

A Meta-Analytic Examination of Assumed Properties of Child Sexual Abuse Using College Samples

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Many lay persons and professionals believe that child sexual abuse (CSA) causes intense harm, regardless of gender, pervasively in the general population. The authors examined this belief by reviewing 59 studies based on college samples. Meta-analyses revealed that students with CSA were, on average, slightly less well adjusted than controls. However, this poorer adjustment could not be attributed to CSA because family environment (FE) was consistently confounded with CSA, FE explained considerably more adjustment variance than CSA, and CSA-adjustment relations generally became nonsignificant when studies controlled for FE. Self-reported reactions to and effects from CSA indicated that negative effects were neither pervasive nor typically intense, and that men reacted much less negatively than women. The college data were completely consistent with data from national samples. Basic beliefs about CSA in the general population were not supported.

Child sexual abuse (CSA) has received considerable attention since the late 1970s from mental health care professionals, legislative, judicial, and law enforcement personnel, the media, and the lay public (Rind & Tromovitch, 1997). Much of this attention has focused on possible effects of CSA on psychological adjustment, as is shown in the professional literature and popular press (Pope & Hudson, 1995) and in the information and entertainment media (Esman, 1994; Kutchinsky, 1992; West & Woodhouse, 1993). The media have frequently presented lurid CSA cases combined with high prevalence estimates, creating the image that CSA produces intensely negative effects for all of its victims (Esman, 1994; Kutchinsky, 1992; West & Woodhouse, 1993). Many publications in the popular press and the professional literature have similarly portrayed CSA as a "special destroyer of adult mental health" (Seligman, 1994, p. 232), and some have attempted to explain much or all of adult psychopathology as a consequence of CSA (Esman, 1994; Nash, Hulsey, Sexton, Harralson, & Lambert, 1993). Examples in the professional literature include McMillen, Zuravin, and Rideout (1995, p. 1037), who commented that "child sexual abuse is a traumatic event for which there may be few peers," and Rodriguez, Ryan, Rowen, and Foy (1996), who combined estimates of national prevalence rates of CSA with selected examples of empirical research to argue that posttraumatic stress disorder is

a common sequel of CSA in the general population. 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. The purpose of the current review was to examine these implied basic properties. Our 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?

An important first step is to discuss terminology. The term *child sexual abuse* has been used in the psychological literature to describe virtually all sexual interactions between children or adolescents and significantly older persons, as well as between same-age children or adolescents when coercion is involved. The indiscriminate use of this term and related terms such as *victim* and *perpetrator* has been criticized because of concerns about scientific validity (e.g., Kilpatrick, 1987; Nelson, 1989; Okami, 1990; Rind & Bauserman, 1993). Kilpatrick argued that researchers have often failed to distinguish between "abuse" as harm done to a child or adolescent and "abuse" as a violation of social norms, which is problematic because it cannot be assumed that violations of social norms lead to harm. Similarly, Money (1979) observed that our society has tended to equate "wrongfulness" with harmfulness in sexual matters, but harmfulness cannot be inferred from wrongfulness. Nelson argued that the indiscriminate use of terms suggesting force, coercion, and harm reflects and maintains the belief that these interactions are always harmful, thereby threatening an objective appraisal of them. Rind and Bauserman demonstrated experimentally that appraisals of nonnegative sexual interactions between adults and

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We thank Ralph Rosnow for his meta-analytic advice and comments on an earlier draft and Steve Wexler for his helpful comments.

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adolescents described in scientific reports can be biased by the use of negatively loaded terms such as CSA.

Problems of scientific validity of the term CSA are perhaps most apparent when contrasting cases such as the repeated rape of a 5-year-old girl by her father and the willing sexual involvement of a mature 15-year-old adolescent boy with an unrelated adult. Although the former case represents a clear violation of the person with implications for serious harm, the latter may represent only a violation of social norms with no implication for personal harm (Bauserman & Rind, 1997). By combining events likely to produce harm with those that are not into a unitary category of CSA, valid understanding of the pathogenicity of CSA is threatened (Okami, 1994). The tendency by researchers to label cases such as the latter as abuse reflects the slippage of legal and moral constructs into scientific definitions (Okami, 1990, 1994). Basing scientific classifications of sexual behavior on legal and moral criteria was pervasive a half century ago (Kinsey, Pomeroy, & Martin, 1948); more recently, this practice has been confined to a much smaller set of sexual behaviors, particularly those labeled CSA.

With these caveats in mind regarding the scientific shortcomings of the term CSA, we have nevertheless retained it for use in the current article because of its pervasive use in the scientific literature and because many researchers as well as lay persons view all types of sociolegally defined CSA as harmful. On the basis of the terminology used in studies reviewed in the current article, CSA is generally defined as a sexual interaction involving either physical contact or no contact (e.g., exhibitionism) between either a child or adolescent and someone significantly older, or between two peers who are children or adolescents when coercion is used.

