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The Effects of Child Sexual Abuse: Comment on Rind, Tromovitch, and Bauserman (1998)

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B. Rind, P. Tromovitch, and R. Bauserman (1998) examined the long-term effects of childhood sexual abuse (CSA) by meta-analyzing studies of college students. The authors reported that effects "were neither pervasive nor typically intense" and that "men reacted much less negatively than women" (p. 22) and recommended value-neutral reconceptualization of the CSA construct. The current analysis revealed numerous problems in that study that minimized CSA–adjustment relations, including use of a healthy sample, an inclusive definition of CSA, failure to correct for statistical attenuation, and misreporting of original data. Rind et al.'s study's main conclusions were not supported by the original data. As such, attempts to use their study to argue that an individual has not been harmed by sexual abuse constitute a serious misapplication of its findings.

"A Meta-Analytic Examination of Assumed Properties of Child Sexual Abuse Using College Samples," published in the July 1998 issue of *Psychological Bulletin*, resulted in an unprecedented amount of media and political attention. The study's authors, Rind, Tromovitch, and Bauserman, argued that the media has promoted the idea that child sexual abuse (CSA) "produces intensely negative effects for all of its victims" (p. 22) and that, as a consequence, many professionals assume that sex between adults and children invariably causes "intense" and "pervasive" harm in both genders (p. 22). To test these assumptions the authors analyzed 59 studies of college students. Their article's main findings can be summarized as follows:

1. "For all symptoms but one, CSA participants as a group were slightly less well adjusted than control participants" (p. 32); "the magnitude of this association (i.e., its intensity) was small" (p. 42).
2. Family environment "explained considerably more adjustment variance than CSA" (p. 22).
3. "When negative effects occur, they are often temporary, implying that they are frequently not intense" (p. 37).
4. "The relation between CSA and adjustment problems was generally stronger for women than men" (p. 42); "men reacted much less negatively than women" (p. 22).
5. Sexually abused males "in the all-levels-of-consent group [which Rind et al. assumed included detectable amounts of willing experiences]. . . were unique in terms of not differing from their controls in adjustment" (p. 34).

Rind et al. (1998) concluded that basic assumptions about CSA were not supported and that the harm associated with sexual relationships between adults and children, especially those involving boys, has been exaggerated—a viewpoint entirely consistent with their prior writings in this area (e.g., Bauserman, 1989, 1990; Bauserman & Rind, 1997; Rind, 1995a, 1995b; Rind & Bauserman, 1993; Rind & Tromovitch, 1997). They went on to suggest that when labeling events that have "heretofore been defined sociolegally as CSA," a more "scientific" approach would be to focus on the young person's perception of the experience:

> A willing encounter with positive reactions would be labeled simply *adult–child sex*, a value-neutral term. If a young person felt that he or she did not freely participate in the encounter and if he or she

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> experienced negative reactions to it, then *child sexual abuse*, a term that implies harm to the individual, would be valid. (p. 46)

Rind et al.'s (1998) article provoked enormous social controversy and debate. It was celebrated by pedophile advocates (e.g., Riegel, 2000), drew mixed reviews from psychologists (e.g., Byrd, Scharman, & Lauritsen, 1999; Tavris, 1999), and sparked outrage in the popular media (e.g., Saunders, 1999) and in political circles (see Duin, 1999). In the end, the study's conclusions were formally (and unanimously) denounced by both the U.S. House of Representatives (H. R. Cong. Res. 107) and the U.S. Senate. It is believed to be the first time in U.S. history that a scientific study was formally repudiated by a legislative body. Despite the controversy surrounding it, the article by Rind et al. has been referenced in legal proceedings. For example, in *State of Arizona v. Steward* (1999) a convicted child molester used the article to argue for leniency, saying that research shows that children are rarely harmed by sexual molestation. In deposition testimony, a psychologist working as an expert witness for a pedophilic priest relied on the study to form the opinion that a victim's psychological injuries were not due to sexual abuse, because the current scientific literature does not support the existence of a relationship between CSA and maladjustment in the population at large (Brainerd, 1999). The article by Rind et al. has also been cited as evidence that the legal age of consent for sex should be lowered (Graupner, 1999, see Dallam, in press, for a more complete review).

In the current review, we examine the data and methods that Rind et al. (1998) used to form their conclusions. Please note that the purpose of our article is not to argue that all types of sexual abuse do in fact cause pervasive and intense harm in all victims. Indeed, it is well recognized in the empirical literature that the aftereffects of CSA are extremely varied and that a significant percentage of abused children remain asymptomatic (Beitchman, Zucker, Hood, DaCosta, & Akman, 1991; Beitchman et al. 1992; Browne & Finkelhor, 1986; Kendall-Tackett, Williams, & Finkelhor, 1993). Instead, because of their important implications for public policy, we seek to examine the validity and generalizability of Rind et al.'s results.

The purpose of meta-analysis is to combine information from independent studies that address a similar question to provide more accurate estimates of the effects being measured. The potential for meta-analysis to add to research knowledge is great, provided that the meta-analysis meets high standards and well-designed studies are available on the subject matter. Rind et al. (1998) suggested that quantitative literature reviews can avoid "subjectivity and imprecision by using meta-analysis" (p. 24). Although this is certainly the goal, meta-analyses are essentially retrospective analyses of previously collected data. Thus, problems in design or reporting may be uncritically carried into the analysis and influence its results. In other words, the flaws and biases associated with individual studies do not necessarily cancel each other out when studies are combined meta-analytically (Matt & Navarro, 1997). Consequently, it has been recommended that readers look carefully at the studies that were included in a meta-analysis, and require evidence of thoughtful and deliberate conduct from any meta-analytic study before accepting its conclusions (LaValley, 1997).

For our review, we retrieved and examined all published studies analyzed by Rind et al. (1998), along with abstracts of the unpublished dissertations. If a dissertation's abstract did not provide adequate information, a copy of the complete manuscript was obtained. Using guidelines provided by Rosenthal (1994, 1995), we examined criteria for inclusion, study quality, independence, study characteristics, dependent measures, and effect size estimates. We did not, however, attempt to completely replicate the meta-analysis; rather, we looked carefully at how it was conducted and the strength of the evidence used to support Rind et al.'s major findings.

Results

The Limitations of the Method Used to Determine Harm

The article by Rind et al. (1998) provided a coherent research protocol that outlined their data collection and statistical methods. Rind et al. also did a thorough job in tracking down published and unpublished studies of college students that addressed their research question and ensured that the studies were independent (i.e., did not use overlapping samples). Despite these strengths, we identified a number of important limitations in their study's ability to answer the authors' research questions concerning the prevalence and intensity of harm associated with CSA. Some of these limitations were intrinsic to the primary data set, whereas others originated in the Rind et al. study's design. We briefly review each of these problems and describe how they may affect or qualify the findings of the meta-analysis.

Sample Bias: The Use of College Students

Rind et al. (1998) dismissed the conclusions of previous critical reviews, such as Kendall-Tackett et al. (1993) and Beitchman et al. (1992), along with previous meta-analytic reviews, such as Jumper (1995) and Neumann, Houskamp, Pollock, and Briere (1996), with the argument that these reviews were based largely on clinical and legal samples not representative of the general population. However, rather than examining the general population, Rind et al. (1998) examined college students: a young, well-functioning portion of the population. Although the use of college students offers advantages in terms of the availability of a large number of studies using a wide variety of outcome measures, it also limits the generalizability of the study's findings. We identified several major problems associated with relying exclusively on college students to determine whether CSA causes intense or pervasive long-term harm in the general population.

Potential exclusion of those most severely affected. As Haugaard and Emery (1989) noted, "if child sexual abuse is debilitating, fewer victims may be found among college students and these victims may have made a better adjustment to their abuse, since colleges often require above average academic and social performance from entering students" (pp. 89–90). Empirical support for this concern can be found in numerous studies that have identified a relationship between CSA and academic difficulties (e.g., Boney-McCoy & Finkelhor, 1995; Chandy, Blum, & Resnick, 1996; Erickson & Rapkin, 1991; Kendall-Tackett et al., 1993; Lisak & Luster, 1994), failing to finish high school (Edgardh & Ormstad, 2000), and failing to remain in college (Duncan, 2000). These data suggest that by including only studies of college

students, Rind et al. may have excluded some of the individuals most severely affected.

Exclusion of relevant outcomes. The use of college samples also limited the scope of dependent measures available for study. Most studies of CSA–adjustment relations in college students have used generic measures of internalizing behaviors such as depression, anxiety, and eating disorders. Unfortunately, few investigators have studied nonclinical populations using measures specifically designed to detect symptoms of posttraumatic stress. As a result, the meta-analysis by Rind et al. (1998) did not include any measures of posttraumatic stress disorder (PTSD) one of the most commonly reported aftereffects associated with sexual abuse in children and adolescents (Cuffe et al., 1998; McLeer, Deblinger, Henry, & Orvaschel, 1992; Merry & Andrews, 1994).

