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A Critical Appraisal of the 1998 Meta-Analytic Review of Child Sexual Abuse Outcomes Reported by Rind, Tromovitch, and Bauserman

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SUMMARY. The goal of this article is to present a methodological critique of the 1998 meta-analysis of child sexual abuse outcomes by Rind,

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Tromovitch, and Bauserman. Seven major concerns are addressed. Rind et al.'s view is, at best, extremely limited. By restricting a supposedly broad meta-analysis to only some of the population in question, the conclusions they drew regarding this complex topic, primarily that adult-child sex is not necessarily harmful, are invalid. [Article copies available for a fee from The Haworth Document Delivery Service: 1-800-HAWORTH. E-mail address: <getinfo@haworthpressinc.com> Website: <<http://www.HaworthPress.com>> © 2001 by The Haworth Press, Inc. All rights reserved.]

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In 1998, "A Meta-Analytic Examination of Assumed Properties of Child Sexual Abuse Using College Samples," by Bruce Rind, Philip Tromovitch, and Robert Bauserman was published in the *Psychological Bulletin*. The topic is, undeniably, of great import to all concerned with the welfare of children throughout the world. However, the research reported was flawed in two critical areas: the methodology used by the authors, and the extant studies supporting quite different conclusions that they ignored. An October 2000 search of the Social Science Citation Index identified only 28 published papers in which the meta-analytic review by Rind et al. has been cited; none of those publications was a formal critique of the authors' work.

We present such a methodological critique in this first article. In this critique, seven major concerns are addressed: (1) the child sexual abuse population selected, (2) the research strategy adopted, (3) the meta-analytic coding used, (4) the statistical techniques applied, (5) the psychological adjustment symptoms of child sexual abuse analyzed, (6) the confounding of gender and age and the underrepresentation of males, and (7) the limitations of the retrospective cross-sectional approach. At the end, conclusions are presented with suggestions to guide future research on child sexual abuse.

The article by Rind et al. is long, consisting of 30-double column pages, which include 12 tables and 191 references, and a two-page Appendix. To aid the reader in identifying specific passages referred to in the present critique, pertinent quotations are included.

CHILD SEXUAL ABUSE POPULATION SELECTED

Our first methodological question concerns the representativeness of the population selected by Rind et al. to examine child sexual abuse, and the iden-

tification of the underlying distribution characteristics of the entire child sexual abuse population. Rind et al. defined the population of sexually abused youth as both children and adolescents who have suffered any type of sexual abuse. Included in their study were only individuals whose psychological reactions were classifiable into mutually exclusive negative, neutral, or positive categories. Excluded were the more severe clinical and legal cases, considered by the authors to be nonrepresentative of the general child sexual abuse population. Rind et al. argued that samples of college students are representative of the general population of individuals sexually abused as children, while samples drawn from the more severe clinical and legal cases, especially those individuals suffering adverse physical and mental consequences from child sexual abuse, are anomalous. They presented data from three national samples to support this assertion, noting in their article:

[T]he top half of Table 1 [p. 30] shows the estimated pervasive rates in the college population for the different types of CSA [child sexual abuse] for SA [sexually abused] women and men separately and combined. To provide a frame of reference for these results, we estimated corresponding prevalence rates for SA persons in the general population [i.e., the population of those individuals who were sexually abused as children] based on reports from 3 national samples (Baker & Duncan, 1985; Laumann, Gagnon, Michael, & Michaels, 1994; López, Carpintero, Hernández, & Fuertes, 1995). Data in these studies were obtained in face-to-face interviews of respondents selected to be representative of their nations (Britain, United States, and Spain, respectively). The strength of face-to-face interviews in obtaining valid data along with the high response rates of these studies (unweighted mean = 83%) suggest that their prevalence rates serve as good population estimates. . . . The bottom half of Table 1 displays the estimated prevalence rates for the different types of CSA for SA persons in the general population. (1998, p. 30)

Rind et al. conducted computerized database searches of PsycLIT, Sociofile, PsycInfo, Dissertation Abstracts International, and ERIC to produce 59 “usable” studies (36 published studies, 21 unpublished dissertations, and two unpublished master’s theses) that they included in their meta-analysis. While Rind et al. believe that college samples are representative of the general population of those individuals who were sexually abused, not all investigators involved in child sexual abuse research concur (Haugaard & Emery, 1989; Silverman, Reinherz, & Giacona, 1996).

To determine whether the college samples and the three national samples analyzed by Rind et al. came from what can be considered to be the same popu-

lation, one of us (J.A.W.) conducted nonparametric χ^2 tests¹ using the data contained in Tables 1 and 2 of the meta-analysis (1998, pp. 30 and 31); the face validity of these data was assumed for all of the tests.² The χ^2 values obtained (our Tables 1 and 2) indicated that the college samples differed considerably from the national samples (see below) and that these samples do not come from the same child sexual abuse population as stated by Rind et al.