Previous Literature Reviews

Numerous literature reviews have appeared over the last 15 years that have attempted to synthesize the growing body of empirical investigations of CSA effects and correlates (e.g., Bauserman & Rind, 1997; Beitchman, Zucker, Hood, Da-Costa, & Akman, 1991; Beitchman et al., 1992; Black & DeBlassie, 1993; Briere & Elliot, 1994; Briere & Runtz, 1993; Browne & Finkelhor, 1986; Constantine, 1981; Glod, 1993; Jumper, 1995; Kendall-Tackett, Williams, & Finkelhor, 1993; Kilpatrick, 1987; Mendel, 1995; Neumann, Houskamp, Pollock, & Briere, 1996; Rind & Tromovitch, 1997; Urquiza & Capra, 1990; Watkins & Bentovim, 1992). These reviews have not been unanimous in their conclusions. Below, we examine their conclusions regarding the four commonly assumed properties of CSA discussed previously. First we examine the qualitative literature reviews, then the fewer and more recent quantitative (i.e., meta-analytic) reviews.

Qualitative Literature Reviews

Causality. Some qualitative reviewers have been cautious regarding the issue of causality (e.g., Bauserman & Rind, 1997; Beitchman et al., 1991; Beitchman et al., 1992; Constantine, 1981; Kilpatrick, 1987), arguing that the reliable confounding of family environment problems with CSA prevents definitive conclusions regarding the causal role of CSA in producing mal-

adjustment. Other reviewers, although recognizing limitations of correlational data, have nevertheless argued that causality is the likely state of affairs (e.g., Briere & Runtz, 1993; Glod, 1993; Urquiza & Capra, 1990). Some reviewers have strongly implied that CSA causes maladjustment by consistent use of phrases that imply causation (e.g., "effects of CSA," "impact of CSA") and by not addressing alternative explanations (e.g., third variables, such as family environment) that could account for the CSA-maladjustment link (e.g., Black & DeBlassie, 1993; Briere & Elliot, 1994; Kendall-Tackett et al., 1993; Mendel, 1995; Watkins & Bentovim, 1992).

Pervasiveness. Some reviewers have concluded that CSA outcomes are variable, rather than consistently negative (e.g., Bauserman & Rind, 1997; Beitchman et al., 1991; Beitchman et al., 1992; Browne & Finkelhor, 1986; Constantine, 1981; Kilpatrick, 1987). Constantine concluded that there is no inevitable outcome or set of reactions and that responses to CSA are mediated by nonsexual factors. Beitchman et al. (1991) argued that the prevalence of negative outcomes may be overestimated because of overreliance on clinical samples. Browne and Finkelhor noted that only a minority of both sexually abused (SA) children seen by clinicians and adults with a history of CSA show serious disturbance or psychopathology. Other reviewers, however, have implied in several different ways that CSA effects or correlates are prevalent among persons with a history of CSA. First, some reviewers have claimed to have written "comprehensive" reviews of the literature or summaries of "what is currently known" (e.g., Briere & Elliott, 1994; Briere & Runtz, 1993; Glod, 1993; Urquiza & Capra, 1990; Watkins & Bentovim, 1992); their conclusion that CSA is associated with numerous symptoms then implies that negative correlates are prevalent. Second, some reviewers have argued that studies showing a large percentage of asymptomatic persons with a history of CSA can be explained by factors such as insensitive measures or insufficient time for symptoms to have developed (e.g., Briere & Elliot, 1994; Kendall-Tackett et al., 1993). This argument implies that negative effects are prevalent, even if not yet observed in many cases. Third, some reviewers have not discussed limitations on generalizability from their sample of (usually clinical) studies to other CSA populations (e.g., Black & DeBlassie, 1993; Kendall-Tackett et al., 1993; Mendel, 1995), again implying that findings of negative correlates apply to the entire population of persons with CSA experiences.

Intensity. Some reviewers have concluded that the intensity of CSA outcomes varies, rather than usually being intensely negative (e.g., Bauserman & Rind, 1997; Beitchman et al., 1991; Beitchman et al., 1992; Browne & Finkelhor, 1986; Constantine, 1981; Kilpatrick, 1987). Browne and Finkelhor noted that SA persons in community samples tend to be either normal or only slightly impaired on psychological measures. Constantine and Kilpatrick found that negative outcomes were often absent in SA persons in nonclinical samples. Other reviewers, however, have implied that negative psychological effects are frequently intense by describing the "extreme psychic pain" (Briere & Runtz, 1993, p. 320) or the "pronounced deleterious effects" (Mendel, 1995, p. 101) that CSA is assumed to produce. Some reviewers have further implied the intensity of CSA effects or correlates by presenting long lists of severe disorders (e.g., post-traumatic stress, self-mutilation) associated with CSA (e.g.,

Black & DeBlasie, 1993; Briere & Elliot, 1994; Briere & Runtz, 1993; Glod, 1993; Kendall-Tackett et al., 1993; Mendel, 1995; Urquiza & Capra, 1990; Watkins & Bentovim, 1992).