The relationship between CSA and externalizing has also been neglected in college samples. The absence of such measures may have caused Rind et al. (1998) to underestimate the adverse effects associated with CSA, as numerous nonclinical studies of high school students have reported that CSA is associated with a wide variety of high-risk behaviors, including antisocial behavior, conduct disorders, self-destructive behavior, substance abuse, younger age at first coitus, more frequent and risky sexual activity, not using condoms or birth control, sexually transmitted diseases, increased HIV risk, and teen pregnancy (Bensley, Spieker, Van Eenwyk, & Schoder, 1999; Bensley, Van Eenwyk, Spieker, & Schoder, 1999; Fiscella, Kitzman, Cole, Sidora, & Olds, 1998; Harrison, Fulkerson, & Beebe, 1997; Hibbard, Brack, Rauch, & Orr, 1988; Hibbard, Ingersoll, & Orr, 1990; Nagy, DiClemente, & Adcock, 1995; Stock, Bell, Boyer, & Connell, 1997). Moreover, studies of high school students have reported that sexual abuse has a particularly negative impact on the behavior of adolescent males (e.g., Chandy et al., 1996; Garnefski & Arends, 1998; Hibbard et al., 1990). Chandy et al. (1996) examined gender-specific outcomes for 370 abused boys and 2,681 abused girls who were identified in a study of over 36,000 students in Grades 7 to 12. Compared with their female counterparts, male adolescents who acknowledged experiencing CSA were at significantly higher risk for poor school performance, delinquent activities, sexual risk taking, and dropping out of high school. These results suggest that by restricting their analysis to college samples, Rind et al. may have missed some of the most harmful effects associated with CSA, particularly those found in boys.

Comparison to national samples. Despite the problems with age restriction and the use of a healthy population, Rind et al. (1998) argued that their results should be considered generalizable to the population as a whole. Their argument was based in large part on their assertion that the (a) prevalence, (b) severity, and (c) effects of CSA in their college sample were similar to those found in three studies using nationally representative samples: Baker and Duncan (1985; Great Britain); Laumann, Gagnon, Michael, and Michaels (1994; United States); and López, Carpintero, Hernández, and Fuertes (1995; Spain) (see Rind et al., 1998, Table 1).

We examined each of these studies and found that meaningful comparisons were limited by the fact that CSA definitions, outcome measures, and data collection methods differed across the studies. Moreover, when meaningful comparisons were possible we found that Rind et al. (1998) often presented the data in a misleading manner. For example, whereas college samples categorized victims by highest category of abuse severity they had experienced, Rind et al. averaged simple frequency counts to obtain numbers for the national samples. For instance, 28% of college students reported exhibition to be the most severe form of abuse they had experienced. Because López et al. (1995) was the only national study that reported exhibition, Rind et al. noted that numbers they reported for the prevalence of exhibition in national samples (33% for male and female samples combined) came from this study (see Rind et al., Table 1). When we translated López et al.[1], we found that the authors had reported abuse severity using two methods: (a) they provided a simple frequency count of each type of abuse experienced (the sum of which is more than 200%) and (b) they categorized respondents by the highest category of abuse severity experienced (see López et al., 1995, Table 2). Therefore, whereas López et al. reported that 33% of their combined sample had experienced exhibition, only 16% of Spanish nationals had reported exhibition as the most severe form of abuse they underwent. Rather than reporting the data from López et al. that was comparable to that reported for students (16%), Rind et al. reported data from the simple frequency count (33%). This method of data reporting made it appear that national samples and college samples had reported a similar abuse severity, while masking the fact that Spanish nationals had reported a higher prevalence of contact CSA than college samples.

To allow readers to more fully appreciate how college and national studies differed, we compare prevalence rates, as well as levels of CSA severity (i.e., exhibitionism, fondling, oral sex, and intercourse) and number of occurrences for college samples and the three national surveys cited by Rind et al. (1998) in our Table 1. This table shows that the severity of abuse reported by U.S. college students differs with that reported in Spain. The other two national samples cited by Rind et al. either did not provide information on abuse severity (i.e., Baker & Duncan, 1985) or did not provide data that could be broken down and categorized in a comparable format (i.e., Laumann et al., 1994). As a whole, these studies offered little information appropriate to determine whether the severity of abuse experienced by U.S. college students is similar to that experienced by the U.S. population as a whole.

A study more relevant to the U.S. general population (which was included in Rind and Tromovitch's, 1997, study, but which Rind et al., 1998, failed to cite) is Finkelhor, Hotaling, Lewis, and Smith (1990). Finkelhor et al. reported the results of a *Los Angeles Times* national telephone survey of 2,626 American men and women 18 years of age or older. CSA was reported by 16% of the men and 27% of the women, which corresponds well with the prevalence rates of 14% for men and 27% for women that Rind et al. reported for college samples. However, Finkelhor et al. reported that 62% of men and 49% of women reported experiencing actual or attempted intercourse. These numbers are more than double those reported for college students (33% of men and 13% of women) and do not support Rind et al.'s claim that "SA [sexually abused] students experienced as much intercourse. . . as persons in the general population" (p. 44).

[1] Antonio Cepeda-Benito, whose native language is Spanish, translated this article.

Table 1
Comparisons of Prevalence Rate Estimates of Types of CSA in College and National Samples

Sample	*N*	CSA (%)	Exhibitionism (%)	Fondling (%)	Oral sex (%)	Intercourse (%)	Multiple occurrences (%)
U.S. college							
(as reported by Rind et al., 1998)							
Female	2,172	27	32	39	3	13	—
Male	506	14	22	51	14	33	—
Combined[a]	2,918	22	28	42	6	17	46
U.S. national							
Laumann et al., 1994							
Female	269	17	—[b]	—	—	—	—
Male	159	12	—	—	—	—	—
Combined	428	16	—	—	—	—	66
Foreign national							
Baker & Duncan, 1985 (Britain)							
Female	915	12	—	—	—	5	34
Male	844	8	—	—	—	5	41
Combined	1,759	10	—	—	—	5	37
López et al., 1995 (Spain)							
Female	203	15[c]	23	55	5	13[d]	47
Male	134	22	5	71	8	6	40
Combined[e]	337	19	16	62	6	10	44

Note. People were categorized into the most severe form of abuse experienced, with exhibitionism considered the least serious and intercourse considered the most. Direct comparisons between prevalence rates are problematic because the definition of child sexual abuse (CSA) differed considerably across studies. Dashes indicate that data were not applicable.
[a] Rind et al. indicated that combined values were based on two additional studies (with a male and female sample in each) that reported only combined results. [b] Laumann et al. reported percentages for various forms of abuse but did not categorize people by highest level of CSA reported. Instead, they reported the percentage of people experiencing each category of abuse, and experiences with male and female perpetrators were counted separately. Because of this, one person could have contributed anywhere from once to one category to twice to every category. Because there was no way to determine what percentage of participants experienced any form of abuse, comparable numbers cannot be computed. [c] Prevalence rates based on $N = 1,821$. [d] López et al. reported both attempted and actual intercourse, and we combined these numbers. [e] Numbers do not equal 100% because López et al. also included a separate category for being propositioned.

Rind et al. (1998) also compared effect sizes for college samples and national samples and reported that "the magnitudes of CSA–adjustment relations in the college samples and in the national samples meta-analyzed by Rind and Tromovitch (1997) were identical, $r_u = .07$ for men, and $r_u = .10$ for women" (p. 42). We evaluated this claim by examining the three national samples Rind et al. referenced in their article, along with two surveys of the U.S. population (Finkelhor et al., 1990; Boney-McCoy & Finkelhor, 1995) also included in Rind and Tromovitch's analysis. Effect sizes were computed for the three national surveys that provided appropriate outcome data: Laumann et al. (1994), López et al. (1995), and Boney-McCoy and Finkelhor (1995). Our results, which are displayed in Table 2, show that the effect sizes that Rind et al. reported for college students (after appropriate correction for base-rate differences) were similar to those we estimated for Laumann et al., who studied sexual dysfunction in the U.S. population, but lower than those estimated for Boney-McCoy and Finkelhor, who studied PTSD in a national survey of adolescents, or for López et al., who asked participants about emotional and behavioral problems.

In summary, prevalence, severity, and effect sizes varied considerably across college and national samples. These results contradict Rind et al.'s (1998) contention that "the college data were completely consistent with data from national samples" (p. 22) and provide little support for the claim that their findings should be considered generalizable to the population as a whole.

Independent Measures: Lack of Operational Definition of CSA

It is obvious that how CSA is defined is critical for studying its effects and that varying definitions cause difficulties when combining data for study. In the current meta-analysis, the use of studies that lacked a common definition was unavoidable, as there are nearly as many different definitions of abuse as there are studies (e.g., see Appendix from Rind et al., 1998, pp. 52–53). Definitions of CSA ranged from whatever the victim thought the term *sexually molested* meant (e.g., Fritz, Stoll, & Wagner, 1981) to more structured definitions (e.g., Finkelhor, 1984). Structured definitions differed widely on the upper age limit for the victim to be classified as a child; some set the age at puberty, others picked an age somewhere between 12 and 17. This resulted in similarly abused adolescents being placed in the study group in some studies and in the nonabused control group in others. Studies also varied as to what constituted a perpetrator, whether the sexual experience had to be forced, and whether any physical contact between the perpetrator and the child was required. As might be expected, studies that used broader definitions of CSA reported higher prevalence rates. The variability in definitions of CSA is reflected in prevalence rates in the primary data set that varied from 3% to 71%.