Based on the sample, J.A.W. converted the percentages of those respondents who had experienced exhibitionism, fondling, oral sex, or intercourse (1998, p. 30, Table 1) into frequencies and performed eight χ^2 tests as shown in our Table 1. The results of the analyses comparing the college samples with the national samples revealed that there are statistically significant differences with regard to two of the four types of child sexual abuse. This indicated that, with regard to two of the four types of child sexual abuse, the college samples are not representative of the child sexual abuse population represented by the national samples. The results of the analyses comparing females and males revealed that there are highly significant gender differences with regard to all four types of child sexual abuse. Specifically, the differences in the observed and the expected frequencies between females and males indicated that females reported relatively more cases of exhibitionism than did the males and that the males reported relatively more cases of fondling, oral sex, and intercourse than did the females. Apparently, more females in the samples were abused but more passively, whereas fewer males were abused but were so more actively.

Rind et al. also compared the college samples and the national samples with regard to relationship information (1998, p. 31, Table 2), such as the close family (e.g., biological parents, stepparents, grandparents, older siblings) and the wider family (both close family and other relatives). The results of our χ^2 analyses (see our Table 2) indicated that the college samples differed considerably from the national samples with regard to child sexual abuse involving both the close and the wider family members. Rind et al.'s inclusion of the close family data as a component of the wider family data in their prevalence rate estimates is questionable; this confounding affected the results of our χ^2 analyses (our Table 2). Nevertheless, since both the wider family and close family entities demonstrated statistically significant differences between the college samples and the national samples, it is reasonable to conclude that our family relationship findings are valid.

To reiterate, our finding that four of the six χ^2 comparisons we made between the college samples and the national samples are statistically significant (our Tables 1 and 2) indicates that for those measures the samples are not from the same population. In view of this finding, we want to emphasize that Rind et

TABLE 1. χ^2 Analyses of Prevalence Rate Estimates of Four Types of Child Sexual Abuse for Population Samples and Gender as Reported in the Meta-Analysis by Rind, Tromovitch, and Bauserman (1998, p. 30, Table 1)

Type of Child Sexual Abuse*	Population/Gender	Reported Abuse	Frequency		χ^2 Value	<i>p</i> Value
			O†	E		
Exhibitionism	College‡	Yes	806	827	2.93 (1 df)	> .05
		No	1,872	1,851		
	National	Yes	316	295		
		No	640	661		
	Female	Yes	919	853	30.80 (1 df)	< .001
		No	1,843	1,909		
	Male	Yes	203	269		
		No	669	603		
Fondling	College‡	Yes	1,105	1,292	198.78 (1 df)	< .001
		No	1,573	1,386		
	National	Yes	648	461		
		No	308	495		
	Female	Yes	1,242	1,332	49.14 (1 df)	< .001
		No	1,520	1,430		
	Male	Yes	511	421		
		No	361	451		
Oral sex	College‡	Yes	136	199	81.92 (1 df)	< .001
		No	2,542	2,479		
	National	Yes	134	71		
		No	822	885		
	Female	Yes	118	205	165.7 (1 df)	< .001
		No	2,644	2,557		
	Male	Yes	152	65		
		No	720	807		
Intercourse	College‡	Yes	449	436	1.77 (1 df)	> .05
		No	2,229	2,242		
	National	Yes	142	155		
		No	814	801		
	Female	Yes	376	449	59.00 (1 df)	< .001
		No	2,386	2,313		
	Male	Yes	215	142		
		No	657	730		

*The combined total number of the four types of sexual abuse for all college samples and all national samples was 3,634.

†Because all of these values were calculated by one of us (J.A.W.) from percentages calculated by Rind et al., rounding errors may exist.

‡For the college population sample ($n = 2,678$), data for the "combined" values from Rind et al.'s Table 1 (1998, p. 30) were not used to calculate the observed (O) and expected (E) frequencies because the data for males and females were commingled.

TABLE 2. χ^2 Analyses of the Prevalence Rate Estimates of the Relationship Between Sexually Abused Respondents and Their Abusers for Population Samples and for Gender as Reported in the Meta-Analysis by Rind et al. (1998, p. 31, Table 2)

Relationship to Abuser*	Population/ Gender	Reported Abuse	Frequency		χ^2 Value	p Value
			O†	E		
Wider Family	College‡	Yes	1,145	1,080	25.39 (1 df)	< .001
		No	2,170	2,235		
	National	Yes	255	320		
		No	726	661		
	Female	Yes	1,218	1,089	138.32 (1 df)	< .001
		No	2,123	2,252		
	Male	Yes	182	311		
		No	773	644		
Close Family	College‡	Yes	180	149	15.63 (1 df)	< .001
		No	882	913		
	National	Yes	106	137		
		No	875	844		
	Female	Yes	249	196	52.94 (1 df)	< .001
		No	1,149	1,202		
	Male	Yes	37	90		
		No	608	555		

*Calculations were confounded by inclusion of close family data as a component of wider family data in the meta-analysis by Rind et al.