Gender equivalence. Several reviewers have argued that the data are insufficient to address the issue of gender differences in outcomes (e.g., Beitchman et al., 1991; Beitchman et al., 1992; Browne & Finkelhor, 1986). Constantine (1981) concluded that girls react more negatively than boys, attributing this difference to differences between girls' and boys' CSA experiences. 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 & DeBlasie, 1993; Briere & Runtz, 1993; Mendel, 1995; Urquiza & Capra, 1990; Watkins & Bentovim, 1992). Black and DeBlasie 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).

Limitations of Qualitative Literature Reviews

The qualitative literature reviews present a mixed view of causality, pervasiveness, intensity, and gender equivalence. This inconsistency suggests the need for additional work in synthesizing the literature. Two other considerations also indicate such a need: sampling biases in many of the qualitative reviews, and the vulnerability of qualitative reviews to subjectivity and imprecision.

Sampling biases. Qualitative literature reviews have been primarily based on clinical or legal samples, which cannot be assumed to be representative of the population of persons with a history of CSA (Bauserman & Rind, 1997; Okami, 1991; Rind, 1995). Some reviews were based exclusively or almost exclusively on clinical and legal samples (e.g., Beitchman et al., 1991; Black & DeBlasie, 1993; Glod, 1993; Kendall-Tackett et al., 1993; Mendel, 1995; Watkins & Bentovim, 1992). Others were based on a majority of clinical and legal samples but included a sizable minority of nonclinical and nonlegal samples (e.g., Beitchman et al., 1992; Briere & Elliott, 1994; Briere & Runtz, 1993; Browne & Finkelhor, 1986; Constantine, 1981; Kilpatrick, 1987; Urquiza & Capra, 1990). Only one of the qualitative reviews cited previously (Bauserman & Rind, 1997) included a majority of nonclinical and nonlegal samples.

Drawing conclusions from clinical and legal samples is problematic not only because these samples cannot be assumed to be representative of the general population, but also because data coming from these samples are vulnerable to several biases that threaten their validity (Pope & Hudson, 1995; Rind & Tromovitch, 1997). Okami (1991) studied adults who had experienced CSA as negative, neutral, or positive. Negative responders included both clinical and nonclinical subjects. Clinical negative responders showed substantially more pronounced adjustment

problems than nonclinical negative responders. Okami argued that clinical participants with negative CSA experiences constitute the negative extreme of CSA outcomes. Pope and Hudson argued that reliance on clinical samples is problematic for several reasons. One problem is information bias, in which clinical patients, in a search for the causes of their problems (termed *effort after meaning*), are more likely than nonclinical participants to recall events that can be classified as CSA, thus inflating the CSA-maladjustment relationship. Another potential bias is investigator expectancies (cf. Rosenthal, 1977), in which clinical researchers who believe that CSA is a likely cause of their patients' difficulties may transmit this expectancy to patients, thereby increasing confirming responses. Finally, Pope and Hudson argued that causality cannot be inferred from clinical samples because CSA and family disruption are highly confounded in this population (Beitchman et al., 1991; Ney, Fung, & Wickert, 1994). Legal samples are also likely to contain the more serious cases, limiting their generalizability.

Subjectivity and imprecision. Qualitative reviews are entirely narrative and therefore susceptible to reviewers' own subjective interpretations (Jumper, 1995). Reviewers who are convinced that CSA is a major cause of adult psychopathology may fall prey to confirmation bias by noting and describing study findings indicating harmful effects but ignoring or paying less attention to findings indicating nonnegative outcomes. For example, Mendel (1995) focused on results from Fromuth and Burkhardt's (1989) midwestern sample of males to argue that boys are harmed by their CSA experiences but paid little attention to the southeastern sample of males reported in the same article, for whom all CSA-adjustment correlates were nonsignificant. In a quantitative review, the latter sample would typically have received more weight because it had 30% more participants than the former. Even when study results generally indicate statistically significant differences in adjustment between CSA and control participants, summarizing this information alone is inadequate (Rosenthal & Rosnow, 1991). The sizes of these differences (i.e., effect sizes) are also important; these effect sizes can be used to assess the intensity of CSA effects or correlates (Rind & Tromovitch, 1997). Only quantitative (i.e., meta-analytic) reviews can provide this important information.

