses for Puerto Ricans, but it would not have affected the two symptom counts. Given that the identical pattern of results was found for all three ways of characterizing affective disorders (Puerto Ricans had higher levels of affective problems than Mexican American immigrants, but lower levels than U.S.-born Mexican Americans and Non-Hispanic whites), it is highly unlikely that this modification would have altered our findings for this category of mental disorder. For the other four categories, our previous research justified our measures.

THE NEED FOR CONSTRUCTIVE CRITICISM IN CROSS-CULTURAL RESEARCH

There is no doubt that cross-cultural research presents many methodologic challenges, as does all research on mental health in the general population. These challenges require a critical perspective and constant improvement of existing methodology. We think it is unlikely that any study will be perfect, and thus we need to be constantly open to criticism.

For this reason we sent our working manuscript (complete with its scholarly lapses) to Prof. Rogler well before it was published, and we sent a copy to Prof. Malgady at a later date. We heard nothing from them until the editor sent us their submitted critique. We hope that the readers find some aspects of our interchange helpful, but we would have preferred to have improved our original paper prior to publication, and to have helped them clarify their critique prior to this public exchange.

We remain chagrined over our scholarly errors, but we believe that the methodological criticisms raised by Malgady and Rogler do not undermine the findings we reviewed above. Like other commentators on their critiques (Lopez, 1988), we wish that Malgady and Rogler would present more empirically based suggestions for improved research of mental health in different cultures.

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