†Because all of these values were calculated by one of us (J.A.W.) from percentages calculated by Rind, Tromovitch, and Bauserman, rounding errors may exist.

‡Data for the "combined" values from Rind et al.'s Table 2 (1998, p. 31) were not used to calculate the observed (O) and expected (E) frequencies because the data for males and females were commingled.

al. neglected to perform a single statistical analysis of the data they presented in their Table 1 and Table 2 (1998, pp. 30-31).

Another concern is the use of the face-to-face interviewer-interviewee method used to collect data from all three national samples. Rind et al. believe such methodology to be very effective. However, it has limitations, particularly when the topic of interest is as emotionally charged as is that of child sexual abuse. An important limitation is the potential risk that the interviewer may transfer to the interviewee whatever biases he or she may have (Babbie, 1973).

With regard to general volunteer bias, investigators of such bias in sexuality research, for example, have pointed out that reliance on college student samples limits the generalizability of study results (Strassberg & Lowe, 1995). Overreliance on college student samples may, in fact, even give us biased

views of human social behavior (Sears, 1986). Sears states, “[This is because] college students are likely to have less-crystallized attitudes, less-formulated senses of self, stronger cognitive skills, stronger tendencies to comply with authority, and more unstable peer group relationships” (1986, p. 515). He concludes, “[A] greater effort must be made to conduct research on persons from life stages other than late adolescence” (1986, p. 527).

Before such biases in data collection are addressed, and attempts are made to ensure the selection of an appropriate study population, however, the definition and scope of child sexual abuse must be explicitly identified by any investigator in the field (Itzin, 1997).

RESEARCH STRATEGY ADOPTED

Our second methodological question concerns the appropriateness of the research strategy employed by Rind et al. in their meta-analysis. On the surface, their meta-analytic findings appear to present a very different picture of the problem of sexual abuse and its effects on children’s psychological adjustment from that of many other researchers. Upon closer examination, however, it is obvious that they meta-analyzed only a very carefully selected number of the many published studies easily available for examination. They asserted:

Opinions expressed in the media and by many popular press and professional writers imply that CSA has certain basic properties or qualities irrespective of the population of interest. These implied properties are (a) CSA causes harm, (b) this harm is pervasive in the population of persons with a history of CSA, (c) this harm is likely to be intense, and (d) CSA is an equivalent experience for boys and girls in terms of its widespread and intensely negative effects. (1998, p. 22)

Rind et al. employed a strategy not commonly used in scientific studies: setting up a straw man and then knocking the straw man down. The authors presented what they considered to be the four commonly assumed properties of child sexual abuse and then disputed their significance with the findings from their meta-analysis of other child sexual abuse studies. Most researchers would approach such a complex research problem differently, by either clearly stating hypotheses to be tested within the scope of a study design or by conducting exploratory studies to identify useful information for analysis and possibly for future development of hypotheses. In effect, once Rind et al. removed the more severe clinical and legal studies as being nonrepresentative of the general child sexual abuse population, they made no effort to test their own hypothesis.

As discussed in the preceding section, the college student samples on which Rind et al. based their conclusions cannot be considered to be representative of the general child sexual abuse population. Rind et al. themselves acknowledged that numerous literature reviews published during the preceding 15 years “have attempted to synthesize the growing body of empirical investigations of CSA effects and correlates. . . . These reviews have not been unanimous in their conclusions” (1998, p. 23). In any case, even if the four assumed properties of child sexual abuse identified by Rind et al. were based on more than just opinions offered in both the lay and the professional literature, and the findings were more consistent, the concept of causality cannot be inferred with only correlational data. Rind et al. drew their conclusions from such data.

META-ANALYTIC CODING USED

Our third methodological question concerns the objectivity of the coding procedure used by two of the authors. Rind and Tromovitch reviewed all of the 59 studies they selected as usable and “independently coded” them for subsequent meta-analysis. Rind et al. explained:

For each study, the following information was coded: (a) all statistics, if provided, on psychological correlates of CSA . . . (b) types of psychological correlates reported; (c) all statistics regarding relations between moderator variables (e.g., force, penetration, frequency of CSA) and psychological correlates; (d) sex of participants; (e) definition of CSA, including ages that defined a ‘child’ and an older person, whether peer experiences were included, whether CSA experiences were limited to contact sex or also included noncontact sexual experiences, and whether CSA experiences were limited to unwanted sex or also included willing sexual experiences; (f) all reaction data, if provided, including both retrospectively recalled reactions to and current reflections on the CSA experiences; (g) all self-reported effects data, if provided, including responses to how these experiences affected participants overall and how they affected their sex lives; (h) types of family environment measures used; and (i) all statistics on family environment measures, including their relations with CSA and with psychological correlates (1998, p. 27)

