

Methodological Issues in the Study of Sexual Abuse Effects

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Despite its relative infancy, child abuse research has provided a substantial literature on the psychological sequelae of sexual molestation. These findings have been helpful in informing social policy and guiding mental health practice. Because of the recency of interest in this area, however, as well as the costs and time investment associated with more rigorous longitudinal research, many of these studies have used correlational designs and retrospective reports of abuse. The implications of this methodology are outlined, and remedies are suggested where possible.

Research on the psychological sequelae of childhood sexual abuse has grown rapidly in the last decade. This burgeoning literature reflects not only increased scientific interest in sexual abuse and its potential effects but also growing public concern about this form of child maltreatment. Given findings that perhaps one third of women and one sixth of men in our culture have experienced sexual contact with someone substantially older by their mid-teens (Finkelhor, 1979; Finkelhor, Hotaling, Lewis, & Smith, 1989; Russell, 1986; Wyatt, 1986), research linking such events to later psychological symptomatology or distress would necessarily have substantial implications for social policy and clinical intervention.

Most recent studies do indicate that, in fact, childhood molestation is associated with multiple short- and long-term psychological difficulties. These correlations occur in nonclinical as well as clinical samples and are present in both men and women. Among the problems and symptoms that have been associated repeatedly with a childhood sexual abuse history are symptoms of posttraumatic stress, low self-esteem and guilt, anxiety, depression, somatization, dissociation, interpersonal dysfunction, eating disorders, sexual problems, substance abuse, and suicidality (Briere, 1989; Browne & Finkelhor, 1986).

Methodological Issues

Partly because of the recency of scientific interest in sexual abuse, research in this area has made use of relatively simple methodologies and designs. As noted elsewhere (Briere, 1988b), such research may be considered *first wave*: primarily focused on probing the link between childhood molestation and both proximal and more distal psychosocial difficulties. Typically, the psychological functioning of subjects who have been identified (or identify themselves) as sexual abuse victims is com-

pared with that of other subjects with no equivalent indications of sexual abuse. Differences found between these groups, assuming equivalence on background variables, are attributed to the sexual abuse experiences that discriminate them. When such differences are found in adults, many years after their abuse, it is generally assumed that childhood sexual victimization has long-term effects on psychological adjustment.

It is with regard to the generalization from sexual abuse correlates to conclusions about the long-term impacts of abuse that the sexual abuse literature is most vulnerable to inferential error. The following sections of this article outline some of the problems inherent in current sexual abuse research methodology and offer suggestions for ways in which research in this area might be improved. Although most of this discussion will focus on long-term correlates of childhood sexual abuse, many of the points raised here pertain to research on short-term sequelae as well.

Cross-Sectional Versus Longitudinal Designs

Subjects of sexual abuse research are often simultaneously questioned about (a) abusive events that occurred many years ago and (b) their current level of psychological functioning. Childhood abuse reports are then designated as independent variables, whereas subjects' responses on psychological measures are considered dependent variables. By virtue of the correlational and retrospective character of such designs, cause and effect can become blurred. Although one might assume that the victimization experiences reported by subjects antedate their current psychological functioning, the reverse is possible as well: Current distress or symptomatology may impact on respondents' retrospective reports of abuse. As will be described later, such difficulties may arise from the effects of time on recollection, as well as the influence of current psychological functioning on the accuracy of recall.

Cross-sectional research can be problematic when time-specific abuse sequelae are examined. Clinical experience and preliminary research suggest, for example, that abuse-related symptomatology can wax and wane across the life span (Friedrich & Reams, 1987). This variability may reflect developmental issues: For example, certain intimacy and sexual problems may emerge as the child victim grows to be an adolescent and

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confronts social and biological prodding to form sexual/romantic connections with others (Briere, 1989; Gelin, 1983; Maltz & Holman, 1987). Other sleeper effects first evident in adolescence or adulthood may include aggression, substance abuse, and proneness to revictimization. Assessment of these intermittent or stage-specific effects (Kagan's [1971] "heterotypic continuity") often suffers from methodologies that rely on one-shot adult retrospective reports. Such research requires the adult subject not only to recall that a given symptom occurred at some point in the past but also to approximate when the symptom in question began and ended.