Rind et al. (1998) compounded this lack of standardization in the literature by failing to delineate any inclusion or exclusion criteria and analyzing all studies that were even remotely relevant

Table 2
Comparisons of Effect Sizes in College and National Samples

Sample	Prevalence of CSA	Point biserial (r) corrected for base rates[a]
U.S. college (as reported by Rind et al., 1998)		
Female	27%	.11
Male	14%	.10
National		
Laumann et al., 1995 (U.S.)[b]		
Female	17%	.11
Male	12%	.11
López et al., 1995 (Spain)[c]		
Combined	19%	.16
Boney-McCoy & Finkelhor, 1995 (U.S.)[d]		
Female	15%	.22
Male	6%	.17

Note. Direct comparisons between effect sizes are problematic because the definition of child sexual abuse (CSA) and dependent measures differed considerably across studies.
[a] We corrected for base rates on the basis of data reported by Rind et al.; however, these adjustments should in practice be made at the level of individual study effect sizes. [b] The data from Laumann et al. came from a larger study of sexual activity and practices. Participants who reported being sexually touched prior to age 18 by an older person were asked to answer questions about sexual difficulties in the last year. [c] For López et al., we combined the two outcome measures reported (only one of which was broken down by gender). [d] Boney-McCoy and Finkelhor reported effects for posttraumatic stress disorder in U.S. youth (age 10–16) after controlling for demographic and parental factors.

to their research questions. Thus, in contrast to what we consider to be an underinclusive selection of populations to be studied, Rind et al. tended to be overinclusive in the studies they used in their primary data set. For example, Rind et al. included a number of studies in their primary data set that did not even purport to examine the effects of CSA. Landis (1956), for example, examined experiences with "sexual deviants" at any age, Schultz and Jones (1983) looked at all types of "sexual acts" before age 12, and Sedney and Brooks (1984)[2] examined all types of "sexual experiences" during childhood. Some studies even included sexual experiences that occurred after age 17 (e.g., Greenwald, 1994; Landis, 1956; Sarbo, 1985).

At the same time, Rind et al. (1998) excluded from analysis two studies that examined the effects of incest: Jackson, Calhoun, Angelynne, Maddever, and Habif (1990) and Roland, Zelhart, and Dubes (1989). These studies were eliminated because Rind et al. claimed that their relatively large effect sizes (r = .36 and r = .40, respectively) were outliers in the higher direction. In our opinion their elimination, given a close reading of Rind et al., is quite baffling. In a footnote Rind et al. noted that these two studies "*may capture more accurately the essence of abuse in a scientific sense* [italics added]—that is, more persuasive evidence of harm combined with the likely contextual factors of being unwanted and perceived negatively" (p. 46).

This statement raises an obvious question: If it were these samples that, in a scientific sense, more accurately captured the essence of abuse and are clearly more consistent with what the public associates with CSA, why did Rind et al. (1998) choose to exclude these studies while including studies (such as those including consensual peer experiences) that did not capture the essence of abuse in any way? Upon further investigation, we found that these studies were only outliers because Rind et al. erroneously coded a third study, Silliman (1993), in the lower direction. Had Silliman not been miscalculated, the entire distribution would have shifted slightly in terms of effect sizes, and Jackson et al. (1990) and Roland et al. (1989) would no longer have been statistical outliers.[3] It should be noted that inclusion of these outliers would not have substantively changed the overall findings of Rind et al.'s meta-analysis. However, the inclusion of a large number of studies that looked at milder forms of abuse or non-CSA experiences can be expected to minimize the likelihood of identifying any significant consequences experienced by the smaller number of people who underwent more severe forms of abuse. As Rind et al. examined this problem in moderator analysis, we address this issue in more depth in our review of this section of their article.

Dependent Measures: Omission of Relevant Measures

In reviewing the primary data set, we found a number of outcomes we consider relevant to the question of harm that Rind et al. (1998) failed to report. For example, a number of studies asked about illegal drug use, revictimization, and whether respondents had been treated for emotional problems. In each case, those acknowledging a history of CSA indicated more problems in these areas than their peers. For example, revictimization, defined as sexual assault or rape occurring after age 18, was a dependent measure that was reported in at least five of the studies included in the authors' primary data set (i.e., Alexander & Lupfer, 1987; Fromuth, 1986; Gidycz, Coble, Latham, & Layman, 1993; Wisniewski, 1990; Zetzer, 1991).

To assess the significance of the exclusion of data on revictimization, we meta-analyzed the data from these studies along with data from three more recent studies that used college samples (Humphrey & White, 2000; Johnsen & Harlow, 1996; Schaaf & McCann, 1998). The association between CSA history and revic-

[2] For example, Neumann et al. (1996) rejected this study from their meta-analysis of long-term sequelae of CSA, noting that the study reported data "which may not meet the criteria for sexual abuse" (p. 8).

[3] Silliman (1993) was a one-page report with irreconcilable problems with what little data were provided. However, rather than clarify the problems with the author, Rind et al. (1998) assumed that the abused group scored much higher on self-esteem than the nonabused group and calculated an effect size of approximately −.49 (when averaged with the effect size for the other dependent variable in the study, one arrives at the effect size of −.25). To clarify the statistics, we contacted the author of the original study and found that Rind et al.'s inference was incorrect. The mean values for the abused and control groups were 327 and 337, respectively (M. Riposo [formerly M. E. Silliman], personal communication, July 31, 1999). This mean difference corresponds to a point-biserial r of .16 and an overall effect for self-esteem of approximately .08. Had the Silliman report not been miscalculated and eliminated, the entire distribution would have shifted slightly in terms of effect sizes, and Jackson et al. (1990) and Roland et al. (1989) would no longer have been statistical outliers. We reanalyzed their data (using the r values reported by Rind et al. in their appendix) after coding Silliman correctly and without eliminating any outliers, and the results suggested a homogenous sample, $\chi^2(52, N = 15{,}872) = 67.85, p = .07$.

Table 3
Meta-Analysis of the Association Between Child Sexual Abuse and Revictimization in Female College Students

Revictimization	k	N	r_u	r_c	95% CI for r_c	H
Value after trimming outliers	7	7,202	.17	.22	.19–.25	11.08
Original value	8	7,295	.17			47.10*

Note. Cutting or trimming outliers was performed when effect sizes were heterogeneous, in an attempt to reach homogeneity. The 95% confidence intervals (CI) are based on final (cut or trimmed) distributions. k = the number of effect sizes (samples); N = total number of participants in the k samples; r_u = the unbiased effect size estimate (positive values indicate better adjustment for control participants); r_c = the biserial effect size after adjusting for base rates of child sexual abuse in each sample; H = within-group homogeneity (chi-square based on $df = k - 1$).
* $p < .05$.

timization was consistent across studies and robust (see Table 3). Moreover, several studies compared rates of revictimization for varying levels of CSA severity and found a linear relationship with more invasive experiences during childhood being associated with higher rates of revictimization during adulthood (Gidycz et al., 1993; Humphrey & White, 2000). Fromuth (1986) reported that the relationship between CSA and revictimization remained strong even after controlling for family environment. These results show that Rind et al. may have underestimated some of the adverse effects associated with CSA in the college populations by not including all available dependent measures in their analyses.

Statistical Analysis: Sources of Attenuation

Choice of Effect Size

Throughout their article Rind et al. (1998) used the Pearson correlation coefficient (r) as the effect size statistic rather than Cohen's measure of effect size (d), the standardized mean difference, between the CSA and control groups. Although there is no problem with the conversion of d to r per se, one must be careful with its interpretation because unlike d, r is affected by the base rates of the independent variable when it is dichotomous. As the prevalence of the independent variable approaches either 0 or 1, the value of r, for any fixed value of d, approaches 0. For example, an effect size of $d = 0.8$ is one that Cohen (1977) described as "large," and indeed with this effect size, comparing a randomly selected CSA and non-CSA sample, 71% of the time the CSA group is "more distressed" than the non-CSA group. However, as Figure 1 demonstrates, this "large" effect size (based on interpretation of d) translates to an r anywhere from 0 (when the prevalence of CSA approaches 0% or 100%) to a maximal value of .37 (slightly above medium on the r scale) when the prevalence is 50%. Thus, Rind et al.'s decision to use r as the effect size statistic for studies that mainly used group comparison designs with very unequal proportions in the respective groups created a situation in which clinically large effects could be represented by what appear to be small r values.

Other sources of attenuation. Hunter and Schmidt (1994) listed 10 sources of attenuation in meta-analyses (see Table 4), and the current study had problems with attenuation from most of these

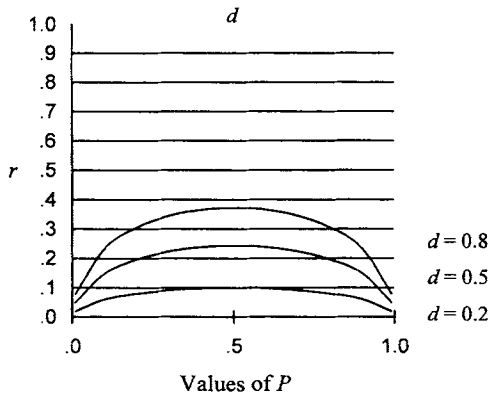

Figure 1. The relationship between r and d, based on the prevalence of the independent variable in the population studied. In this case, P represents the proportion of those in the sample with reporting child sexual abuse. Assuming that the two samples have the same variance, the exact relationship between r and d is: $r = d/(d^2 + 1/PP')^{1/2}$, where P is the probability that $X = 1$, and P' is the probability that $X = 0$ in the population sampled. As P approaches either 0 or 1, the value of r, for any fixed value of d, approaches 0.

sources. For example, there was attenuation due to dichotomization for the obvious reason that the studies were using dichotomies created from an underlying continuum. The attenuation due to error of measurement was large because of the definitional problems described above, and the use of biased samples led to attenuation due to range restriction. Some of these limitations were also mentioned by Rind et al. (1998). For example, they noted that correlations attenuate with small base rates (p. 41) and dichotomized or unreliable variables (p. 41) and that semipartial correlations will be conservative (p. 33). Although Rind et al. could not correct for all sources of attenuation, many sources, like attenuation due to base-rate differences and dichotomized variables, are easily correctable.