To establish interrater coding reliability, Rind and Tromovitch “independently” coded six different measures: psychological correlates; reactions of students to their child sexual abuse experiences; self-reported effects of child sexual abuse; relationships between family environment and child sexual abuse; relationships between family environment and psychological adjust-

ment; and results of statistical control. The interrater agreement for these codings ranged from 85% to 100%, and all disagreements were resolved by discussion. However, because the two authors are colleagues who undoubtedly share similar attitudes regarding child sexual abuse (e.g., Rind & Tromovitch, 1997), it is unlikely that they can be considered to be truly independent judges. Anything less than high interrater reliability in this case would be surprising.

In addition, the raters classified the students' retrospectively recalled reactions and current reflections into three mutually exclusive categories: negative, neutral, or positive. Child sexual abuse experiences, especially those believed to be negative, however, are known to be complex and diverse (e.g., Ronai's personal account [Ronai, 1995]). Compared with the relatively narrow ranges of positive and neutral experiences, such a limited classification system accounts for neither the type nor the intensity of the much broader range of negative experiences. Reduction of the broad range of negative abuse experiences into a single category increases the likelihood that statistical correlations involving these experiences will be artificially reduced as well. In addition, abusive childhood experiences believed to be positive may actually represent a developed coping strategy that generally fails in interpersonal relationships (Styron & Janoff-Bulman, 1997). The eventual failure of such coping strategies also calls self-reports of willing or consensual participation in adult-child sexual relations into question.³ The perception of willing or consensual participation might simply be a component of the developed coping strategy.

STATISTICAL TECHNIQUES APPLIED

Our fourth methodological question concerns the defensibility for Rind et al.'s use of parametric statistics. In their meta-analysis, they used a parametric statistic, Pearson correlation coefficient r , to determine direction (negative or positive) and strength of relationships between variables. They also used a nonparametric statistic, χ^2 , in their test of the homogeneity of variance among the 54 independent samples of individuals who had been sexually abused as children for whom psychological effects or correlates could be computed. As Rind et al. described:

The effect size used in this review was r , the Pearson correlation coefficient. For CSA-psychological adjustment relations, positive r s indicted poorer adjustment for CSA participants compared to control participants. For CSA-family environment relations, positive r s indicated poorer family functioning for CSA subjects. For family environment-ad-

justment relations, positive r s indicated that poorer family functioning was associated with poorer adjustment. Pearson r s were also computed to assess the magnitude of the relation between various moderating variables (e.g., force) and outcome measures (i.e., psychological adjustment and self-reported reactions). Positive r s indicated that higher levels of moderators were associated with higher levels of symptoms or more negative reactions to the CSA. Finally, Pearson r s were computed to assess the size of the differences in reactions and self-reported effects between men and women who had CSA experiences. In this case, positive r s indicated that men reported fewer negative reactions or effects than [did] women, or conversely, that they reported more positive reactions or effects than [did] women. (1998, p. 29)

In addition, Rind et al. pointed out, "To examine the intensity of CSA psychological effects or correlates, we first meta-analyzed the sample-level effect sizes from the 54 [independent] samples for which these could be computed . . . The resulting unbiased effect size estimate, based on 15,912 participants, was $r_u = .09$, with a 95% confidence interval from .08 to .11" (1998, p. 31). They continued, "In an attempt to achieve homogeneity, we examined the distribution of sample-level effect sizes to determine whether outliers existed. We defined outliers to be effect sizes that were at least 1.96 standard deviations away from the unweighted mean effect size (i.e., falling in the extreme 5% of the distribution). Three outliers were found" (1998, p. 31).

The primary advantage of the meta-analytic method is that by combining the data for different sample sizes from several similar studies into a single larger sample group, greater statistical power can be attained. But use of this method in the examination of child sexual abuse has drawbacks. Individuals react quite differently to their different abuse experiences; the technical soundness of study designs varies widely; the utility and the consistency of the procedures followed in the studies also varies; and study outcomes reveal wide variability among the dependent variables, both within a specific study and across studies (Haugaard & Emery, 1989).

Large samples, such as those used by Rind et al., often produce statistically significant findings that have no practical and little theoretical value. For example, based on the 54 independent samples that contained data that could be correlated ($N = 15,912$ participants), Rind et al. found a statistically significant r value of .09 between the intensity of child sexual abuse and psychological adjustment effects. Although this suggested that students who had been sexually abused as children were less well adjusted than those who had not been abused, such a low correlation accounted for less than 1% of the total adjusted variance in Rind et al.'s meta-analysis (in other words, unknown variables accounted

for more than 99% of the reasons for the lower psychological adjustment of sexually abused individuals).