Quantitative Literature Reviews

Three recent quantitative literature reviews (Jumper, 1995; Neumann et al., 1996; Rind & Tromovitch, 1997) represent a significant advance in assessing CSA-adjustment relations because they all (a) included a sizable proportion of nonclinical and nonlegal samples and (b) avoided subjectivity and imprecision by using meta-analysis. Meta-analysis is a statistical technique in which statistics from a set of studies are converted to a common metric (e.g., standard normal deviate z s, Cohen's d s, Pearson's r s), which are then combined into one overall statistic that can be used to (a) infer whether one variable (e.g., CSA) is significantly associated with another (e.g., adjustment) and (b) estimate the strength of this association (Rind & Tromovitch, 1997). Common metrics such as d and r are referred to as effect sizes and can be interpreted as assessing the size of the difference of some attribute between two groups or the magnitude of association between two variables. As a guideline,

Cohen (1988) has suggested that small, medium, and large effect sizes correspond, respectively, to d s of .20, .50, and .80, and to r s of .10, .30, and .50. Thus, these reviews were well suited to examining not only whether control and SA respondents differ in adjustment, but also to what extent they differ. Two of the reviews (Jumper, 1995; Rind & Tromovitch, 1997) were also able to precisely compare the genders in terms of CSA outcomes.

Jumper (1995) examined CSA–adjustment relations from 26 published studies with 30 samples. Of 23 samples with identified sources, 30% were clinical, 26% community, 22% student, and 22% mixed. Thus, at least 48% of the identified samples were nonclinical and nonlegal. Most samples (83%) consisted of female participants. Using a weighted means approach (Shadish & Haddock, 1994), Jumper meta-analyzed effect sizes (r s) across samples for depression, self-esteem, and symptomatology (i.e., psychological difficulties other than depression and self-esteem problems). The overall magnitude of the relation between CSA and symptomatology was of medium size, $r = .27$. Community ($r = .29$) and clinical samples ($r = .27$) were similar in magnitude, but student samples were substantially lower ($r = .09$). For self-esteem, community ($r = .34$) and clinical samples ($r = .36$) were also similar, whereas student samples were much lower ($r = -.02$). For depression, the community samples ($r = .17$) were closer to student ($r = .09$) than clinical samples ($r = .34$). Jumper concluded that the student samples were anomalous, possibly because symptoms had not yet manifested at college age. The CSA–symptomatology relation was the same for men ($r = .29$) and women ($r = .26$); the CSA–self-esteem relation, however, was lower for men ($r = -.02$) than women ($r = .24$). On the basis of the symptomatology results, which were derived from nearly twice as many samples as the self-esteem results, Jumper concluded that SA men and women do not differ in terms of subsequent psychological adjustment.

Neumann et al. (1996) examined CSA–adjustment relations using 38 published studies consisting exclusively of female participants, half of which were based on nonclinical samples. These researchers computed an overall effect size (d) for each study (i.e., a study-level effect size) and then meta-analyzed them, obtaining a small to medium weighted mean effect size ($d = .37$). Using Rosenthal's (1984) formula, and assuming a 19% CSA prevalence rate for women in the general population based on Rind and Tromovitch's (1997) estimate, we converted this d to an r . The obtained result ($r = .14$) was considerably smaller than Jumper's estimate of $r = .27$. Neumann et al. also found that the magnitude of the effect sizes differed between nonclinical ($d = .32$) and clinical ($d = .50$) samples. Converting these values to r with the procedure described previously yielded $r = .12$ and $.19$, respectively. Thus, whereas Jumper found that community and clinical samples were similar in terms of mean effect sizes, Neumann et al. found that nonclinical samples had a lower mean effect size than clinical samples. This difference might be due to the fact that Neumann et al.'s nonclinical samples included student samples (but see below). Finally, Neumann et al. found virtually identical effect sizes for samples with a mean age of 30 or below ($d = .39$) and above 30 ($d = .40$), casting doubt on Jumper's speculation that her student

results might be attributable to a lack of time for symptoms to manifest.

Rind and Tromovitch (1997) examined CSA–outcome relations from 7 male and 7 female national probability samples from the United States, Canada, Great Britain, and Spain. These results are especially important for estimating population parameters because these samples were all chosen to be representative of their national populations. Rind and Tromovitch meta-analyzed mean effect sizes from each sample (i.e., sample-level effect sizes) separately by gender and found that the magnitude of CSA–adjustment relations was small for both men ($r = .07$) and women ($r = .10$). These mean effect sizes were not statistically different. For self-reports of CSA effects, significantly more women (68%) reported the presence of some type of negative effect at some point after their CSA experience than did men (42%); the size of this difference was small to medium ($r = .23$). Self-reports in Baker and Duncan's (1985) national study in Great Britain suggested that lasting negative effects for SA persons are rare: 13% for women and 4% for men. Several of the national studies also examined third variables that might account for CSA–adjustment relations. In one study, greater sexual activity in adulthood was confounded with CSA (Laumann, Gagnon, Michael, & Michaels, 1994). In two others (Boney-McCoy & Finkelhor, 1995; Finkelhor, Hotaling, Lewis, & Smith, 1989), most CSA–adjustment relations remained statistically significant after controlling for several possible confounds. However, nonsexual abuse and neglect variables were not held constant in these analyses, weakening any causal interpretations because CSA often occurs along with physical abuse or emotional neglect (Ney et al., 1994) and because CSA–adjustment relations have been shown to disappear when these factors are held constant (e.g., Eckenrode, Laird, & Doris, 1993; Ney et al., 1994). Finally, Rind and Tromovitch reviewed the results of another national study that found that SA girls tended to have disruption in their family, school, and social environments both before and after their CSA experience (Ageton, 1988), weakening causal interpretations regarding CSA effects in the general population.