Base-rate differences for men and women. Although Rind et al. (1998) correctly noted that correlations attenuate with small base rates and that this problem was larger for male samples than

Table 4
Possible Sources of Attenuation of Effect Sizes and Whether They Were a Problem in the Rind et al. (1998) Meta-Analysis

Possible sources of attenuation	Problem in Rind et al.?
Random error of measurement in dependent variable	Partial
Random error of measurement in independent variable	Yes
Artificial dichotomization of continuous dependent variable	In some studies
Artificial dichotomization of continuous independent variable	Yes
Imperfect construct validity of the dependent variables	Yes
Imperfect construct validity of the independent variables	Yes
Range restriction on the independent variable	Yes
Range restriction on the dependent variables	Yes
Bias in the correlation coefficient	Minimal
Study-caused variation	Yes

female ones (p. 41), they chose not to correct for this problem, suggesting the results would be too small to bother with. They stated, "the attenuation is small in absolute magnitude for small effect sizes. For the small CSA–symptom effect size estimates obtained in the current review, adjusted effect size estimates, based on a 50-50 split increase *at most* [italics added] by .03 (based on formulas provided by Rosenthal & Rosnow, 1991)" (p. 41). At the same time, Rind et al. concluded that "the relation between CSA and adjustment problems was generally stronger for women than men" (p. 42).

Although we did not find such formulas in Rosenthal and Rosnow (1991), an appropriate formula can be found in Becker (1986).[4] When the point-biserial correlation is given directly, the formula for the corrected point-biserial r, r_c, is $r_c = r/\text{sqrt}(r^2 + 4pq(1 - r^2))$, where r is the uncorrected biserial correlation and p and q are the proportions in each of the two groups. By applying this formula to the overall r_us (listed in Rind et al., 1998, Table 4)[5] of .07 for men and .10 for women, and using the weighed prevalence rates of CSA that Rind et al. reported for men and women (14% and 27%, respectively), one can calculate r_cs of .11 for women and .10 for men. Thus, the overall effect size for men increases by .03, making the effect sizes for men and women, based on corrected point-biserial correlations, nearly identical. The authors admitted this fact in a subsequent article in which they defended their use of correlations and their decision not to correct for attenuation, stating, "we directly addressed this issue in our review. . . . We noted that effect size attenuation . . . is small in magnitude for small effect sizes—i.e., an $r = .07$ based on a 14-86 split [CSA prevalence for male samples] increases at most by .03 (to $r = .10$) in a 50-50 split" (Rind, Tromovitch, & Bauserman, 2000a, p. 29).

By applying the same correction, for the four attenuated r_u values of .13 (for anxiety, depression, paranoia, and psychotic symptoms) in their Table 3 (Rind et al., 1998, p. 32), one would obtain a corrected point-biserial r_c of .16 for both men and women. However, in practice, the base-rate problem should be attended to at the sample level, where, contrary to Rind et al.'s claim, effect sizes in some cases would have increased by much more than .03. For example, we examined Bendixen, Muus, and Schei (1994), whose male sample reported a CSA prevalence of only 3%. Our unbiased effect size estimate for the male sample ($r_u = .09$) was similar to the one reported by Rind et al. ($r_u = .08$). However, after correction for attenuation due to the low base rate of CSA, the effect size estimate was .26 (an increase of .17). These results show that using correlations without correcting for unequal base rates of the independent variable minimized effect sizes and created the appearance of gender-related differences in CSA adjustment, when effect sizes for men and women were actually equivalent.

The cost of dichotomization. Effect sizes were also attenuated because the original studies used dichotomies created from an underlying continuum. Although it is clear that CSA is a problem with varying degrees of severity (Bouvier et al., 1999; Rind et al., 1998; Wyatt & Peters, 1986), most studies dichotomized CSA. Point-biserial *r*s should be used only when there is a true dichotomy (e.g., men vs. women). When a dichotomy is created on the basis of an underlying continuum (as we would argue is the case with CSA),[6] power to detect differences is lost and the appropriate r is the biserial correlation, or the *tetrachoric correlation* if the dependent variable is also dichotomized (see Hunter & Schmidt, 1990). Cohen (1983) noted that even with a 50-50 distribution (which minimizes the attenuation) there is a 20% reduction in r (or a multiplicative factor of .80) if one variable is dichotomized, and a 40% reduction (or multiplicative factor of .64) if both are. With one variable dichotomized and a 90-10 split (as was frequently the case with the male samples), the attenuation is 41%.

Table 5
Global Effect Sizes for Male and Female Samples Before and After Correction

Rind et al. (1998) sample	Point biserial (r)	Point biserial (r) corrected for base rates[a]	Biserial r[a,b]
Female	.10	.11	.14
Male	.07	.10	.13

[a] These adjustments should in practice be made at the level of individual study effect sizes. [b] Biserial correlation is corrected for dichotomization of the independent variable child sexual abuse.

The corrected point-biserial r_cs of .11 for women and .10 for men can be converted to the biserial r by dividing the correlation by the multiplicative factor of .80. This yields an overall effect of .14 for women and .13 for men (see Table 5). Biserial *r*s for the individual symptoms reported by Rind et al. (1998) in their Table 3 (e.g., anxiety, depression, paranoia, and psychotic symptoms) could be as high as .20 for both men and women.

Interpretation of effect sizes. In addition to failing to correct for effect size attenuation, Rind et al. (1998) interpreted the magnitude of the overall CSA–adjustment effect using the concept of *variance accounted for*, or r^2. They wrote, "according to Cohen's (1988) guidelines; in terms of variance accounted for, CSA accounted for less than 1% of the adjustment variance" (p. 31). However, texts on meta-analysis, including the one most relied on by Rind et al. (Rosenthal, 1984), consistently discourage the use of the r^2 statistic as an effect size estimate. For example, Rosenthal

[4] Becker (1986) noted that it may or may not be appropriate to use such adjustments, depending on the construct being investigated. However, if one is attempting to compare the size of the r values for men versus women, it would seem imperative to base such comparisons on r values unaffected by base rates. Furthermore, given that the research question examines whether CSA is harmful, we take the position that it is also appropriate to correct the point-biserial r for unequal sample sizes in the CSA and non-CSA groups. If the question of interest were to know, from a public health perspective, the effect of CSA on the entire population, r attenuations due to low base rates of CSA would accurately reflect this type of effect. However, the question of interest in this meta-analysis is a quasi-experimental one, and thus correction for unequal cells in the control (CSA) and experimental (non-CSA) group is warranted.

[5] These adjustments should in practice be made at the level of individual study effect sizes. We report them here on overall effect sizes as an illustration of how they change, and do so differentially for men and women.

[6] Throughout their article, Rind et al. (1998) also conceptualized CSA as a continuous variable by noting that there are different types and levels of abuse. For example, in comparisons to national samples, the authors discussed CSA in terms of four levels of severity (i.e., exhibitionism, fondling, oral sex, and intercourse).

wrote, "because of the probability of seriously misinterpreting its practical significance. . . we shall not use r^2 as an effect size estimate" (p. 24). It is interesting to note that in an earlier article, Rind and Tromovitch (1997) recognized this fact when they wrote, "r^2 is not currently considered to be a good measure of effect size by leading methodologists because it underestimates this metric" (p. 240).

To more accurately reflect the real world significance of an effect size, Rosenthal and Rubin (1982) developed the binomial effect size display (BESD). Although the BESD is most appropriate for situations with approximately equal size groups with similar variances, some examples presented in Rosenthal and Rubin (1982; see also Rosenthal, 1984) make it quite clear that effect sizes associated with CSA may be much more meaningful than argued by Rind et al. (1998). For example, López et al. (1995) reported the lifetime prevalence of diagnosed depression to be 19% in students who reported CSA and 12% in students who did not. After correcting for the low base rate of CSA, López et al.'s results correspond to an $r = .10$, or an r^2 of .01. One percent of explained variance might not be very impressive in theory, but in practice it represents a 7% increase of diagnosed depression in the Spanish population as a whole and could mean up to a 10% absolute increase in the prevalence of depression for an at-risk population (e.g., fraternal twins of individuals diagnosed with depression; see Bertelsen, Harvald, & Hauge, 1977). Because Rind et al.'s findings show CSA to be highly prevalent and associated with a wide range of problems, the relatively small r values they reported could represent enormous costs to society in terms of human suffering and lost productivity (see Dallam, 2001, for a review).

Examination of Moderators

Rind et al. (1998) performed multiple regression analyses on three variables (contact, gender, and consent) to determine whether they accounted for some of the variability found in sample-level effect sizes.

Contact versus noncontact CSA. Rind et al. (1998) reported that they found no significant differences in outcome for contact versus noncontact forms of abuse. This is a surprising finding which bears closer examination, as it runs contrary to the results of numerous nonclinical studies, which have reported higher rates of adjustment problems for individuals who acknowledged more severe forms of abuse (e.g., Bennett, Hughes, & Luke, 2000; Bifulco, Brown, & Alder, 1991; Cheasty, Clare, & Collins, 1998; Fergusson, Horwood, & Lynskey, 1996; Fleming, Mullen, Sibthorpe, & Bammer, 1999; Mullen, Martin, Anderson, Romans, & Herbison, 1993). After carefully reviewing the data, we found reason to believe that significant differences in outcome may have been present, but not demonstrable because of the inability of moderator analysis to compensate for attenuated and overlapping variables.