Parametric statistics, which use the actual data values obtained, are more powerful for obtaining statistically significant findings involving two samples than are nonparametric statistics, which use rank-order data (Siegel, 1956). The use of parametric statistics, however, requires that two assumptions be met: the population must be normally distributed, and the intervals must be equal (Edwards, 1960). Rind et al. provided information about neither the shape nor the nature of the population of individuals who had been sexually abused as children with regard to either the intensity of the abuses or the nature of the effects. Researchers investigating the complexities of child sexual abuse have recognized for some time that, given such an ill-defined population, it is essential that appropriate statistical techniques be selected and applied (Plunkett & Oates, 1990).

For example, investigators will usually establish a priori a statistically significant probability figure for stating that a null hypothesis is rejected or that a statistically significant relationship exists between two samples. Rind et al. used two different probability values for determining statistical significance: .05 with the nonparametric χ^2 statistic, and .10 with the parametric Pearson correlation coefficient r statistic. However, they provided no rationale for why they chose two different probability values.

When several studies are combined to obtain a larger sample size, heterogeneity of variance is also a potential problem. Any comparisons involving Pearson correlations that contain heterogeneous variances will not yield accurate estimates of the true relationships. For example, 10 of the 18 symptom-level meta-analyses (paper-and-pencil instruments were used to measure psychological adjustment) yielded heterogeneous results; repeated removal of the supposedly anomalous outliers increased the number of homogenous sets to 15 of the 18.

Thus, given the ill-defined child sexual abuse population, together with the wide variability within the samples and the heterogeneity of variances between the samples, the appropriateness of using parametric statistics and applying the meta-analytic technique is questionable.

PSYCHOLOGICAL ADJUSTMENT SYMPTOMS OF CHILD SEXUAL ABUSE ANALYZED

Our fifth methodological question concerns the absence both of a rationale for giving all 18 psychological adjustment symptoms equal weight with regard to child sexual abuse and of any relevance of low, albeit statistically significant, correlations. In the 59 usable studies examined by Rind et al., standardized tests,

inventories, checklists, questionnaires, scales, a diagnostic instrument, a grid, surveys, schedules, and investigator-authored items were used to identify and measure what, if any, negative attitudinal and behavioral consequences might be correlated with child sexual abuse.

Rind et al. “[coded] the studies [that] resulted in 18 categories of psychological correlates of CSA; several additional correlates were infrequently reported and were therefore not considered in the meta-analyses” (1998, p. 28). At least one of the 18 different types of psychological adjustment “symptoms” was reported in each of the 54 independent samples analyzed: alcohol problems, anxiety, depression, dissociation, eating disorders, hostility, interpersonal sensitivity, locus of control, obsessive-compulsive symptomatology, paranoia, phobia, psychotic symptoms, self-esteem, sexual adjustment, social adjustment, somatization (which reflects bodily related distress), suicidal ideation and behavior, and wide adjustment (which is a general measure of psychological adjustment or symptomatology). It would have been desirable for the frequency with which each of the symptoms was identified in the 54 independent samples and their relevance in each sample to have been presented.

Rind et al.’s meta-analysis produced small but positive correlations, ranging from $r_u = .04$ to $.13$ (1998, p. 32, Table 3); all effect size estimates except one, locus of control, were statistically significant. According to Rind et al., the 17 statistically significant findings indicate that, for all symptoms except one, child sexual abuse participants as a group were slightly less well adjusted than the control participants. As discussed earlier, however, large samples often produce low albeit statistically significant correlations that have no practical value; the sample sizes for each of the 18 symptoms ranged from a low of 1,324 to a high of 7,949 (1998, p. 32, Table 3). Rind et al. interpreted the small correlations between psychological symptoms and child sexual abuse experiences to be supportive of their argument that CSA is not necessarily harmful. A simpler explanation for the small correlations is that many mediating factors degrade their sizes.

Specifically with regard to the relationship between child sexual abuse and psychological adjustment, Rind et al. concluded that “[t]he small magnitude of all effect size estimates implied that CSA effects or correlates in the college population are not intense for any of the 18 meta-analyzed symptoms” (1998, p. 32). Later in their paper, when they “analyzed the relationship between family environment and CSA in the college samples to determine whether they were confounded as a first step in examining whether CSA caused symptoms” (1998, p. 38), Rind et al. concluded that “the majority of significant CSA-symptom relations examined . . . may have been spurious. These results imply that significant CSA-symptom relations in studies based on college samples cannot be assumed to represent effects of CSA” (1998, p. 40). They

summarized, “CSA-symptom relations could be underestimated relative to family environment-symptom relations because of unreliability of CSA measures, low base rates for CSA, and artificial dichotomization of CSA” (1998, p. 41). Underestimation appears to be quite likely; several earlier studies found the family environment to be significant as an influence on the behavioral and emotional life of individuals sexually abused as children; moreover, knowledge of the family environment can be invaluable to supplement information obtained from the victims themselves (Bushnell, Wells, & Oakley-Browne, 1992; Finkelhor, Moore, Hamby, & Straus, 1997).