Most important, cross-sectional research is rarely very helpful in discriminating abuse-specific from abuse-concurrent or abuse-antecedent events. In the relative absence of studies that examine the psychological functioning of children before and after sexual abuse has transpired (although see the *at-risk* studies of Egeland and colleagues [Egeland & Brunnel, 1979; Egeland & Farber, 1984; Egeland, Sroufe, & Erickson, 1983]), researchers have yet to determine (a) the premolestation functioning of sexually abused children, and thus the extent to which abuse "effects" represent at least partially preexisting risk factors or psychological disturbance; (b) the exact role of coexisting familial dysfunction and other forms of maltreatment; or (c) the impacts of social or demographic factors as they moderate or exacerbate what are thought to be simple abuse effects.

The most obvious resolution of the difficulties presented by retrospective, cross-sectional, and correlational designs is to avoid this form of data-gathering entirely. The ideal research plan for examining sexual abuse and its effects might involve a longitudinal study whereby (a) subjects and their families are randomly selected and evaluated before sexual abuse, so that baseline conditions and levels of psychological functioning can be ascertained; (b) a detailed assessment of the type and extent of sexual abuse is accomplished when it occurs in that unfortunate subsample; and (c) both abused and nonabused subjects are repeatedly and regularly studied as they progress through the life span, with within- and between-groups analyses performed to study the form and development of abuse-specific psychological effects. Less ideally, children identified as recently sexually abused could be studied with matched, nonabused children as both groups matured (e.g., Cohen & Mannarino, 1988), or subjects found to have been abused in the past (e.g., as reported in clinic or social casework records) could be followed up in the present to determine their psychological status (e.g., McCord, 1983).

Longitudinal analyses of sexual abuse effects can be subject to difficulties, however, despite their improvement over the one-shot retrospective study. Among these are (a) the method whereby subjects are initially selected (e.g., the potential difficulties in generalizing from solely at-risk subjects), (b) changes in measurement relevance or sensitivity as a function of subject age (i.e., subjects who outgrow childhood-specific psychological tests), (c) the potential effects of repeated measurement (*panel conditioning*) on both abused and comparison subjects (e.g., how to control for—as opposed to merely documenting—reactivity or monitoring effects if they occur), (d) subject attrition over time, (e) the potential impacts of treatment in the abused group during follow-up (i.e., its impact on the natural course of postabuse symptomatology), and (f) the problems

inherent in accurately matching nonabused to abused subjects in studies using previously identified abused children (e.g., how to adequately match on presence of abuse risk factors [Finkelhor, 1980] in children who were not, in fact, abused). Although much could be written on the methodology of longitudinal child abuse research, the costs and difficulties inherent in following subjects over extended periods of time have made such studies far less common in the child abuse literature. As a result, although in no way denigrating the greater value of longitudinal methodologies, I have chosen to focus this article on issues more common to the conduct of retrospective, correlational abuse research.

Report Biases

As indicated above, retrospective research on sexual abuse effects relies almost entirely on subjects' reports of past events. Yet, recollection of abuse in childhood may be affected by what Cicchetti and Rizley (1981) refer to as "the influence of contemporary adaptation on recall" (p. 40). For example, the adult who, by virtue of her need to avoid painful abuse memories, is amnesic for much or all of her childhood victimization (Briere & Conte, in press; Herman & Schatzow, 1987) may truthfully report no knowledge of having been abused. If such individuals are studied in sexual abuse research, their automatic inclusion in *no abuse* comparison groups might easily obscure or confound between-groups differences—especially if, as reported by Briere and Conte (in press), amnesic abuse victims are more symptomatic than their nonamnesic but similarly abused cohorts. Although conscious suppression of abuse reports may be partially ameliorated by experimenter assurances of confidentiality and attempts to provide an environment supportive of disclosure, the problem of repressed memories remains a significant concern.

The passage of time may also mitigate against accurate or complete recall of childhood traumas (Menard, 1991). There are little data available, in this regard, on the incremental impacts of passing time on subjects' specific recall of discrete life events. Similarly, age-specific socialization may influence subjects' willingness to report sexual abuse. Individuals who grew up in an earlier era, for example, may be more prone to suppress or deny abuse experiences by virtue of the greater secrecy and stigmatization associated with sexual victimization in previous decades. As a result, one cannot rule out the possibility that a *main effect* of abuse history on a given dependent variable may more accurately reflect an *abuse by time (or age) interaction* on said measure. An example of the potential impacts of time on recollection may be the cohort effects reported by Russell (1984). Russell found that older women report less molestation than do younger women, leading her to suggest that the incidence of sexual abuse may be increasing over the years. Although this is not an unreasonable hypothesis, it is also possible that the greater passage of time between childhood and interview for older subjects resulted in less complete abuse memories for these subjects. Additionally, these older subjects may view sexual abuse as more stigmatizing or embarrassing than do younger subjects, and thus they may underreport molestation, a process that may result in older abuse victims being inappropriately placed in *nonabused* comparison groups.