In all, 73% of the studies used by Rind et al. (1998) included noncontact forms of abuse in their definitions of CSA, and the vast majority of these did not differentiate outcomes for contact and noncontact forms of abuse in their data. Thus, rather than making direct comparisons, the moderator analysis compared studies whose definition was restricted to contact CSA with studies that included varying mixtures of both contact and noncontact forms of abuse. This, of course, diminished the precision by which Rind et al. could measure differences and, as they correctly noted (p. 33), likely underestimated the relationship. The possibility that such differences would have been demonstrated if direct comparisons had been possible is supported by the results of those studies in the primary data set that did directly compare outcomes for contact versus noncontact forms of abuse (e.g., Bergdahl, 1983; Collings, 1995; Haugaard & Emery, 1989; Peters & Range, 1995; Rew, Esparza, & Sands, 1991). The results of these studies were remarkably consistent: The adjustment of respondents reporting only noncontact forms of abuse usually did not differ significantly from that of their controls. However, when investigators restricted their definition of CSA to experiences involving physical contact, significant differences between abused students and nonabused controls emerged. The impact of definitional differences was most clearly demonstrated by Haugaard and Emery (1989), who systematically examined the effect of four levels of CSA definition on psychological and social adjustment. A *dose–response* relationship was reported, with increasing levels of severity associated with poorer adjustment.

It is also important to remember that the effect Rind et al. (1998) reported for contact versus noncontact CSA, $sr(41) = .15$, was a semipartial r, which means that it was the effect after accounting for every other variable in the equation (see p. 33 for a description of this analysis). Considering that the model included gender, contact, consent, and two interactions (Contact × Gender and Consent × Gender)—variables for which there was no doubt a problem of multicollinearity—and that the correlation suffered from many of the sources of attenuation listed in our Table 4, finding a semipartial r of .15 suggests a robust effect. However, because the correlation was not significant, Rind et al. concluded that "contact and Contact × Gender were not related [to effect sizes]" (p. 33)—implying that the null hypothesis had been proven. In our opinion, a more appropriate wording would have been to simply say that the effect sizes in question were not statistically significant, while noting the appropriate limitations of the data and their methods.

In summary, Rind et al.'s (1998) finding that physical contact during sexual abuse was not related to adjustment should be viewed cautiously. Problems with multicollinearity and attenuation, along with the lack of differentiation in the original studies and the limited ability of moderator analysis to compensate for this lack, make it very questionable whether Rind et al.'s findings apply to more serious forms of CSA.

Gender. In their abstract, Rind et al. (1998) stated, "Self-reported reactions to and *effects from* [italics added] CSA indicated that . . . men reacted much less negatively than women" (p. 22), suggesting that gender moderated adjustment. This finding, however, is not supported by their moderator analysis of gender. The global unbiased effect sizes Rind et al. reported for males and females were .07 and .10, respectively, which moderator analysis revealed "was nonsignificant" (p. 33). Moreover, as we previously demonstrated and as Rind et al. (2000a, p. 29) have admitted, correcting for base rates would have increased effect sizes for male samples from .07 to at least .10.

Consent. It appears that some of the evidence Rind et al. (1998) used to support their claim of significant gender differences in CSA adjustment was based on their finding that *consent* was an important moderator of adjustment in males. The authors later summarized their findings, stating, "We showed that for boys in nonclinical populations, willing relations are generally experienced positively or neutrally and are not associated with maladjustment" (Rind, Bauserman, & Tromovitch, 1999, p. 2185). To

evaluate the validity of this claim, it is important to carefully examine how Rind et al. conceptualized and performed their analysis of consent.

It should be noted that whereas some studies asked respondents to report only unwanted experiences, few of the other studies included in the authors' primary data set asked participants to specify whether the sexual experiences reported were wanted. Thus, Rind et al. (1998) attempted to examine a variable that was, for the most part, never directly measured. To get around this problem, the authors *assumed* that detectable portions of "willing" sex were included in any study that did not explicitly state that the student should report only unwanted experiences. Rind et al. then divided studies into two groups: unwanted and all levels of consent. Studies that required all CSA to be unwanted were placed in the unwanted group. All other studies, most of which used standard definitions of CSA based predominantly on the age difference between the child and the perpetrator, relationship to the perpetrator, or the use of force, were placed in the all-levels-of-consent group. Rind et al. then attributed all differences between the two groups to the presence or absence of willing experiences, or what they termed *consent.*

Rind, Tromovitch, and Bauserman (2000b) argued that their use of consent in this manner was "scientifically justified because . . . the same construct appeared in many of the primary studies" (p. 108). However, we were able to find only one study that directly asked students whether they had participated in the CSA experience willingly. Bergdahl (1983) studied 430 college women, 154 of whom reported some form of CSA, which was broadly defined as any type of sexual experience (including noncontact) with an adult while they were under the age of 18. Thirty-five respondents (23% of abused women) placed a check mark next to an item that indicated a willingness to participate in at least one of the sexual experiences they had reported. It is important to note that the mean age at first occurrence of willing participation was 15.8 years, which was higher than the mean age of any other group. Because of how Bergdahl defined CSA, a consensual peer relationship between a 17-year-old girl and an 18-year-old boy was categorized as CSA. Most other studies in the all-levels-of-consent group required a 5–10-year age difference before a relationship was categorized as CSA. As a result, Bergdahl provides little information on whether the other groups in the all-levels-of-consent group could be expected to contain detectable amounts of willing experiences.

When Rind et al. (1998) compared 12 samples in which CSA was defined so as to only include unwanted CSA with 35 samples of all levels of consent, the results were not significant (see Rind et al., 1998, Table 4). The authors then looked at the interaction of gender and consent (see Rind et al., 1998, Table 5). When they compared female samples, women in the group that putatively included wanted sex appeared to be less well adjusted. However, when four samples of males reporting only unwanted CSA were compared with 10 male samples categorized as including all levels of consent, the latter group had a smaller combined effect size (suggesting better adjustment) and the lower limit of the 95% confidence level was zero. Despite the fact that effect sizes for male samples were attenuated, Rind et al. took this result to mean that "consenting" males were as well adjusted as their control counterparts.

We obtained the 14 studies of male college students used to form this conclusion and replicated the moderator analysis. Our results did not support Rind et al.'s (1998) conclusions. In Table 6 we have listed the prevalence rates, definitional criteria, dependent measures, and effect sizes we obtained for the male samples analyzed. On close inspection we could find no evidence to suggest studies included in the all-levels-of-consent group included much in the way of willing experiences. For example, male students in the all-levels-of-consent group did not report higher prevalence rates, which we would expect to find if in addition to unwanted CSA, the original investigators had captured significant portions of wanted experiences. In addition, many studies in the all-levels-of-consent group defined experiences after age 12 or 13 as CSA only if they were unwanted (Finkelhor, 1979; Fishman, 1991; Greenwald, 1994) or defined experiences as CSA only if they were accomplished through the use of force (Greenwald, 1994; Predieri, 1992; Preuss, 1988). These studies appear to be unlikely sources for finding significant amounts of wanted experiences. Moreover, the moderator analysis was confounded, as effect sizes reflected not only differences in definitional criteria but also differences in the instruments being used and the outcomes being studied.[7]

When we calculated effect sizes for the objective measures (see Table 6) we found effects sizes that were slightly higher than reported by Rind et al. (1998, Appendix), even before correcting for base-rate differences.[8] Table 7 compares the global effect sizes we computed for both groups before and after correcting for base rates (to be conservative, we did not adjust for attenuation due to dichotomization of the independent variable). It is worth noting that even before correcting for attenuation due to low base rates, the confidence interval (CI) for the male all-levels-of-consent group (95% CI = .03–.19) did not include zero, suggesting that contrary to Rind et al.'s conclusion, men in this category did differ from their controls. In addition, the confidence levels for the male unwanted and all-levels-of-consent groups overlapped, suggesting that the two groups were not significantly different. It should be noted that Rind et al.'s own data (see Rind et al., 1998, Table 5) also showed overlapping CIs between effect sizes for these groups, although they disregarded this result in forming their conclusions.

[7] The problem with confounding can best be illustrated by examining Greenwald (1994), a study from the male all-levels-of-consent group with a negative effect size (Rind et al., 1998, reported an effect size of −.09), suggesting better adjustment for abused men. However, this low effect size cannot be attributed to wanted experiences, as Greenwald reported that over two thirds of the abuse reported by men was accomplished by "more than a moderate degree of force" (p. 114). In this case, the most obvious reason for the lower effect size was the outcome being studied. Greenwald studied a new and unvalidated construct called romantic and sexual self-esteem (Reed, 1988) and found that for males, scores correlated positively with increased sexual activity and negatively with a need for and sense of mutuality in a relationship. Thus, rather than measuring psychological adjustment in males, this measure appeared to serve as a proxy for increased sexual activity among abused males.

[8] For Fishman (1991), Rind et al. (1998) reported a negative effect size of −.04 (suggesting better adjustment for abused males). It should be noted that Fishman compared abused and nonabused students on only one objective measure, sexual self-esteem and dysfunction, and found abused males to be less well adjusted on every item. For instance, in his abstract, Fishman stated that males who reported CSA "were more likely to. . . identify higher levels of sexual dysfunction, and lower levels of sexual self-esteem" (p. viii). On the basis of the results he reported for this measure (see Fishman, 1991, p. 150), we calculated an uncorrected effect size of .07.