Synthesis of the Quantitative Reviews

Causality. All three reviews expressed caution regarding causal inferences about CSA–adjustment relations. Jumper (1995) noted that researchers need to differentiate between effects related to CSA and those related to other traumatic events, and to control for family variables. Neumann et al. (1996) argued that third variables such as other forms of maltreatment may be responsible for the CSA–adjustment relation, and that most studies in their review did not consider the possible role of family dynamics. About 72% of the studies in Jumper's review were also reviewed by Neumann et al., suggesting that most of Jumper's studies also did not consider the role of family environment. Rind and Tromovitch (1997) found that the studies in their review usually did not use statistical control, and when they did, it was inadequate. Thus, a quantitative review of studies using statistical control of important potential confounds (e.g., family environment) has yet to be done and is needed to address the issue of causality.

Pervasiveness. Only Rind and Tromovitch's (1997) review

presented data relevant to how widespread negative outcomes are in the population of persons with a history of CSA. Their findings suggest that lasting negative effects are rare, but these results are based on only one study (Baker & Duncan, 1985). These considerations point to the need for further attention to this issue.

Intensity. The meta-analytic reviews were especially useful for assessing the intensity of CSA correlates or effects, indicated by weighted mean effect sizes. Neumann et al. (1996) and Rind and Tromovitch (1997) found that the magnitude of the relation between CSA and adjustment in the general population is small. In contrast, Jumper's (1995) meta-analysis of community samples suggests that the magnitude of the CSA-adjustment relation in the general population is medium in size and equivalent to that in the clinical population. To investigate this discrepancy, we examined the community samples used by Jumper. For symptomatology, Jumper reported the following effect sizes: Bagley and Ramsay (1986), $r = .13$; Mullen, Romans-Clarkson, Walton, and Herbison (1988), $r = .16$; Murphy et al. (1988), $r = .13$; Peters (1988), $r = .30$; Stein, Golding, Siegel, Burnam, and Sorenson (1988), $r = .31$ for the female sample and $r = .37$ for the male sample. We calculated the effect sizes for these samples and obtained, respectively, $r_s = .21, .16, .16, .14, .15$, and $.12$. Because we obtained substantially lower effect sizes in the last three samples, we asked an expert meta-analyst to calculate these values independently; his calculations confirmed ours.¹ We meta-analyzed the recomputed effect sizes, obtaining a small weighted mean effect size ($r = .15$), which is consistent with the results of the other two meta-analytic reviews.

We next examined the four community samples in Jumper's meta-analysis of depression and the three in her meta-analysis of self-esteem. Although we obtained similar effect sizes, two of the samples used in each meta-analysis (from Hunter, 1991) were not valid community samples. Hunter recruited participants through newspaper advertisements and community notices asking for volunteers who were "sexually molested as children" (p. 207). The recruitment method suggests a convenience sample rather than a community sample; further, the notice wording was likely to attract volunteers who had more negative experiences. Thus, the results of Jumper's meta-analyses of depression and self-esteem for community samples have limited generalizability.

In sum, the quantitative reviews indicate that in the entire population of persons with a history of CSA, the magnitude of the CSA-adjustment relation is small, implying that CSA does not typically have intensely negative psychological effects or correlates. The results from the Neumann et al. (1996) and Rind and Tromovitch (1997) meta-analyses, as well as results from the recomputed meta-analysis of Jumper's (1995) community samples, suggest that the student population is not anomalous with respect to CSA-adjustment relations. Instead, it appears that the clinical population is anomalous.

Gender equivalence. Using the recomputed effect sizes for Jumper's (1995) community samples, we recalculated the weighted mean effect sizes for male and female participants for symptomatology and found $r_s = .11$ and $.22$, respectively, compared with reported values of $r = .29$ and $r = .26$, respectively. These revised results suggest a sex difference. Rind and Tromovitch's (1997) meta-analysis did not reveal a sex differ-

ence in CSA-adjustment relations (although the direction of the mean effect sizes was consistent with greater problems for SA women), although it did show a sex difference in self-reported effects. Each meta-analysis was based on only a small number of male samples (Jumper used four; Rind and Tromovitch used five for CSA-adjustment relations and three for self-reported effects). Neumann et al. (1996) examined only female samples. The mixed results regarding CSA-adjustment relations, along with the small number of samples used, suggest the need for a more extensive meta-analytic examination of sex differences.