Rind et al. failed to hypothesize a priori that, for example, certain psychological problems should show statistically significant correlations with child sexual abuse based on a stated theoretical construct; instead, they treated all 18 psychological adjustment correlates as if they were of equal weight, which suggests that they were simply noting that 17 of the 18 indicators supported their position that child sexual abuse had few statistically significant, even marginally negative effects. Nor did they offer a posteriori any hypotheses regarding future, more detailed study of any of the indicators. Essentially, their investigation of the 18 psychological adjustment indicators lacked theoretical focus. Some psychological adjustment symptoms have been found to be common across a number of studies: anxiety disorder, depression, self-esteem, sexual problems, and suicidal behaviors, as well as conduct disorder, sleep and somatic disorders, and substance abuse disorder (Fergusson, Horwood, & Lynskey, 1996; Kendall-Tackett, Williams, & Finkelhor, 1993; Swanston, Tebbutt, O’Toole, & Oates, 1997).

The wide variability in the types of psychological adjustment symptoms identified in the samples Rind et al. examined (1998, p. 32, Table 3) overlap with the symptoms identified by other researchers. Least often found are hostility and phobia (five studies), while most often found is depression (23 studies); seven types of symptoms (anxiety, depression, low self-esteem, poor sexual adjustment, poor social adjustment, somatization, and poor general psychological adjustment) were found in 15 or more of the studies. Rind et al. failed to consider these findings either in their study design or in their discussion of their own meta-analytic findings.

CONFOUNDING OF GENDER AND AGE AND UNDERREPRESENTATION OF MALES

From their meta-analytic study, Rind et al. concluded that young women experienced more severe negative reactions to and effects from earlier child sexual abuse than did young men. In fact, according to the authors, the reac-

tions of many of the young men in their college samples who had experienced earlier abuse were either neutral or positive. This finding, Rind et al. acknowledged, disputes the findings of other investigators who have asserted that child sexual abuse is an equivalently negative experience for both males and females in both scope and effect.

Our sixth methodological question concerns inadequacy of the gender sampling reported, the confounding of gender and age in the meta-analysis, and the underrepresentation of males relative to females in the child sexual abuse literature. Rind et al. emphasized that their “[meta-analytic review] goal was to address the question: In the population of persons with a history of CSA, does this experience cause intense psychological harm on a widespread basis for both genders?” (1998, p. 22). They indicated:

Bauserman and Rind (1997), on the basis of a review of college, national, and convenience samples, concluded that reactions and outcomes for boys are more likely to be neutral or positive than for girls. Many reviewers, however, have concluded or implied that CSA is an equivalent experience for boys and girls in terms of its negative impact (e.g., Black & DeBlassie, 1993; Briere & Runtz, 1993; Mendel, 1995; Urquiza & Capra, 1990; Watkins & Bentovim, 1992). Black and DeBlassie stated that CSA ‘has, at the very least, an equivalent impact on males and females’ (p. 128). Watkins and Bentovim claimed that one prevalent myth about CSA is that boys are less psychologically affected than girls. Mendel dismissed as an ‘exercise in futility’ efforts to determine whether boys or girls are more adversely affected by CSA, and concluded that CSA ‘has pronounced deleterious effects on its victims, regardless of their gender’ (p. 101). (1998, p. 24)

Concerning comparability of male and female reactions, Rind et al. reviewed 15 studies of self-reported effects of child sexual abuse experiences (1998, p. 36, Table 7). Ten studies involved 1,421 females; 11% considered their child sexual abuse to have been a positive experience, 18% considered it to have been neutral, and 72% considered it to have been negative. Nine studies involved 606 males; 37% considered their abuse to have been positive, 29% considered it to have been neutral, and 33% considered it to have been negative. (Rind et al. noted that the percentages do not total exactly 100 because of rounding.) Nevertheless, while the gender differences reported in these studies regarding negative effects from child sexual abuse are great, that one-third of the reporting males indicated the effects to be negative is not an insignificant number.

In order for Rind et al. to validly compare effects of child sexual abuse on females with its effects on males, it is essential that at least three methodologi-

cal prerequisites of population sampling be comparable in the studies of both genders, specifically, the definition of child sexual abuse, the measurement of abuse prevalence rate, and the sampling method. One way of testing for comparability would be to determine whether the same type of relationship exists between sample sizes and abuse prevalence rates within the gender samples; to determine these relationships the senior author of the present article (J.A.W.) calculated Spearman rank correlation coefficients, a nonparametric statistic.