Despite the importance of accurate disclosures in the conduct of retrospective studies, researchers in this area have found no satisfactory way to ensure the validity of subjects' recollections of childhood sexual abuse (Briere, 1990; Briere & Zaidi, 1989). This problem has led some clinicians (and journal editors) to express concerns regarding "what is reported and what in fact happened" to subjects studied in abuse research (Rich, 1990, p. 1389). The question is often raised whether some sexual abuse reports reflect, in fact, fantasies, delusions, or intentional misrepresentations for secondary gain. Because there are almost no empirical data in this area, the possibility of abuse confabulation cannot be overlooked by researchers, even given the common clinical impression that such misrepresentation is rare (Briere, 1989). Among the ways that one might rule out abuse misrepresentation are (a) independently corroborating abuse reports from other sources, (b) restricting study to abuse cases that have been validated by the child protection or criminal justice systems, and (c) decreasing the potential rewards of falsifying one's childhood history.

Independent corroboration of adults' retrospective abuse reports is problematic in two respects. First, because sexual abuse often occurs in secrecy and frequently results in embarrassment or shame (Finkelhor, 1979), many victims report that they did not disclose their molestation to others at the time it occurred. As a result, a significant proportion of subjects would be unable to provide corroborating information were it required. Second, even if corroboration were possible, the intrusion and possible distress to subjects inherent in investigating the veracity of their reports (i.e., contacting and questioning relatives, neighbors, or friends) seemingly precludes its widespread use (Briere, 1990).

Because the majority of sexual abuse cases are never brought to the attention of authorities, data collection limited to subjects whose cases had been validated (*founded*) by child protection or criminal justice agencies would significantly reduce the number of subjects available for study. Further, given that founded cases often differ from others in terms of, for example, severity of abuse and the social class of the abused child, findings generated from such unusual samples are likely to have limited external validity.

Fortunately, concerns about secondary gain for falsely reporting sexual abuse are to some extent addressed by the typical abuse research paradigm, which does not reward or punish response to sexual abuse items, *per se*, and often does not record the subject's name or other identifying data. Under such circumstances, the motive for deliberate falsification of abuse history is less clear. Researchers should avoid, of course, paradigms that could reinforce abuse reports, such as offering money for participation in research solely to those who report childhood sexual abuse.

Given the above, it appears that the accuracy of sexual abuse reports cannot be assured, in terms of ruling out either false positives or false negatives. As a result, retrospective studies in this area cannot entirely guarantee the complete validity of their criterion variable. Future studies might include additional variables relevant to report bias, such as social desirability, tendency toward repression, and attitudes toward abuse disclosure, and researchers might especially endeavor to eliminate any conceivable study-specific rewards for reporting sexual abuse. Furthermore, research may be indicated to probe the

test-retest reliability of sexual abuse reports and the role of study-specific variables (e.g., method of interview, wording of abuse inquiry) as related to the frequency and extent of subject abuse reports.

Effects of Abuse Definition

As indicated by Peters, Wyatt, and Finkelhor (1986), sexual abuse researchers have used different definitions of what, at minimum, constitutes childhood sexual abuse. Russell (1984), for example, defines sexual abuse as any unwanted sexual experiences before age 14, or attempted or completed rape by age 17, or any attempted or completed sexual contact that occurred between relatives before the victim turned 18. Others restrict this category to actual sexual contact between someone under 15 years of age and another person 5 or more years older (e.g., Briere & Runtz, 1988). Obviously, such variability influences estimates of sexual abuse incidence or prevalence in any given sample. Less appreciated, however, are the effects of different abuse definitions on the identification of abuse-related psychological disturbance. It is not unreasonable to assume, for example, that researchers who restrict themselves to earlier or more intrusive forms of abuse might report more extreme outcomes than those using broader definitions (Peters, 1988). Until researchers settle on a standard definition of what does and does not constitute sexual abuse, findings regarding abuse correlates must be evaluated in terms of the specific definition being used.

