

The present article offers a critique of the semipartial correlation approach to abuse effects research. This procedure attempts to "control" statistically for the potentially confounding effects of family environment in the relationship between child abuse and later psychological symptomatology. Unfortunately, the use of such statistics in abuse research may be problematic, especially when control, predictor, and criterion variables are significantly intercorrelated (multicollinear) and the intent is to determine the relative importance of abuse to later psychological functioning. Other problems discussed include the effects of (a) small sample sizes, (b) unreliability of the control variable, (c) ambiguity regarding the causal relationship between control and predictor variables, and (d) theoretical problems inherent in considering abuse "minus" family dysfunction. It is suggested that semipartial correlation analysis can be a highly conservative test of the "abuse effects" hypothesis.

Controlling for Family Variables in Abuse Effects Research A Critique of the "Partialling" Approach

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The last 10 to 15 years have witnessed a significant increase in research on the effects of child abuse, especially in regard to the impact of sexual exploitation. As noted by Browne and Finkelhor (1986), most recent studies in this area find that women with childhood histories of sexual abuse are more likely than their nonabused peers to report psychological problems as adults. The question is, of course, whether these correlates of sexual abuse

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represent effects, or whether both history of abuse and adult symptomatology covary as a function of some common third set of variables. The importance of this issue—whether abuse produces later symptomatology or merely correlates with it—cannot be overstated, since therapeutic and public policy decisions often rest on assumption regarding the causes and effects of child maltreatment.

"Third variables" most often suggested by potential critics of abuse-effects research are those involving disrupted or dysfunctional family systems. This perspective posits that chaotic or conflict-laden families may produce various types of child abuse and neglect, along with other more subtle traumas (e.g., "object loss" arising from paternal abandonment, confusion due to inconsistent parental roles), unknown combinations of which may produce long-term effects. Thus, from this viewpoint, a history of sexual abuse may covary with symptomatology because such experiences reflect (or are an example of) a broader family dysfunction.

In contrast to a "family dysfunction" perspective, an "abuse-related" analysis might see the association between early victimization and later symptomatology as more directly causal. This viewpoint does not discount family disturbance, but sees physical or sexual abuse as having specific effects in addition to those of family dysfunction *per se*, and emphasizes notions such as exploitation and mistreatment as specific causal factors (Finkelhor & Browne, 1985).

Unfortunately, the data available to us at present can be interpreted as evidence for either a family or an abuse perspective. Family conflict or disruption and parental loss or absence have been associated with long-term psychological difficulties, just as have sexual and physical child abuse. Further, family problems often constitute "risk factors" for childhood sexual abuse (e.g., Finkelhor, 1980) and, in fact, it appears that all of these variables are intercorrelated. Ultimately, we are left with the question, "What causes what?"

There are several reasons why this question has yet to be definitively answered. The most substantial problem in this area involves a well-known statistical conundrum: If two or more predictor variables are intercorrelated (a phenomenon sometimes referred to as "multicollinearity"), and each are correlated with the criterion variable, it is quite difficult to determine the unique (nonredundant) contribution of any given predictor to the variance in the criterion. In the words of two noted statisticians in this area, "redundancy among explanatory variables is the plague of our efforts

to understand the causal structure that underlies observations in the behavioral and social sciences" (Cohen & Cohen, 1975, p. 86-87). Stated in the current context, since negative family variables are often associated with sexual abuse (e.g., Finkelhor, 1980; Fromuth, 1986), and since family problems and sexual abuse are each associated with later psychological difficulties (e.g., Bagley & McDonald, 1984; Briere & Runtz, 1986, 1987; Browne & Finkelhor, 1986; Dorpat, 1965), there is no easy way to determine how much of adult symptomatology is "due" to abuse as opposed to family influences.

Despite such problems, however, researchers continue to be interested in the extent to which abuse can be shown to exert a unique effect upon later symptomatology. Recently, this has been attempted through the use of "partialling" procedures—multivariate statistical methodologies that explore the relationship between child abuse and adult symptoms when the variance shared by child abuse and family variables has been removed ("controlled for" or "partialled out"). The intent of the current article is to suggest that under certain conditions such statistical adjustments are, at best, very conservative tests of abuse hypotheses, and that the results from such procedures, if not fully understood, can lead to erroneous conclusions. The bases for this concern lies in two related realms: the statistical limitations of "partialling" procedures, and theoretical problems with artificial variables.