Of the 70 independent samples listed in the Rind et al. Appendix (1998, pp. 52-53) that provided data usable for meta-analytic estimation, 45 (64%) involved only female college students, whereas 25* (36%) involved only male college students. In these samples, 5,851 (27%) of the 21,999 women self-reported reactions to and effects from child sexual abuse as compared with 1,903 (14%) of the 13,651* men. (The two asterisks note numerical discrepancies. In their Results section [1998, p. 29], Rind et al. indicated that the male samples totaled 26. Apparently, one study [$N = 53$] was not included in the Appendix. When sample sizes are calculated from the percentages listed in the Appendix, therefore, the total is not 13,704. Rounding is not the cause; there are no discrepancies in the female figures.)

The 45 female samples revealed a statistically significant positive relationship between sample sizes and abuse prevalence rates ($r = .60, 43 \text{ df}, p < .005$ [1-tail test]). In contrast, the 25 male samples revealed a nonsignificant relationship ($r = .21, 23 \text{ df}, p > .05$ [1-tail test]).

These findings indicate that at least one methodological prerequisite was not met. The definition of child sexual abuse, as well as the measurement of its prevalence rate, might have differed for females and for males. The sampling method for the males might also have differed from study to study; some investigators might have explicitly sought individuals who had experienced child sexual abuse, others might have used random sampling strategies, and still others might have simply relied upon convenient availability of subjects. Thus, in addition to the inappropriateness of sampling only college students when attempting to assess the prevalence rate of child sexual abuse in the general population, which we have already addressed, basic differences between the gender samples themselves make even conclusions regarding college students inappropriate.

Moreover, concerning gender and age, the two measures were confounded in the meta-analysis. Rind et al. did not present the ages of the individual males and females who were sexually abused as children, only their upper ages are given in the Appendix (1998, p. 53). Because many researchers have found that girls are younger than boys when they are initially abused sexually, confounding of gender and age is likely (Fergusson, Lynskey, & Horwood, 1996; Silverman, Reinherz, & Giacona, 1996). Besides the basic biological and de-

velopmental differences between males and females, today's social norms regarding the sexually related behavior of females, including peer pressure, still differ substantially from those regarding the sexually related behavior of males, particularly during adolescence (De Gaston, Weed, & Jensen, 1996; Lab, Feigenbaum, & De Silva, 2000).

Concerning the underrepresentation of males relative to females in the child sexual abuse literature, Lab, Feigenbaum, and De Silva (2000) discuss the fact that for various cultural and social reasons far less research has been done on males than on females who experienced sexual abuse as children. When queried about their experiences, for example, males tend either to volunteer less frequently than females do or to respond less frankly than females do (Widom & Morris, 1997; Williams, 1994). Such underrepresentation is borne out in the studies sampled by Rind et al. As noted earlier, of the 70 independent samples identified for estimating prevalence rates, 64% involved women and 36% involved men (of the total of 35,650 college students, 62% were women and 38% were men, a ratio of approximately 3 to 2); 27% of the women and 14% of the men self-reported child sexual abuse. From the 59 usable studies examined by the authors, effect size data were based on 15,824 participants (3,254 men from 18 samples and 12,570 women from 40 samples), a ratio of almost 4 to 1.

LIMITATIONS OF THE RETROSPECTIVE CROSS-SECTIONAL APPROACH

Retrospective cross-sectional studies, in which descriptive data are collected at a single point in time from a specific population regarding events that occurred during one or more earlier time periods, have dominated child sexual abuse research. They are inexpensive and easy to conduct, and they can be initiated and completed within a relatively short time. Compared with long-term, scientifically rich longitudinal studies that are far more expensive and demanding, however, they are limited to information collection, descriptive analyses, and the development of theoretical constructs and hypotheses; it is difficult to test hypotheses in retrospective cross-sectional studies.

Our seventh and most important methodological question concerns the major limitation of the retrospective cross-sectional approach used by Rind et al. Reliance on but a single human source, the victim, to accurately depict events and emotions associated with a probably traumatic experience that occurred many years earlier, as well as the present psychological adjustment of that individual, raises issues regarding both validity and reliability. Rind et al. reported that "[f]or each study . . . information . . . coded [included] (f) all reaction data, if provided, including both retrospectively recalled reactions to

and current reflections on the CSA experiences; (g) all self-reported effects data, if provided, including responses to how these experiences affected participants overall and how they affected their sex lives . . . ” (1998, p. 27).

All of the college samples included in the meta-analysis used the retrospective cross-sectional approach. The students had been asked to recall their child sexual abuse experiences and their reactions to them then, as well as their current reflections about the earlier experiences. The limitations of this type of research are well documented. Three specific limitations (distortion of information, absence of information, and personal maturation) affect the conclusions that can be drawn in child sexual abuse research.