Nonequivalent Comparison Groups

Most research on the long-term sequelae of sexual abuse uses designs best considered *preexperimental* or *correlational* (Campbell & Stanley, 1966; Cook & Campbell, 1979). This is because researchers in this area are forced to begin with a subject variable rather than a manipulated one. Because sexual abuse is not a randomly assigned condition, it is impossible to ensure that abused and nonabused subjects are equivalent in all other respects and thus that differences found between these groups are due to abuse, *per se*. For example, if two groups vary not only according to sexual abuse history but also as a function of age, social class, or family dysfunction, symptom differences between these groups cannot be linked solely to abuse status.

Because sexual abuse is often correlated with other variables, special attention must be paid to the comparison group's status on these factors. Although matching on relevant variables often appears to be an attractive option, it is not always clear on which variables subjects should be matched. Among those variables seemingly relevant to child abuse are demographic characteristics such as age, race, sex, education, and socioeconomic status (SES), along with clinical status, various measures of family functioning, and history of childhood traumas and stressors other than sexual abuse. Because child abuse is a complex variable with complex antecedents, however, even this level of control is likely to be insufficient. For example, the experimenter whose abuse group is sampled from university students and whose comparison group (albeit carefully matched on each of the above variables) is drawn from the general population risks

the possibility that unmonitored—but nevertheless important—variables other than abuse still discriminate these groups.

Finally, because matching subjects usually means differentially discarding other subjects, this procedure can reduce the generalizability of obtained between-groups differences, especially if the matching variables include risk factors for abuse. Individuals who match an abused sample on negative family environment and economic distress, for example, may best represent an atypical population: people who were at risk to be abused but, for whatever reasons, were not victimized. This problem also may occur when different groups of abused subjects (e.g., sexually abused, physically abused, and psychologically neglected) are matched and compared because the resultant groups may no longer represent the (unmatched) populations from which they were drawn.

Rather than matching to approximate equivalence, it is usually best to draw representative abused and nonabused subjects from the same population, whether it be university students, clients from a mental health center, or a random sample of women from a specific geographic area. If differences on relevant variables emerge, the results should be qualified accordingly in the discussion section. Ultimately, although the researcher may wish to match on any number of subject dimensions, Miller's (1987) conclusion remains true: "Matching cannot be used to make two groups equivalent when the groups are not in fact equivalent" (p. 50).

Other Forms of Abuse

Since the beginning of research in this area, there has been a tendency for investigators to examine sexual abuse in a relative vacuum. As noted more generally by Rosenberg (1987), abuse researchers have tended to overlook other forms of maltreatment that a child might experience in a given family, despite the fact that physical, psychological, and sexual child abuse frequently occur together. Recent research by Briere and Runtz (1990), for example, points to the confusion that may accrue from examining one form of abuse alone. In this study various forms of child maltreatment were found to covary, and each type of abuse was correlated with a number of later psychosocial difficulties. When all other forms of abuse were controlled by canonical correlation analysis, however, each type of abuse was found to have a considerably smaller number of unique psychological correlates.

The copresence of various child abuse types and sequelae suggest that, at minimum, researchers should examine not only the main effect of sexual abuse on psychological functioning but also its interaction with physical and psychological maltreatment factors. Other designs might consider all three forms of abuse simultaneously, using multivariate procedures such as multiple regression or canonical correlation.

Conclusions Regarding Causality

As noted at several points in this article, reliance on retrospective, nonlongitudinal designs generally precludes definitive inferences regarding abuse effects. Among the impediments to such conclusions are the potentially confounding effects of simultaneously determining previous abuse and current symp-

toms, the potential contributions of injurious events copresent with or antecedent to sexual abuse, and the absence of data on children's psychological functioning before molestation.

Although clinical experience and the characteristics of some postabuse sequelae (e.g., flashbacks to specific abusive events in childhood) suggest that many of the difficulties associated with a child abuse history are in fact abuse effects, it is erroneous to make such conclusions solely on the basis of the available research. The role of science in such endeavors is to as precisely as possible determine (a) the exact relationships between childhood sexual abuse and various types of psychological dysfunction; (b) which "effects" are likely to be epiphenomena of sexual abuse (i.e., arising from third variables, including other types of maltreatment); and (c) how preabuse functioning, family-of-origin dynamics, and other variables moderate these relationships.