Table 6
Overview of Samples Used by Rind et al. (1998) to Determine Effect of Consent on CSA–Adjustment Relations in Male College Students

Study	Contact	Response	Definition of CSA[a]	Dependent measures	Prevalence and effect sizes[b]		
					N	CSA (%)	r_c
Sample that specified CSA had to be unwanted							
Bendixen et al., 1994	B	U	<18	Complaints (IA)	486	3	.26
Collings, 1995	B	U	<18	BSI	284	29	.29
Rau, 1994	B	U	<12 w/5+; 12–16 w/10+	PFQ, HSF–MMPI, FSSS	60	—	.10
Rew et al., 1991	B	U	<18 w/older partner	SES, JCS, GWBS	160	22	.16
"All types" (assumed by Rind et al. to contain wanted experiences)							
Finkelhor, 1979, 1984	B	M	<13 w/≥ 16; 13–16 w/10+ (relative or unwanted)	FSSS	260	7	.23
Fishman, 1991	B	M	<13 w/5+; 13–16 w/10+ or unwanted	SSD	148	21	.12
Fromuth & Burkhart, 1989 [MW]	B	A	<13 w/5+ or ≥ 16; 13–16 w/10+	SCL–90, BDI–SF, LCS, RSES	249	15	.18
Fromuth & Burkhart, 1989 [SE]	B	A	<13 w/5+ or ≥ 16; 13–16 w/10+	SCL–90, BDI–SF, LCS, RSES	324	13	.01
Greenwald, 1994	B	M	<16 w/5+; ≥ 16 w/10+; unwanted; intrafamial	RSSS	111	9	−.22
Hatfield, 1988	C	A	<14 w/5+	HSCL–Mod	213	12	.14
Predieri, 1992	B	A	<13 w/5+; 13–16 w/8+; force; authority figure	DSFI, MMPI-Sc5	62	—	.09
Preuss, 1988	B	A	<13 w/5+; 13–17 w/10+; force	MAST, bulimia (IA)	288	20	.09
Sarbo, 1985	B	M	<12 w/≥ 16; <16 w/relative ≥16; unwanted	CAQ	112	24	.12
Urquiza, 1989	C	A	<18 w/5+	SC, TSCS, MAST	88	—	.19

Note. Subjective perceptions were not included. Dashes indicate that values were not applicable. CSA = child sexual abuse; B = both contact and noncontact; U = only unwanted sex included in the definition; IA = investigator-authored item; BSI = Brief Symptom Inventory (Derogatis & Spencer, 1982); PFQ = Psychosexual Functioning Questionnaire (Schover, Friedman, Weiler, Heinman, & LoPiccolo, 1982); HSF–MMPI = Hugo Short Form of the MMPI (Hugo, 1971); FSSS = Finkelhor Sexual Self-Esteem Scale (Finkelhor, 1984); SES = Self-Efficacy Scale (Sherer et al., 1982); JCS = Jalowiec Coping Scale (Jalowiec, 1988); GWBS = General Well-Being Schedule (McDowell & Newell, 1987); M = mixed, unwanted experiences are included in the definition for older age groups; SSD = Sexual Self-Esteem and Dysfunction (IA); MW = Midwestern sample; A = all types of sexual experiences, unwanted and wanted; SCL–90 = Symptom Checklist (Derogatis, Lipman, & Covi, 1973); BDI–SF = Beck Depression Inventory—Short Form (Beck, Ward, Mendelson, Mock, & Erbaugh, 1961); LCS = Locus of Control Scale (Coleman et al., 1996); RSES = Rosenberg Self-Esteem Scale (Rosenberg, 1965); SE = Southeastern sample; RSSS = Romantic and Sexual Self-Esteem Scale (Reed, 1988); C = contact only; HSCL–Mod = Hopkins Symptom Check List—Modified (Briere & Runtz, 1985; Derogatis, Lipman, Rickels, Ulenhuth, & Covi, 1974); DSFI = Derogatis Sexual Functioning Inventory (Derogatis & Melisaratos, 1979); MMPI-Sc5 = Masculinity–Feminity Scale (Scale 5) of the Minnesota Multiphasic Personality Inventory (Hathaway & McKinley, 1967); MAST = Michigan Alcoholism Screening Test (Brady, Foulks, Childress, & Pertschuk, 1982); CAQ = Clinical Analysis Questionnaire (Cattell, 1973); SC = Symptom Checklist (Briere, 1984); TSCS = Tennessee Self-Concept Scale (Fitts, 1964).
[a] Upper age of *child* is given first; then minimum age of perpetrator is given (e.g., w/5+ = with someone at least 5 years older); last, other conditions for CSA are given. [b] *N*s are number of participants in analyses of psychological correlates of CSA, effect sizes are at the sample level, and r_cs are point-biserial effect sizes corrected for base-rate differences of CSA but not dichotomization.

We also failed to find support for Rind et al.'s (1998) conclusion that "adjustment was associated with level of consent for men, but not for women" (p. 34).[9] Because the effect sizes Rind et al. estimated for men were more attenuated than those for women, we would expect the range of effect sizes for men to be lower. After correcting for base-rate differences, effect sizes estimates for male (r_c = .11) and female (r_c = .12)[10] samples in the all-levels-of-consent group were nearly identical.

In summary, the lack of any direct measurement of "willing" experiences and the presence of confounding variables raise doubts as to what Rind et al.'s (1998) analysis of "consent" was actually measuring. Despite their claim of significant findings, the differences that Rind et al. reported for effects between male unwanted and all-levels-of-consent groups were marginal at best. Our own analysis of the data failed to reveal any significant effects for gender or what Rind et al. called "consent." Our findings also failed to support Rind et al.'s conclusion that willing sex with adults is not harmful to children.

Family Environment

Despite finding that students who reported a history of CSA were less well adjusted in 17 of the 18 types of psychological adjustment examined, Rind et al. (1998) suggested that the rela-

[9] Rind et al.'s (1998) data also showed overlapping CIs between effect sizes for males and females in both groups (see Rind et al., 1998, Table 5), undermining their own argument that there is a gender difference.

[10] The correction for females was made by using the unbiased effect size estimate provided by Rind et al. (1998, Table 5) for females in the all-types-of-consent group (r_u = .11) using the overall prevalence rate that Rind et al. reported for female samples of 27%.

Table 7
Effect of Consent on CSA–Adjustment Relations in Male College Students

Consent	k	N	r_u	r_c	95% CI for r_c	H
Unwanted	4	826	.18	.22	.13–.30	1.93
All types	10	1,525	.09	.11	.05–.19	7.92

Note. k = number of effect sizes (samples); N = the total number of participants in the k samples; r_u = unbiased effect size estimate (positive values indicate better adjustment for control participants); r_c = effect size after adjusting for base rates of CSA in each sample; CI = confidence interval; H = within-group homogeneity (chi-square based on $df = k - 1$).

tionship may be spurious because of the confounding of CSA with family dysfunction. Although there has been debate in the literature concerning whether some effects associated with CSA are artifacts of family dysfunction (see Briere & Elliot, 1993; M. R. Nash, Hulsey, Sexton, Harralson, & Lambert, 1993), we would argue that Rind et al.'s conclusions in this regard are stronger than their data warrant.

First, the stronger relationship between environment and CSA symptoms may merely be an artifact of the way CSA was treated statistically. In the vast majority of the original research Rind et al. (1998) analyzed, CSA was treated as a dichotomous variable. At the same time, family environment was usually measured by well-validated measures such as the Family Environment Scale, which has multiple continuous scales. By comparing the predictive ability of one dichotomous variable (CSA) with multiple continuous variables, the deck was stacked against sexual abuse before any statistical analysis occurred.[11]

Second, the strong relationship between family dysfunction and distress in college samples using broad definitions of CSA is not surprising. As Briere (1988) noted, "nonclinical samples (especially involving students) may not include many severe cases of sexual or physical victimization . . . and thus any 'abuse effects' may be trivial in comparison to the variance accounted for by the 'control' variables" (pp. 83–84). This contention is supported by the results of several studies included in the current meta-analysis. For instance, Collings (1995) reported that outcomes associated with noncontact forms of CSA were not significant after controlling for the influence of dysfunctional parenting behaviors. However, CSA involving physical contact was associated with elevated scores on all subscales of the Brief Symptom Inventory even after controlling for family dysfunction. Similar results were reported by Edwards and Alexander (1992), who found that "psychological distress was significantly predicted by sexual abuse severity and marginally predicted by parental conflict" (p. 315).

Finally, the fact that both family dysfunction and abuse exposure are related to subsequent distress does not allow the inference that the shared variance is due to family dysfunction rather than abuse. The data examined by Rind et al. (1998) came predominantly from the partialization of family environment based on either analysis of covariance (ANCOVA) or hierarchical regression. For a covariate to be effective in controlling for an extraneous variable, the covariate must be linearly related to the dependent variable and be unaffected by the treatment or independent variables (Hinkle, Wiersma, & Jurs, 1988; Porter & Raudenbush, 1987). Given that the relevant independent variable in the studies reviewed by Rind et al. was CSA, to meet this condition one would have to assume that sexual abuse was uncorrelated with family dysfunction. This is clearly not the case, as families that abuse their children are by by definition dysfunctional, and children are often targeted by sex offenders because of parental noninvolvement (Budin & Johnson, 1989; Christiansen & Blake, 1990). When a covariate is significantly correlated with both the dependent variable(s) and the independent variables, the results of an ANCOVA may be misleading (Pedhazur, 1982), as the analysis will partial out not just variance attributable to the covariate but also variance shared by the covariate and the independent variable. That is, while partialing out the effects of family environment one is also removing the effects of CSA (see Briere, 1988; Briere, 1992; Briere & Elliot, 1993).[12] Furthermore, when there is a high degree of multicollinearity, standard errors will be larger, parameter estimates will be unstable, and p values will be increased (e.g., Wampold & Freund, 1987).

Although Rind et al. (1998) briefly addressed some of these issues, they suggested that these concerns "do not appear to be problematic in the current review" (p. 43). We disagree. Their statistical methods could not compensate for the limitations of retrospective, quasi-experimental data, and their conclusions are at variance with those of numerous largescale nonclinical studies whose methods are more appropriate for disentangling the effects of CSA from those of family dysfunction (see Table 8). Moreover, some investigators have found that family dysfunction and abuse each exert a negative influence independent of the other and that these effects appear to be additive (e.g., Felitti et al., 1998; Yama, Tovey, Fogas, & Teegarden, 1992). Finally, we would submit that much of the etiological picture of the complexities of human functioning is missed when researchers attempt to reduce something as broad as psychological maladjustment to univariate causation.