STATISTICAL ISSUES

The basic model for altering a predictor (e.g., sexual abuse) by removing its communality with another predictor (e.g., family background) is derived from the Multiple Linear Regression approach. This model allows one to examine the unique ability of two or more variables ($X_1, X_2 \dots X_p$) to predict the variance in an additional "criterion" variable (Y). By "unique," we mean the extent to which X_p can predict Y above and beyond the extent to which any other X or combination of X s can predict Y . The statistic that allows this determination is the *semipartial* (sometimes referred to as "*part*") *correlation coefficient*.¹ In the current context, this coefficient reflects the degree to which a history of sexual abuse (X_2) adds to our ability to predict symptomatology (Y) beyond that which can be predicted by family dysfunction (X_1) alone.² This relationship may be expressed algebraically in the following formula:

$$sr^2 = r^2_{Y(X_2, X_1)}$$

where sr^2 refers to the squared semipartial coefficient, and X_2, X_1 refers to X_2 where any variance shared with X_1 has been removed.

Herein lies the problem: If X_1 and X_2 are correlated (overlap), a semipartial analysis will "give more of the credit" to X_1 (family dysfunction) than may be appropriate, and thereby cause it to appear as if X_2 (abuse) is not as important in the prediction of adult symptomatology as may actually be the case. Pedhazur (1982, p. 167) notes, in the extreme case of this dilemma, that "partialling out from one predictor another predictor from which it is highly correlated will generally result in a small, even meaningless, semi-partial correlation." Pedhazur, in fact, stresses that semipartial analysis is not "intended to provide information about the relative importance of variables, but rather about the effect of a variable(s) after having controlled for another variable(s)" (p. 178).

The problem of "variance stealing" (Cohen & Cohen, 1975) by the control variable (e.g., family background) is of special concern in certain situations:

- (1) *If the control variable is unreliable.* Cohen and Cohen (1975, p. 370) note that measurement error in the control variable "may decrease or increase, or even change the sign of, a partial relationship."
- (2) *If the sample size is small*, such that the control variable may artifactually account for more variation than appropriate—a process referred to as "capitalization on error variance" (e.g., Cohen & Cohen, 1975).
- (3) *If the causal or directional relationship between the control and X_2 variables is unknown.* Various writers (e.g., Fisher, 1958; Gordon, 1968; Pedhazur, 1982) have noted that it is inappropriate to "control" for X_1 while examining the role of X_2 on Y if either (a) there is a possibility that X_2 caused X_1 (e.g., that sexual abuse may produce family dysfunction), or (b) that X_1 and X_2 represent different measures or components of the same construct (resulting in what Gordon [1968] refers to as "partialling the relation out of itself"). Both of these points may be especially relevant to abuse effects research, where active intrafamilial abuse undoubtedly contributes to a negative family environment, and where both variables may have a synergistic effect on later psychological adjustment.
- (4) *If the sample underrepresents abuse severity.* Finally, the results of partialling approaches can vary according to the type of sample used to study sexual abuse effects. For example, nonclinical samples (especially involving students) may not include many severe cases of

sexual or physical victimization (Runtz & Briere, 1986), and thus any "abuse effects" may be trivial in comparison to the variance accounted for by the "control" variables. Fromuth (1986), for example, examined the relationship between broadly defined sexual abuse and symptom checklist scores in a sample of university women, controlling for "family support," and found that although abuse was slightly correlated with symptomatology, this relationship ceased to be significant when the (more strongly correlated) family variable was accounted for. On the basis of her semipartial analysis, she concluded that "the long-term effects often attributed to the sexual abuse then may not be actually due to the abuse per se" (p. 14). Fromuth nevertheless notes that the failure of sexual abuse to predict symptomatology after family support was removed may have been partially due to the lower levels of abuse severity present in a sample as "high functioning" as college students (Fromuth, personal communication, April 16, 1987). In other words, had severe abuse been more prevalent (as is likely in clinical samples: see Runtz, 1987, for a comparison of clinical versus nonclinical abuse parameters), effects of victimization might have exceeded family support effects and produced considerably different semipartial results.

THEORETICAL CONCERNS

In addition to statistical issues, a theoretical difficulty arises when we partial out everything correlated with family problems from child abuse—what does "child abuse" represent when family dysfunction has been removed from it? To the extent that this new, residualized variable is more a statistical concept than a "real world" event, its relation to other variables (e.g., symptomatology) may be theoretically meaningless (Pedhazur, 1982, p. 110). In other words, since child abuse often occurs within the context of a disturbed or disrupted family environment, the variable "abuse without family dysfunction" may have little construct validity. Although this concern is most salient for intrafamilial abuse (e.g., incest), it would also hold for situation where family dysfunction places a child at greater risk for extrafamilial abuse (Finkelhor, 1980) and subsequent trauma.

ALTERNATIVE APPROACHES

If partialling procedures cannot directly establish the relative importance of correlated predictors, how can researchers "disentangle

the sources of trauma" (Browne & Finkelhor, 1986) in child abuse research? The most honest answer may be "we can't completely." As noted by Pedhazur (1982), there are no known statistical procedures that definitively indicate the causal relationship (or "true" importance) of several correlated predictors to a criterion variable. Thus, although three possible approaches will be presented here, the reader should keep in mind the adage that "correlation does not imply causation."