Because correlational designs are, in and of themselves, insufficient to imply causation, the abuse researcher must use strategies that lessen the number of possible alternative explanations for their findings. This is, of course, a common goal of research design. Kazdin (1980) notes that

Experimental results by their very nature are ambiguous because any particular finding may depend upon one of the many unique features of the setting and experimental arrangements where the finding was obtained. Because ambiguity can never be eliminated, minimizing ambiguity is the primary task of research design. (p. 4)

Ambiguity regarding abuse sequelae is probably best addressed by procedures that rule out competing explanations for symptomatology. Alternative hypotheses, for example, are that abuse effects are at least in part the results of dysfunctional family dynamics that support sexual abuse and produce psychological disturbance (Fromuth, 1986) and that concomitant physical or psychological abuse may account for some of the difficulties otherwise attributed to sexual abuse (Briere & Runtz, 1990). Attention to these possibilities suggests what are likely to be minimal criteria for modern abuse sequelae research: that the design control for extraneous variables by both (a) sampling from a single population (e.g., inner city adolescents or patients from a dissociative disorders clinic) and (b) formally including relevant factors as additional independent variables or predictors so that their interactions with sexual abuse can be tested directly.

Alternatively, variables relevant to competing etiologic scenarios may be examined through structural equation modeling (see, for example, Wyatt & Newcomb, 1990). This approach allows one to examine specific hypotheses (models) regarding the causal links among such variables and permits consideration of phenomena that may mediate between cause and effect. Such analyses are only valid, however, if the sample size is adequate, the variables in question have been well operationalized and reliably measured, and the proposed model is based on adequate theory (MacDonald, 1977). Even this more sophisticated analytic approach, however, cannot prove a specific causal hypothesis; it can only provide information on how well a given model fits the data at hand. Because of the error that almost inevitably creeps into the estimation of linear models, however, this goodness-of-fit criterion may be misleading in

some instances (Anderson & Shanteau, 1977). Ultimately, the various weaknesses inherent in correlational designs demand that abuse researchers avoid unequivocally attributing causality to findings not derived from rigorous longitudinal methodology.

Measurement Issues

Research on abuse sequelae has been compromised, to some extent, by the use of questionable measurement systems. Frequently, investigators use either home-spun measures of unknown reliability and validity or generic measures that may be insensitive to abuse-specific symptomatology. As a result, findings in this area can be difficult to interpret.

To the extent that adequate reliability and validity is established, the use of study-specific measures is not inherently problematic. Unfortunately, a number of studies have foregone psychometric evaluation of their instruments, leaving the consumer to trust that the measure is stable and that it taps the construct intended by the researchers. Without such data it is not clear whether negative findings reflect an absence of between-groups differences or the impacts of unreliability on validity. Positive findings, on the other hand, are interpretable only to the extent that the measure has construct validity. For these reasons, study-specific instruments should be developed according to accepted psychometric principles (e.g., Anastasi, 1988; DeVellis, 1991) and accompanied by data on reliability and validity.

A second problem for abuse researchers is the generality of many measures of psychological dysfunction. Because most available instruments were developed without reference to abuse or trauma, they may be less sensitive to abuse-specific symptoms (Elliott & Briere, 1991). An example of the promise of abuse specificity is the findings of Briere and Runtz (1990), who found that although a standard measure of low self-esteem was unrelated to child abuse history, a newly created measure incorporating self-denigrating statements often made by former abuse victims was significantly associated with childhood maltreatment. Similarly, Bagley's (in press) community study of 345 Canadian women revealed that the Trauma Symptom Checklist (Briere & Runtz, 1989)—a scale developed to specifically tap abuse-related symptomatology—was more effective than traditional measures such as the Middlesex Hospital Questionnaire, the Center for Epidemiological Studies in Depression (CESD) scale, or the Coopersmith self-esteem inventory in identifying adults who were sexually abused as children.

The importance of abuse-relevant measures resides not in their potential ability to identify abuse victims, however, as much as in the development of an accurate database regarding abuse-specific symptom patterns. By identifying precisely how former abuse victims differ from nonvictims, for example, such research can help clinicians to more accurately diagnose post-abuse disturbance, as opposed to perhaps missing victims' distress on generic measures of psychological functioning.