Qualitative Analysis

In the qualitative section of their article, Rind et al. (1998) examined college students' retrospective reports of their initial emotional reactions to CSA, how long it took them to recover from the experience, and whether they continued to be affected.

Length of time to recover. On the basis of the data summarized in their Table 8, Rind et al. (1998) concluded that "when negative effects occur, they are often temporary" (p. 37). This conclusion, however, is severely limited by the fact that only three studies examined this variable, and these studies included very little in the way of serious abuse. In fact, the largest of these studies, Landis (1956), did not even study CSA. Instead, Landis examined college students' encounters with "a sexual deviate" (p. 93). Because no age limits were placed on when the experience occurred, a signif-

[11] Rind et al. (1998) attempted to counter this criticism (regarding artificial dichotomization) by noting that a few studies have used continuous scales for CSA and found that CSA did not explain adjustment variance above and beyond that explained by various family environment factors in a hierarchical regression. We find this argument unconvincing given that no reliability or validity data were presented for these scales and that the studies mentioned were retrospective in nature and thus subject to the partialing effect previously noted.

[12] Rind et al. (1998) attempted to counter this type of criticism by pointing out (p. 41) that in one study physical abuse predicted adjustment above and beyond family environment even though CSA did not.

Table 8
Well-Designed Nonclinical Studies That Controlled for Family Dysfunction

Researchers	Method	Controls	Results
Boney-McCoy & Finkelhor (1995)	Random, nationally representative probablity sample of 2,000 youths age 10–16 years	Social class, race, and quality of parent–child relationship	Reports of CSA were associated with increased levels of psychological and behavioral symptoms, including PTSD symptoms and school difficulties. In addition, abused boys reported significantly more sadness than other children.
Boney-McCoy & Finkelhor (1996)	Longitudinal: Reinterviewed sample from 1995 study 15 months later	Prior symptoms and quality of parent–child relationship	Reports of sexual abuse during a 15-month interim were associated with PTSD-related symptoms and depression not present prior to the assault.
Dinwiddie et al. (2000)	Cotwin: Examined twins discordant for CSA drawn from 5,995 Australian male and female twins	Family dysfunction and genetic predisposition	The twin reporting CSA consistently displayed more psychopathology than the nonabused cotwin. However, only a single outcome reached statistical significance; the association between CSA and suicidal ideation in males.
Fergusson et al. (1996)	Prospective study of a birth cohort of 1,019 male and female youths	Family, social, and individual factors (e.g., family functioning, intelligence, adolescent life events, school achievement, etc.)	Reports of CSA were associated with higher rates of major depression, anxiety disorder, conduct disorder, substance use disorder, and suicidal behavior, with those reporting the most severe CSA involving intercourse having the highest risk of disorder.
Johnson, Cohen, Brown, Smailes, & Bernstein (1999)	Prospective study of a representative community sample of 639 youths	Age, parental education, parental psychiatric disorders, and other types of childhood maltreatment	Reports of CSA were associated with higher incidence of a variety of personality disorders during early adulthood including borderline, histrionic, and depressive disorders, along with higher total numbers of disorders.
Fleming et al. (1999)	Subsample of 710 women selected from a larger study involving 3,958 women randomly selected from electoral rolls in Australia	Family dysfunction and socioeconomic background	Reports of CSA were associated with higher reports of experiencing domestic violence, rape, sexual problems, mental health problems, low self-esteem, and problems with intimate relationships. More severe CSA involving intercourse was associated with the highest risk of disorder.
Kendler et al. (2000)	Cotwin: Examined twins discordant for CSA drawn from a sample of 1,411 adult female twins	Family dysfunction, parental psychopathology, genetic predisposition, and reporting bias	The twin reporting CSA was consistently at higher risk for lifetime psychiatric and substance use disorders compared with the nonabused cotwin, with odds ratios generally increasing with the severity of the abuse.
Mullen et al. (1993)	Stratified, random community sample of 1,376 adult women in New Zealand	Family dysfunction and socioeconomic background	Reports of CSA were associated with greater levels of psychopathology on a range of measures, along with higher rates of substance abuse and suicidal behavior. A dose–response relationship was found, with those suffering the most severe forms of abuse having the greatest level of psychopathology.
Stein, Golding, Siegel, Burnam, & Sorenson (1988)	Random community sample of 3,132 male and female adults. Included various ages and ethnic and socioeconomic backgrounds	Gender, ethnicity, age, education, and abuse during adulthood	Over 75% of respondents with CSA experienced symptoms of distress, with anxiety, anger, guilt, and depression the most commonly reported. Significant associations were also found between reporting CSA and meeting diagnostic criteria for at least one lifetime psychiatric disorder, especially substance abuse disorders, major depression, phobia, panic disorder, and antisocial personality.

Note. CSA = child sexual abuse; PTSD = posttraumatic stress disorder.

icant percentage of the experiences occurred after age 17. Consensual experiences were also included, and the vast majority (83%) of the experiences reported by male participants were homosexual advances during older adolescence or adulthood, which usually occurred in public places and were easily rebuffed. Most of the experiences reported by the female participants involved exhibition. The majority of these women reported they immediately left the scene. None of the men reported being raped, and less than 2% of the women reported attempted or committed rape. Although the students had an overwhelming negative perception of the experience, as might be expected, few reported that the episode had a long-term negative effect on their life. Similarly, West and Woodhouse (1993) noted that most of the incidents recalled by their male sample were either relatively trivial homosexual approaches that were soon rejected or minor indecencies, that boys looked on as unimportant. The most relevant of the three studies, C. Nash and West (1985), used a broad definition of CSA and reported a prevalence rate of 54%. Of note, 22% of students

who acknowledged abuse reported that they were still being adversely affected by the experience.

It is obvious that these data are unsuitable to answer the question of how long it takes a child to recover from CSA. The most scientifically valid method of determining the length of time to recover from CSA is to follow abused children prospectively. Longitudinal studies of sexually abused children contradict Rind et al.'s (1998) findings, as they show that the pattern of recovery is different for different symptoms and often unpredictable (Calam, Horne, Glasgow, & Cox, 1998; McLeer et al., 1992; Tebbutt, Swanston, Oates, & O'Toole, 1997). For instance, Tebbutt et al. (1997) reassessed 68 children 5 years after their abuse was discovered. There were no significant changes in depression, self-esteem, or behavior over 5 years. At follow-up, 43% of the children were sad or depressed, 43% had low self-esteem, and 46% had behavioral dysfunction. Whereas some children improved, a nearly equal number deteriorated, with no clear pattern of change. Moreover, family variables did not predict outcome.

Effects on current life. On the basis of self-reported effects from seven studies, Rind et al. (1998) concluded that "lasting negative effects are not prevalent" (p. 37) among college students who report CSA. Our review of the original studies revealed that this conclusion could be considered valid only for those studies looking at milder forms of abuse or, as in the case of Landis (1956), experiences that do not fulfill normal criteria for CSA. Very different results were reported by studies that included more serious CSA. For instance, Fishman (1991), reported that "47% [of male respondents] stated that this experience has had a negative effect on their life, and 23% reported that the CSE [child sexual experience] had a negative effect on their current sexual life" (p. 162; Rind et al. misreported these results as 27% and 13%, respectively). Fischer (1991) reported that the majority of students reporting CSA (93% of females and 79% of males), reported that the experience had caused or was continuing to cause them stress.

Initial emotional reactions. Rind et al. (1998) reported that "two-thirds of SA men and more than one-fourth of SA women reported neutral or positive reactions," which they suggested is "inconsistent with the assumption of pervasive and intense harm" (p. 44). On examination, we found there were numerous problems with how Rind et al. reported and interpreted the results of the original studies. For example, in three instances we found that Rind et al. misreported the participants' reactions to CSA (Fishman, 1991;[13] Brubaker, 1991, 1994[14]). In each case, the misreporting inflated positive reactions and simultaneously diminished negative ones.

Although males were more likely than females to ascribe neutral or positive initial feelings to the experience, our review of the original data revealed a number of mediating variables that should be considered when interpreting these findings. For example, Rind et al. (1998) reported only males' initial reactions to the abuse. Studies that compared initial reactions with current ones found that males' initial reactions tended to become more negative over time. Therefore, whereas almost 38% of males surveyed by Urquiza (1989) reported a positive or mostly positive initial reaction to CSA, only 15% indicated that they still felt positively about the experience as an adult. Another factor that mediated reactions was the type of abuse experienced. As Rind et al. noted, "college men and women. . . tended to have different experiences; SA women experienced close family CSA more than twice as often as SA men and experienced force about twice as often" (p. 43). In general, males reacted most negatively to encounters involving other males, force, or close relatives (Finkelhor, 1979; Fishman, 1991; Fromuth & Burkhart, 1989; O'Neill, 1991; Risin & Koss, 1987; Urquiza, 1989). Positive reactions were most apparent when the experience was with an unrelated female, someone who was closer in age to the boy, and when the boy felt that he had initiated the contact (Condy, Templer, Brown, & Veaco, 1987). In other words, the experiences most likely to be perceived positively by males were those that most closely resemble the types of sexual contacts that a boy might have with a peer.