Current Review

The shortcomings of both the qualitative and quantitative literature reviews point to the need for further investigation of the nature of CSA effects or correlates. Qualitative reviews present mixed conclusions regarding the commonly assumed CSA properties of causality, pervasiveness, intensity, and gender equivalence and are limited by sampling bias, subjectivity, and imprecision. The meta-analytic reviews, after correcting for Jumper's (1995) community sample effect sizes, show low intensity of CSA effects or correlates (in terms of effect size). However, their contributions regarding causality, pervasiveness, and gender equivalence are either absent or wanting because of inadequate reports in the primary studies or the small number of samples included in the analyses. The purpose of the current review was to address these shortcomings and to achieve a more accurate and precise understanding of CSA in the general population. To do so, we meta-analytically examined the literature on CSA-outcome relations in college samples.

College samples were used for several reasons. First, this population provides the largest group of studies on nonclinical populations, which are essential for understanding CSA in the general population. The college population is useful for addressing questions regarding the general population because about 50% of U.S. adults have some college exposure (Fritz, Stoll, & Wagner, 1981; U.S. Bureau of the Census, 1995). Second, studies using college samples provide the most extensive data on moderators of CSA-adjustment relations. Many of these studies have examined confounding variables such as family environment, making them useful for examining causality as well as the magnitude of CSA-adjustment relations. Third, many of these studies have reported a rich variety of other results useful for addressing the issues of pervasiveness of effects and gender equivalence. The CSA literature on college students includes numerous male samples, allowing for a more thorough comparison of the genders than previously reported. In addition, this literature has never been systematically reviewed before, and many studies based on college samples have never

¹ Ralph Rosnow served as the expert meta-analyst. In an attempt to resolve our discrepancies with Jumper, we contacted her. She informed us that her meta-analysis came from her master's thesis and that all her data and calculations were in storage in a different part of the country. She therefore advised us that she would be unable to help but nevertheless suggested that we proceed with our report, mentioning that we were unable to resolve the discrepancies with her.

been published but should be more widely known to counteract a possible publication bias.

A possible shortcoming of focusing on the college population is that college students may be too young for symptoms to have appeared, or they may be better able to cope with CSA stresses than persons in other populations (Jumper, 1995). However, younger and older adults did not differ in CSA-adjustment relations in Neumann et al.'s (1996) meta-analysis. Furthermore, mean effect sizes from college samples, as reported by Jumper, were similar to those from national samples (Rind & Tromovitch, 1997), nonclinical samples (Neumann et al., 1996), and community samples (Jumper, 1995, after corrections). Therefore, the argument that college students are better able to cope and thus present fewer adverse reactions than people in other nonclinical populations lacks empirical support.

We addressed the assumed CSA properties of causality, pervasiveness, intensity, and gender equivalence in several ways. First, we meta-analyzed effect sizes for CSA-symptom relations to estimate the magnitude (i.e., intensity) of the relationship between CSA and adjustment in the college population. Second, we performed semipartial correlation and contrast analyses on the effect sizes to examine gender differences (i.e., gender equivalence), as well as other moderator variables. Third, we meta-analyzed results from self-reported reactions to and effects from CSA to examine gender differences further. Additionally, we analyzed these self-reports to examine the prevalence of negative effects. Fourth, we meta-analyzed relations between CSA and family environment, as well as between symptoms and family environment, to examine the causal role of CSA in producing symptoms. We addressed the issue of causality more directly by examining the results of statistical control from studies that reported this information.

Method

Sample of Studies

Studies were obtained by conducting computerized database searches of PsycLIT from 1974 to 1995, Sociofile from 1974 to 1995, PsycInfo from 1967 to 1995, Dissertation Abstracts International up to 1995, and ERIC from 1966 to 1995. Key terms entered for these databases were *adjustment or effect or effects, college or undergraduate or undergraduates, and sex abuse or sexual abuse or child and adult and sexual*. Studies that we already knew were also included. Reference lists of all obtained studies were read to locate additional studies.

To be included, studies must either have used samples exclusively of college students, or, if noncollege subjects were also included, then results of measures of college students had to be reported separately. For inclusion in analyses of psychological correlates of CSA, studies had to (a) include a control group that contained no students with CSA experiences; (b) use a distinct CSA group, rather than a general "abused" group that could include participants without a history of CSA; (c) report on at least one of the 18 symptoms described below; and (d) provide sufficient data to compute one or more effect sizes. Studies not including reports of psychological correlates were included if they contained data on reactions to CSA, either retrospectively recalled or current reflections; these data had to be classifiable into mutually exclusive negative, neutral, or positive categories. Studies were also included if they contained data on self-reported effects of CSA.