Constraints on Generalization

Researchers of sexual abuse sequelae have sampled from a wide variety of subject groups, including psychiatric inpatients,

mental health outpatients, university students, professionals, and members of the general population. Although most studies have restricted themselves to female subjects, recent investigations have increasingly included male subjects (e.g., Briere, Evans, Runtz, & Wall, 1988; Urquiza & Crowley, 1986). Similarly, although most long-term sequelae studies have been of Caucasian subjects, more attempts are being made to sample from other racial and ethnic groups (e.g., Wyatt, 1986). Socioeconomic status often varies with subject type; for example, university students are typically from the middle class, whereas some patient groups and general population samples have lower SES. This heterogeneity is a positive development in terms of our understanding of abuse in different social contexts. It is more problematic, however, when researchers seek to generalize from a given sample to the universe of sexual abuse victims.

Not only is generalization hampered by the varying clinical status, occupations, races, and SES of these samples but the type and extent of sexual abuse reported by subjects often differs significantly from group to group. For example, clinical subjects typically report more frequent molestation by more perpetrators, a longer abuse duration, a greater likelihood of intercourse, and more symptomatology than do nonclinical subjects (Elliott & Briere, 1991). As a result, findings derived from clinical groups may not generalize well to general population samples or to other individuals with less severe abuse histories.

Given differences in subject characteristics and abuse severity, blanket statements about sexual abuse or its mental health sequelae should be carefully avoided when they are based on a single study. In fact, short of large, general population studies of sexual abuse and its correlates (i.e., Wyatt & Newcomb, 1990), conclusions drawn from any given child abuse study should be limited to careful inferences regarding individuals with similar demographics, social status, and abuse histories.

An important caveat to the above-noted concerns is that of study replication. Although the results of a single investigation may have limited generalizability, findings that are stable across multiple studies (and therefore multiple samples) have considerably greater applicability to the universe of abuse victims. For example, the association between childhood sexual abuse and depression found in a number of clinical and nonclinical studies (Briere & Runtz, 1991; Browne & Finkelhor, 1986) suggests that this symptom is a common sequel of sexual abuse, to some extent irrespective of subject type. Consistent replication of findings at least partially addresses those threats to internal and external validity present in many sexual abuse studies. *Meta-analysis*, defined as "the statistical analysis of a large collection of . . . individual studies for the purpose of integrating the findings" (Glass, 1976, p. 6), may be especially helpful in summarizing the findings of replicated abuse studies and, in some instances, uncovering relationships between abuse and outcome variables not readily apparent in any given single study (Rosenthal, 1991).

Statistical Issues

In addition to the methodological concerns outlined above, there are several statistical issues that may arise in sexual abuse research.

Insufficient statistical power. Studies with inadequate sample sizes, unreliable or insensitive measures, or poorly chosen statistical analyses decrease the investigator's ability to detect relationships when they are, in fact, present in the population at hand (Cohen, 1988; DeVellis, 1991). Reduced statistical power is often a problem in studies of abuse sequelae, resulting in an underestimation of the differences between abused and non-abused subjects. For this reason, it is suggested that (a) sample and cell sizes in any given study minimally attain the magnitude suggested by the appropriate power analysis (Cohen, 1988); (b) dependent measures be reasonably reliable and relevant to abuse sequelae; and (c) statistical tests be appropriate for the data, in terms of their sensitivity, underlying assumptions, and use of the highest level of measurement possible. Furthermore, researchers who fail to reject the null hypothesis should consider reporting the results of relevant power analyses, so that the potential contribution of methodological issues (e.g., low N) to nonsignificant results can be evaluated.

Univariate versus multivariate analyses. Assuming that reasonable sample sizes can be attained, the researcher confronted with multiple, related measures of psychological functioning may find multivariate statistical approaches to be advantageous over univariate ones. Such procedures, which include multivariate analysis of variance, canonical correlation, discriminant analysis, and multiple regression, can be helpful in several ways: They control the experimentwise error rate relative to an unprotected series of univariate analyses; they take into account the relationship between dependent variables rather than erroneously treating related dependent variables as independent events; and, in some instances, they may be more able to detect between-groups differences than univariate tests.