Differences between subjective and objective measures. Studies that compared qualitative perceptions with the results of objective measures demonstrated that self-reported ratings of adjustment may not provide the best criterion of harm in abused men. For example, three studies, Fishman (1991), Fromuth and Burkhart (1989), and Urquiza (1989), compared men's overall subjective ratings of current adjustment with scores on objective measures and found that abused men tended to present themselves as doing much better than their objective scores indicated. For instance, Fishman (1991) reported that, compared with their nonabused peers, men acknowledging a history of CSA rated themselves higher on a subjective assessment of overall sexual adjustment and current adjustment. However, an objective measure of sexual dysfunction showed these same men to be less well adjusted in every area assessed. Moreover, nearly three times as many of the abused men reported having been treated for emotional problems as their nonabused counterparts (29% vs. 10%, respectively). Urquiza (1989) noted similar findings. Most men in his study reported neutral reactions to their abuse; however, compared with their nonabused peers, over two times as many abused men reported using illegal drugs, three times as many had sought therapy for emotional problems, and five times as many had attempted suicide.

In summary, although men tended to report less negative perceptions of CSA and attributed less harm to the experience, objective measures did not support their appraisals. A more correct statement of Rind et al. (1998) findings is that men often claimed to be unaffected by CSA but simultaneously demonstrated negative effects similar to those displayed by their female counterparts. In this respect, the findings of the current meta-analysis are consistent with those of numerous other studies and reviews, which have reported that men frequently deny that their abuse had any serious effect on them yet at the same time display poorer adjustment than their nonabused peers (e.g., Briggs & Hawkins, 1996; Fondacaro, Holt, & Powell, 1999; Holmes & Slap, 1998; Myers, 1989; Varia, Abidin, & Dass, 1996; Watkins & Bentovim, 1992).

[13] For Fishman (1991), Rind et al. (1998, Table 7) reported that 27% reported positive immediate reactions, 43% neutral, and 30% negative. However, Fishman reported "53% of these men felt the CSE [child sexual experience; was negative at the time]. . . 12% positive and 35% neutral" (p. 162).

[14] In Brubaker (1991 and 1994), 83% and 92% of the participants reported feeling initially upset, with the largest category reporting having felt extremely upset (35% and 40%, respectively). These figures contrast greatly with those reported by Rind et al. (1998, Table 7): that 40% and 27% of the participants felt neutral or positive at the time of the abuse.

Table 9
Data Supporting Rind et al.'s (1998) Conclusions

Conclusion	Results of meta-analysis	Results of qualitative review
"The negative potential of CSA for most individuals who have experienced it has been overstated." (p. 42)	Equivocal. Despite an overly inclusive definition, a healthy sample, and multiple sources of attenuation, participants who reported abuse were consistently found to be less well adjusted in 17 of the 18 types of psychological adjustment examined.	Not supported. Despite the preponderance of mild experiences, a significant percentage of both men and women indicated that the abuse continued to exert a negative effect on their life.
"The college data were completely consistent with data from national samples." (p. 22)	Not supported. Prevalence, severity, and effect sizes differed across the national surveys and often differed from college data.	Not examined.
Harm associated with CSA is likely due to negative family factors.	Equivocal. Family environment and CSA are too highly correlated to accurately disentangle in retrospective quasi-experimental designs.	Not examined.
Negative effects associated with CSA "are often only temporary." (p. 37)	Not supported. Adults who reported CSA were less well adjusted than their peers despite the fact that in most cases the abuse had occurred many years previously.	Not supported when inappropriate data from non-CSA experiences are disregarded.
"The relation between CSA and adjustment problems was generally stronger for women than men." (p. 42)	Not supported. Moderator analysis did not demonstrate a gender difference. After correcting for base-rate differences, global effect sizes for men and women were nearly identical.	Equivocal. Men were more likely to report neutral or positive reactions; however, they tended to experience less serious abuse. Men's subjective perceptions often did not correlate with objective outcomes.
"Adjustment was associated with level of consent for men, but not for women." (p. 34)	Not supported. After correcting for base-rate differences, effect size estimates for male and female samples in the all-levels-of-consent group were nearly identical.	Not examined.
"Willing" participants displayed "normal adjustment." (p. 46)	Not supported. "Willing" experiences were never directly measured. The 95% confidence interval did not include zero in the all-levels-of-consent groups.	Not examined.

Note. CSA = child sexual abuse.

Discussion and Conclusions

Although we agree with Rind et al. (1998) that CSA does not inevitably lead to intense and pervasive harm in all individuals, our conclusions, which are summarized in Table 9, differ from those of Rind et al. in almost every other area. It is also important to note that although the results of Rind et al.'s meta-analysis support those of previous reviews that show that extreme long-term effects from CSA are not inevitable (Beitchman et al., 1991, 1992; Browne & Finkelhor, 1986; Kendall-Tackett et al., 1993), their findings also demonstrate a significant association between acknowledging a history of such abuse and an increased vulnerability to a wide range of mental health and social problems in adult life. The fact that many of these associations were small should not be considered surprising given that the use of correlations coupled with attenuation problems served to minimize the appearance of meaningful effects. In addition, it should be remembered that Rind et al. studied a healthy sample and that the meta-analysis tapped a very broad range of sexual experiences, many of which involved no physical contact. It is well recognized that heterogeneity in abuse severity can distort estimates of the consequences of CSA, as the lack of measurable consequences for the majority who experienced milder forms of abuse are likely to obscure the significant consequences experienced by the smaller number of people who experienced more severe forms of CSA (Haugaard, 2000).

An interesting aspect of the data presented by Rind et al. (1998) is the fact that abused students were less well adjusted than controls in 17 of 18 symptoms examined, with a high degree of consistency of the individual effect size magnitudes (most uncorrected $rs = .11 \pm .02$) across different dependent variables (e.g., anxiety, depression, psychotic symptoms; see Rind et al., 1998, Table 3). Although these results are similar to those of other studies indicating a global and diffuse impact of trauma on children (McCloskey & Walker, 2000), Rind et al. averaged the 18 different symptoms together and treated them as if they measured equal portions of a single construct (i.e., maladjustment). This is necessarily an underestimate unless there is perfect overlap of symptoms.

Although the literature does show substantial comorbidity of mental disorders (Gotlib, 1984), on the order of 50% for anxiety and depression (Regier, Rae, Narrow, Kaelber, & Schatzberg, 1998) and 14% for anxiety and psychotic disorders (Cassano, Pini, Saettoni, & Dell'Osso, 1999), most studies conclude that the overlap is only partial at best (Sandanger et al., 1998). Moreover, syndromes often occur at different times and at different periods in the life span, and comorbidity is lower among those with less severe symptoms (Schatzberg et al., 1990). Thus, in the young and relatively high functioning samples reviewed in the Rind et al. (1998) meta-analysis, there should be substantial independence of symptom clusters, on the order of at least 50%. Thus, a practical assessment of the clinical impact of CSA on mental health would be more than just the average of the effects of CSA on the different outcomes.

To the extent that symptoms such as alcohol problems, sexual adjustment, dissociation, and eating disorders are distinct, effects would be additive in their effect on overall adjustment. For example, clinical studies have shown that comorbid depression plus anxiety results in more severe symptoms than either anxiety or depression alone (Angst, 1997). Epidemiologic surveys have also

shown that even when symptoms are not sufficiently severe to fulfill criteria for a specific diagnosis of either depressive or anxiety disorder, patients presenting with both anxiety and depressive symptoms often display significant levels of functional impairment, have a high use of nonpsychiatric medical care, have long-lasting symptoms, and are at risk for more severe psychiatric disorders (Boulenger, Fournier, Rosales, & Lavallee, 1997). Overall, the significant comorbidity suggested by Rind et al.'s data suggests that some abused students may experience significant levels of functional impairment, especially if the majority of the morbidity originates in the smaller number of students who experienced the most severe forms of abuse.

Actually, when placed in the proper context, the results of the current meta-analysis are quite sobering. Despite the use of a healthy sample, the retrospective nature of the data, an overinclusive and variable definition of CSA, and multiple sources of attenuation, the meta-analysis yielded effect sizes that could be interpreted as quite meaningful, especially when one considers that small r values can reflect serious effects when a condition is as prevalent as CSA. As a comparison, Ondersma, Chaffin and Berliner (1999) meta-analyzed data from 14 studies on smoking and the development of lung cancer and found an effect size of $r = .17$.[15] Thus, the effect sizes Rind et al. (1998) reported for the correlation between CSA and anxiety, depression, paranoia, and psychotic symptoms (.13 for each) are only slightly smaller than the effect of cigarette smoking on lung cancer in the general population. Based on a correlation of .17, many might be tempted to argue that smoking only plays a negligible role in the development of lung cancer. Those who did so would, however, greatly underestimate the health risks associated with higher levels of smoking. The risk of lung cancer varies with amount smoked, and for individuals who smoke two or more packs a day, the risk exceeds 20 times that of nonsmokers (Blot & Fraumeni, 1996).

When interpreting the results of both our own and Rind et al.'s (1998) various analyses, several important caveats are in order. First, it is likely that those most severely affected by CSA will be underrepresented in college samples. Therefore, it is not appropriate to generalize from studies of college students to sexual abuse victims in other populations, especially those encountered in the legal or clinical settings. Second, a prominent epidemiologist, Sander Greenland (1994), has suggested that the worst abuse of meta-analysis stems from the temptation to produce a single estimate of effect from disparate study results and then to treat this estimate as a definitive literature synthesis. We reiterate this concern. Finally, a single estimate of adjustment obtained by averaging data on a wide range of exposure levels to CSA cannot be used to draw conclusions about the risk of problems and distress in an individual victim. Accordingly, attempts to use Rind et al.'s study to argue that an individual has not been harmed by sexual abuse constitute a serious misapplication of its findings.

[15] Although Ondersma et al. (1999) reported that the effect size was .12, an erratum was later published saying the actual effect size was .17. (Erratum, 1999).

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