With regard to the distortion of information, recall of earlier events and potentially emotionally and physically traumatic reactions to them have been found to be incomplete and inaccurate. People forget, especially events in the distant past; they tend to remember events as having occurred earlier than the actual dates; and they often reinvent the past to suit their current circumstances and needs (Briere, 1992; Henry, Moffitt, Caspi, Langley, & Silva, 1994). Rind et al. cited a controlled classroom study conducted earlier by two of them, which illustrates the problem of fallibility in retrospective recall. Rind and Bauserman (1993) found that students’ judgments concerning adult-child sexual relations were biased negatively in their experiment by the simple use of negative terminology as opposed to neutral terminology.⁴

With regard to the absence of information, the only source of information regarding whether the college students experienced sexual abuse as children and subsequent effects later was the students themselves. Apparently, no other sources were checked for corroboration, such as family members, friends, peer groups, coworkers, and medical, educational, clinical, and legal records. This is a significant oversight.

With regard to personal maturation, the behavioral and emotional development of the sexually abused individual from childhood to adulthood is strongly influenced by numerous factors. Specific socioeconomic, demographic, and cultural circumstances of the abuse as well as the amount and type of familial support that was available are particularly important. Myriad experiences during the time between abuse and recall also affect that recall. Perceptions and attitudes about the earlier child sexual abuse may be altered by the experiences that occur in the intervening years, such that as a young adult an individual’s reactions may be quite different (Briere, 1992). For example, if negative terminology as compared with neutral terminology could impact the results of the Rind and Bauserman (1993) classroom study, it goes without saying that many years of diverse cultural, social, and emotional experiences in the real world could have an even greater impact on the validity and reliability of the college students’ self-reports of sexual abuse experienced much earlier. Retrospective

studies are extremely limited in their ability to collect information regarding these critical interactive variables.

During the past decade organizational structures have been developed for the long-term planning, coordination, and conduction of longitudinal research (Herrenkohl, Herrenkohl, Egolf, & Wu, 1991; Runyan et al., 1998). In fact, several notable longitudinal studies have been conducted (e.g., Bagley & Mallick, 2000; Fergusson, Horwood, & Lynskey, 1997; Herrenkohl, Egolf, & Herrenkohl, 1997). While longitudinal studies are limited by such factors as economics and the participation of different corroborating informants such as parents and teachers (Briere, 1992; Frothingham et al., 2000), the methodological and scientific obstacles are fewer than those that are found in retrospective cross-sectional studies.

CONCLUSION

In this critical appraisal, seven major concerns have been identified and discussed with regard to the methodology used by Rind et al. in their meta-analytic review of child sexual abuse outcomes. Child sexual abuse is a pressing public problem that demands defensible research that can lead to better ways of identifying such abuse and, consequently, to better intervention and prevention procedures.

In particular, valid conclusions about the nonharmfulness of CSA cannot be drawn from Rind et al.'s poorly supported findings. The fact that this problem area is so very difficult to adequately investigate makes solid research design and conduct even more important. One obstacle has been and continues to be the development of a meaningful definition of child sexual abuse (Haugaard, 2000). A broader conceptualization of child sexual abuse to include all forms of child and adolescent sexual exploitation is needed (Itzin, 1997); this will facilitate articulation of the researchable definition.

A literature review of child sexual abuse research not cited by Rind et al. in their 1998 meta-analysis is presented in our second article in this volume. While some of the studies cited in our second article were published after Rind et al.'s meta-analysis, many papers published earlier could and should have been reviewed by Rind et al. before they undertook their massive effort. Their methodology and conclusions would have been different if they had included the omitted research.

NOTES

1. In χ^2 analyses, expected frequencies are determined from the marginal totals in a 2×2 design. When the expected and the observed frequencies are close in size, the resulting χ^2 values will not be statistically significant; as the size of the differences increases, the probability of obtaining statistical significance increases. The direction of

the differences is also important; when the observed frequencies are larger than the expected frequencies, that group is reporting more abuses than the comparison group, and when the observed frequencies are smaller, that group is reporting fewer abuses than the comparison group.

2. Specific problems with Rind et al.'s representation of the data from the three national studies are described in Dallam, S. J., Gleaves, D. H., Cepeda-Benito, A., Silberg, J., Kraemer, H. C., & Spiegel, D. (in press). The effects of childhood sexual abuse: A critique of Rind, Tromovitch and Bauserman (1998). *Psychological Bulletin*.

3. Our concerns about Rind et al.'s emphasis on perceived willingness as a factor in psychological outcome are addressed in our second article in this volume (pp. 157-182).

4. Our concerns about Rind et al.'s preference for using neutral terminology in child sexual abuse research are addressed in our second article in this volume (pp. 157-182).

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