Although multivariate analysis can be well suited to sexual abuse research, it can be misused as well. For example, multivariate tests are sometimes used in the context of insufficient sample sizes or inadequate subjects-to-variables ratios, thereby potentially capitalizing on error variance and producing misleading results (Tabachnick & Fidell, 1989). Multivariate analysis also can be inappropriate when encompassing a collection of unrelated measures (e.g., subject age, score on a depression inventory, and responses on an analogue parenting task), as opposed to a set of variables whose interrelation is likely to be both significant and of interest, such as the subscales of the Minnesota Multiphasic Personality Inventory (Hathaway & McKinley, 1982) or scores on a neuropsychological test battery (Applebaum & McCall, 1983).

Statistical control issues. Because sexual abuse may be associated with familial dysfunction, other forms of maltreatment, or other variables thought to be risk factors for molestation, researchers have attempted to control for these factors either by matching (as noted previously) or through statistical methods. Of the latter, the most common are the partialing procedures, statistical techniques that allow exploration of the relationship between child abuse and adult psychological functioning after the variance shared by child abuse and other variables has been removed (controlled for or partialled out).¹

Although these methodologies have many valid applications, there are a number of conditions when partialing procedures can lead to excessively conservative or even erroneous conclusions (Briere, 1988a). These include the use of covariates to

provide statistical *equivalence* between nonequivalent groups, the use of semipartial analysis to establish the relative importance or etiologic significance of two or more variables (Pedhazur, 1982; Stevens, 1986), and the use of any partialing technique when certain underlying assumptions or implicit requirements are not met (Briere, 1988a). Regarding the latter, partial or semipartial analysis may be misleading if the following are true:

1. *There is substantial multicollinearity between control and abuse variables.* Pedhazur (1982) notes that in the extreme case of this problem, "partialling out from one predictor another predictor from which it is highly correlated will generally result in a small, even meaningless semi-partial correlation" (p. 167).

2. *The control variable is unreliable.* As noted by Cohen and Cohen (1983), measurement error in the control variable "may decrease or increase or even change the sign of, a partial relationship" (p. 407).

3. *The causal or directional relationship between the control variable(s) and the abuse variable is unknown.* Various writers (e.g., 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 there is a possibility that X_2 caused X_1 (e.g., that sexual abuse produces family dysfunction as well as symptoms), or that X_1 and X_2 interact synergistically, or that X_1 and X_2 are different measures of the same construct. The former point may be especially relevant to abuse effects research, where ongoing intrafamilial abuse undoubtedly contributes to a negative family environment and where both may have synergistic impacts on later psychological adjustment.

At minimum, the algebra of the semipartial correlation is likely to produce a conservative test when the control variable is not causally antecedent to the abuse. In order to be significant, abuse must correlate with symptoms after all variance shared with the control variables(s) has been removed. Such analyses tend to give more of the credit to control variables than to abuse variables, thereby encouraging an underestimation of the potential impacts of sexual abuse.

Because covariate, semipartial, or other control variable analyses are inappropriate solutions to nonequivalent groups, and given the complexity of childhood sexual abuse and its association with other variables, partialing procedures should be used with caution in such research. Among other constraints, partialized results should be interpreted as such (i.e., solely as "the effect of a variable(s) after having controlled for another variable(s)" (Pedhazur, 1982, p. 178).

Conclusion

This article has outlined a number of ways in which the methodology of sexual abuse research might be improved. It should not be inferred from this critique, however, that the extant literature in this area is fatally flawed. The many studies available on abuse sequelae have been of considerable assistance to clinicians, legislators, and social policy planners—individuals who

¹ Although the present discussion concerns the use of semipartial correlations, similar issues may be raised regarding the use of related procedures such as analysis of covariance, partial correlation analysis, or stepwise multiple regression analysis.

could not reasonably wait for the results of rigorous longitudinal research before intervening in this major social problem. In fact, the first wave of abuse research has largely succeeded in terms of increasing social, clinical, and scientific awareness of sexual abuse and its potential impacts.

Given this success, it is time for the second wave: the development of more tightly controlled and methodologically sophisticated studies that seek to disentangle the antecedents, correlates, and impacts of sexual abuse. Although such studies are more likely to be longitudinal in nature, the cross-sectional study is far from dead. Instead, future work in this area is likely to be characterized by greater attention to design sensitivity, greater control over extraneous variables, and careful inferences about causality.

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