Complete multiple regression analysis. As was mentioned earlier, the semipartial correlation is equivalent to a "hierarchical" multiple-regression analysis, with the control variable entered at Step 1. As was also described, this procedure may assign too much importance to the Step 1 variable if it is not, in fact, causally antecedent to the predictor. Another form of multiple regression—known as the "simultaneous solution"—may alleviate this problem to some extent. A simultaneous analysis enters *both* the control (family) and predictor (abuse) variables at Step 1, so that the associated regression weights reflect the contribution of each variable controlling for the other (i.e., not just abuse controlling for family, but also family controlling for abuse). This may ultimately be a more "fair" test, since it allows comparison of the residualized portions of both the control and predictor variables.

A second advantage of a more complete regression analysis is the opportunity to consider formally the interaction of abuse and family background as it affects symptomatology. Specifically, multiple-regression analysis can include a "multiplicative interaction term" that evaluates the joint impact of both variables, and thus can address questions such as "does abuse affect psychological functioning more when the family is dysfunctional?" (Fromuth, 1986). In contrast, the semipartial analysis does not permit assessment of such synergistic relationships. It should be noted, however, that despite the additional information that may accrue from a multiple regression approach, the primary concern remains: In the face of correlated control and predictor variables, the "true" etiologic importance of each to psychological functioning may be unclear.

Within abuse group correlations. Given the potential limitations of multiple regression models in the analysis of abuse and psychological functioning, some researchers have approached this problem from another perspective: If abuse causes later symptomatology, then one might expect specific aspects of the abuse (e.g., victim age when victimization began, duration of abuse, type of act, age of abuser) to correlate with symptomatology as well. Conversely, if symptomatology is actually due to covariates of abuse, such as

family background, one might be less likely to expect specific abuse-related correlations with adjustment. In general, workers utilizing this approach in sexual abuse research have, in fact, linked greater symptomatology to variables such as duration or frequency of abuse (e.g., Bagley & Ramsay, 1985; Briere, 1987; Briere & Runtz, 1985; Russell, 1986; Tsai, Feldman-Summers, & Edgar, 1979), presence of intercourse (e.g., Bagley & Ramsay, 1985; Briere, in press; Briere & Runtz, 1986; Russell, 1986) and age of abuser (e.g., Briere & Runtz, in press; Finkelhor, 1979; Fromuth, 1986; Russell, 1986).

Causal analysis. Although correlations of abuse characteristics with later symptomatology imply specific contribution of abuse to later psychological adjustment, it is possible (although perhaps less likely) that certain negative family factors might selectively covary with certain abuse characteristics such that, for example, “enmeshed” family dynamics might be more likely to produce extended periods of incest. In such a case, a more direct evaluation of a potential cause-effect relationship might be indicated, such as is offered by path analysis or causal modeling. These related procedures allow one to test specific hypotheses (models) regarding the causal linkages among a number of variables, and permit consideration of variables that may mediate between “cause” and “effect.” Such procedures are only valid, however, to the extent that the sample size is adequate, the variables have been well operationalized and reliably measured, and the proposed model is based on adequate theory (MacDonald, 1977). In addition, even these more sophisticated methodologies cannot “prove” a specific causal order—they can only tell us how well a given model “fits” the data at hand (Asher, 1983).

CONCLUSION

In summary, it appears that the use of partialling control procedures can be problematic when the goal is to determine the relative *importance* of sexual abuse (or other variables) in the etiology of adult symptomatology. This procedure may be especially unacceptable when (a) the control, predictor, and criterion variables are intercorrelated, (b) the control variables are unreliable, (c) the sample size is small, or (d) the causal relationship between control and predictor is bidirectional, synergistic, or unknown. Additionally, the algebra of the semipartial correlation ensures a conservative test: In

order to be significant, abuse must correlate with symptoms after all variance shared with the control variable (e.g., family background) has been removed. Thus studies that show that abuse continues to predict later adjustment after "control" variance has been extracted (e.g., Bagley & McDonald, 1984; Bagley & Ramsay, 1985; Finkelhor, 1984; Peters, 1984) should be considered quite seriously, whereas those with negative findings (e.g., Fromuth, 1986) do not necessarily indicate that sexual abuse has no psychological impact.

It should be noted, in passing, that we need not categorically rule out the use of semipartial correlations in abuse studies. There are instances where such procedures are relevant, that is, in prediction as opposed to tests of "etiologic importance," or when the analysis is based on a known causal sequence. It is important, however, to understand the interpretational limits of our research technology, such that the leap from statistical findings to useful theory is not a perilous one.

NOTES

1. Although the present discussion centers on the use of semipartial correlations, similar concerns may be raised for related procedures such as partial correlation analysis or stepwise regression analysis.

2. The reader familiar with multiple regression will recognize this as equivalent to a "forward selection" or "hierarchical" procedure, where the control variable is entered at Step 1, and the "F-to-enter" for X_2 represents the significance test for the semipartial correlation.

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