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More on Reporting Sex Differences

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I read with interest Alice Eagly's (July 1987) comment about routinely reporting sex comparisons, whether or not the findings are hypothesized, replicated, or theoretically relevant. I would like to expand on two further points regarding the way in which sex differences are reported in the psychological literature.

First, there is an assumption that research in psychology is value free and unbiased regarding gender roles. In reality, twice as many articles in personality journals used male subjects as female subjects (Carlson, 1971), and 75% of articles in animal behavior journals used male animals (Hyde & Rosenberg, 1980). On average, over 20% of articles in personality journals (Carlson, 1971) and 10% in behavioral journals (Resick, Calhoun, Rothblum, Dartnall, & Blechman, 1986) do not even state whether the subjects were male or female. This tendency has increased in recent years; over 20% of articles comparing groups on behavior therapy techniques do not state whether the subjects were male or female (Resick et al., 1986). Even when sex of subjects is specified, the majority of articles do not report analyzing data for gender differences or discuss gender differences or similarities in the discussion section (Resick et al., 1986).

Second, even when gender differences are discussed, there may be implicit biases reflecting the author's attitudes toward women. In a recent article in the *Psychology of Women Quarterly*, Michelle Fine (1985) analyzed the conclusions of articles appearing in that journal during

a five-year period. She found three commonalities in discussion sections: (a) the assumption that progress and societal change will be rapid and will eliminate current problems facing women (e.g., stating that passage of the Equal Rights Amendment will help women's opportunities, or that improved math skills will help women overcome powerlessness and low status in their jobs); (b) the tendency to attribute women's stress and distress to individual dynamics rather than societal conditions (e.g., suggesting that women should reject nontraditional jobs rather than discussing the social realities facing women as they enter the labor force); and (c) the suggestion that change focus on women with specific problems rather than on society (e.g., advocating assertion training for battered women).

In sum, I would argue that rather than to ignore gender issues unless these are part of the hypothesis or theory of the research, it would be preferable to highlight gender similarities as well as differences whenever possible in the method, results, and discussion sections of research articles. This is an important way to eliminate unfounded assumptions about gender and gender roles.

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The Empirical Basis of Ethnocultural and Linguistic Bias in Mental Health Evaluations of Hispanics

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Malgady, Rogler, and Costantino (March 1987) should be commended for concisely addressing some of the potential biases in mental health evaluations of Hispanics. Their efforts should serve to raise the consciousness of therapists treating this population. The empirical basis to their claims, however, is very limited. The authors rightly pointed out that as a whole, the Hispanic mental health literature is

plagued with serious limitations. Yet they used some of this and related research without considering the specific limitations of the research on which they based their major points. In this commentary, I point out some of the limitations of the research that they presented and discuss the implications of these limitations for further research.

Bias in Evaluations of Psychopathology

To support their general claim of bias in the psychological assessment of Hispanics, the authors cited two psychiatric "epidemiological" studies (Baskin, Bluestone, & Nelson, 1981; Gross, Knatterud, & Donner, 1969) and a small number of other studies that indicate that Hispanics are more likely to have higher "prevalence rates of psychological disorders" than Whites. There are many problems with the literature they cited, but the most important limitation is that one cannot conclude that ethnic differences in treated prevalence rates are due to bias on the part of the mental health system or professional. Such differences could also reflect true ethnic differences in the types of symptoms and disorders of those who use mental health centers. This is not to say that Hispanics as a group suffer more mental disorders or symptomatology than other groups; current state-of-the-art epidemiological data (Burnam et al., 1987; Karno et al., 1987) indicate that in Los Angeles, Mexican Americans do not differ greatly from White non-Hispanics in regard to lifetime and six-month prevalence rates of most mental disorders as assessed by the Diagnostic Interview Schedule (Robins, Helzer, Croughan, & Ratcliff, 1981). However, the clinical picture of Hispanics who use services may differ from the clinical picture of persons from other ethnic groups who use the same services.

To claim that bias exists using the data presented by Malgady et al., one needs to demonstrate that the reported ethnic differences in patients' diagnoses do not reflect actual differences between groups. No such research has been conducted in regard to Hispanics. There is, however, one such study that examined race bias in the diagnosis of schizophrenia and affective disorders. Simon, Fleiss, Gurland, Stiller, and Sharpe (1973) compared the hospital staff's diagnoses of Black and White inpatients with a research project's diagnoses of the same patients. The assumption of this methodology is that the project diagnoses were more accurate than the staff diagnoses given that the project diagnoses were derived from structured interviews that were likely to