As in other meta-analyses (e.g., Jumper, 1995; Oliver & Hyde, 1993), a single study could report data for more than one sample. Fromuth and

Burkhart (1989) examined two male student samples—one from the Midwest and another from the Southeast—and reported separate statistics for these two samples. These samples were thus treated as distinct. Further, male and female samples within a single study were treated as distinct when results were reported separately for them (cf. Rind & Tromovitch, 1997); this was done to examine gender differences. Many studies reported more than one result, using different measures, for the same psychological correlate (e.g., a depression result from the Beck Depression Inventory and another from the Symptom Checklist). In these cases, effect sizes (r s) were computed for each result and were then averaged using Fisher Z transformations to obtain a single mean effect size. This practice has been used in other meta-analyses (e.g., Erel & Burman, 1995) and has been recommended by Rosenthal (1984). The mean effect size thus computed for a given sample for a particular psychological correlate constituted a "symptom-level" effect size. Finally, numerous studies reported results for more than one type of psychological correlate from a single sample (e.g., anxiety and depression). As in other meta-analyses (e.g., Neumann et al., 1996), we treated multiple different correlates in two ways. First, we computed for each sample with multiple different psychological correlates a "sample-level" effect size by averaging the symptom-level effect sizes from that sample using Fisher Z transformations. We later conducted a meta-analysis on these sample-level effect sizes. Second, we analyzed different psychological correlates (i.e., symptoms) separately in a series of symptom-level meta-analyses.

Applying the above criteria produced 59 usable studies (see the Appendix), consisting of 36 published studies, 21 unpublished dissertations, and 2 unpublished master's theses. These studies yielded 70 independent samples for estimating prevalence rates, 54 independent samples for computing 54 sample-level and 214 symptom-level effect sizes, 21 independent samples that provided retrospectively recalled reaction data, 10 independent samples that provided data on current reflections, and 11 independent samples that provided data on self-reported effects. Prevalence rates were based on 35,703 participants (13,704 men and 21,999 women). Effect size data for psychological correlates were based on 15,824 participants (3,254 men from 18 samples and 12,570 women from 40 samples)—actual numbers of participants are somewhat higher than these because one study, not included in the above totals (Haugaard & Emery, 1989), failed to provide exact sample sizes for men and women. Reaction and self-reported effects data were based on 3,136 participants (783 men from 13 samples and 2,353 women from 14 samples)—actual numbers of participants are somewhat higher because one study, not included in the above totals (Schultz & Jones, 1983), failed to report exact sample sizes for men and women.

Coding the Studies

For each study, the following information was coded: (a) all statistics, if provided, on psychological correlates of CSA, including means, standard deviations, t tests, F ratios, correlations, chi squares, degrees of freedom, and sample sizes; (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. Together, the three basic sets of statistics (differences between CSA and control participants in adjustment, differences between CSA and control participants in family environment, and the relationship between family environment and adjustment) were used to address the question of whether significant relationships between CSA and adjustment were spurious, attributable to the confounding variable of family environment. Finally, the results of all analyses using statistical control were coded (e.g., examining the relationship between CSA and adjustment, holding family environment factors constant). These data were used to directly examine whether any significant relations between CSA and psychological adjustment were spurious.

Psychological Correlates of CSA

Coding of the studies resulted in 18 categories of psychological correlates of CSA; several additional correlates were infrequently reported and were therefore not considered in the meta-analyses. These 18 correlates, along with the measures used to assess them in the various studies, were as follows:

1. Alcohol problems—based on the Michigan Alcoholism Screening Test (MAST; Brady, Foulks, Childress, & Pertschuk, 1982), the alcohol subscale of the Millon Clinical Multiaxial Inventory (MCMI; Millon, 1982), and investigator-authored items.
2. Anxiety—based on the Anxiety subscale of the Symptom Checklist (SCL-90-R; Derogatis, Lipman, & Covi, 1973), the Hopkins Symptom Checklist (HSCL; Derogatis, Lipman, Rickels, Ulenhuth, & Covi, 1974), the Brief Symptom Inventory (BSI; Derogatis & Spencer, 1982), the Trauma Symptom Checklist (TSC-33 and TSC-40; Briere & Runtz, 1989), the MMPI form R (Hathaway & McKinley, 1967), the MCMI, the Institute of Personality and Ability Testing Anxiety Scale Questionnaire (IPAT; Scheier, & Cattell, 1976), the State-Trait Anxiety Inventory (STAI; Spielberger, Gorsuch, & Lushene, 1970), and investigator-authored items.
3. Depression—based on the Depression subscales of the SCL-90-R, the HSCL, the BSI, the TSC-33 and 40, the MMPI form R, the Hugo Short Form of the MMPI (HSF; Hugo, 1971), and the MCMI; depression-related items from the Clinical Analysis Questionnaire (CAQ; Cattell, 1973); the Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961); and investigator-authored items.
4. Dissociation—based on the Dissociative Experiences Scale (DES; Bernstein & Putnam, 1986), Briere's Dissociation Scale (Briere & Runtz, 1988b), and the dissociation subscale from the TSC-33 and 40. This symptom indicates experiences such as depersonalization, memory loss, and not feeling like oneself.
5. Eating disorders—based on the Bulimia Test (BULIT; Smith & Thelen, 1984), the Bulimia Diagnostic Instrument (Nevo, 1985), the Eating Attitudes Test (EAT-26; Garner, Olmsted, Bohr, & Garfinkel, 1982), the Eating Disorder Inventory (EDI; Garner, Olmsted, & Polivy, 1983), and investigator-authored items.
6. Hostility—based on the Hostility subscale of the SCL-90-R and the BSI. This symptom reflects thoughts, feelings, or actions that are characteristic of anger.
7. Interpersonal sensitivity—based on the Interpersonal Sensitivity subscale of the SCL-90-R, HSCL, and BSI. This symptom reflects feelings of uneasiness and marked discomfort when interacting with others, as well as feelings of personal inadequacy and inferiority, especially compared with others.
8. Locus of control—based on the Locus of Control (LOC) scales by Nowicki and Duke (1974), Coleman et al. (1966), and Rotter (1966). This scale measures the extent to which one feels in control of one's life.
9. Obsessive-compulsive symptomatology—based on the Obsessive-Compulsive subscales of the SCL-90-R, HSCL, and BSI. This symptom is concerned with unremitting and irresistible thoughts, impulses, and actions that are ego alien or unwanted.
10. Paranoia—based on the Paranoia subscales of the SCL-90-R, HSCL, BSI, MCMI, MMPI form R, HSF, and CAQ. This symptom reflects a disordered mode of thinking, consisting of thoughts involving, for example, projection, hostility, suspiciousness, grandiosity, and delusions.
11. Phobia—based on the Phobic Anxiety subscales of the SCL-90-R and BSI. This symptom reflects a persistent fear response of an irrational and disproportionate nature to a specific person, place, object, or situation.
12. Psychotic symptoms—based on the Psychoticism subscales of the BSI, SCL-90-R, MCMI, MMPI (form R and HSF, Sc scale), CAQ, and Tennessee Self-Concept Scale (TSCS; Fitts, 1964). For these measures, high scores indicate attributes such as mental confusion and delusions (i.e., first-rank symptoms of schizophrenia such as hallucinations and thought-broadcasting).
13. Self-esteem—based on the TSCS, Rosenberg Self-Esteem Scale (Rosenberg, 1965), Self-Ideal Discrepancy subscale of the Family Perception Grid (Kelly, 1955), the Self subscales of the McPearl Belief Scale (McCann & Pearlman, 1990), subscales from the Erwin Identity Scale (Erwin & Delworth, 1980), and the Coopersmith Self-Esteem Inventory (Coopersmith, 1967).
14. Sexual adjustment—based on Finkelhor's Sexual Self-Esteem Scale (Finkelhor, 1981), Reed's (1988) Romantic and Sexual Self-Esteem Survey, the Derogatis Sexual Functioning Inventory (DSFI; Derogatis & Melisaratos, 1979), the Psychosexual Functioning Questionnaire (Schover, Friedman, Weiler, Heinman, & LoPiccolo, 1982), the Sexual Arousal Inventory (Hoon, Hoon, & Winze, 1976), subscales from the TSC-33 and 40 and the Erwin Identity Scale, and investigator-authored items.
15. Social adjustment—based on the Social Support Questionnaire (Sarason, Levine, Basham, & Sarason, 1983); the Interpersonal Relationship Scale (Schlein, Guernsey, & Stover, 1971); the Inventory of Interpersonal Problems (Horowitz, Rosenberg, Baer, & Ureno, 1988); the Texas Social and Behavioral Inventory (Helmreich & Stapp, 1974); the Social Adjustment Scale (SAS; Weissman & Bothwell, 1976); Rathus' Assertiveness Schedule (Rathus, 1973); Rotter's Interpersonal Trust Scale (Rotter, 1967); the Intimacy Attitude Scale (Treadwell, 1981); the Intimacy Behavior Scale (Treadwell, 1981); subscales from the TSCS, McPearl Belief Scale, the College Self-Expression Scale (Galassi, DeLo, Galassi, & Bastien, 1974), the Student Development Task and Lifestyle Inventory (Winston, Miller, & Prince, 1987), and the Miller Social Intimacy Scale (MSIS; Miller & Lefcourt, 1982); and investigator-authored items.
16. Somatization—based on MacMillan's Health Opinion Survey (MacMillan, 1957); subscales from the HSCL, TSC-33 and 40, BSI, SCL-90-R, MCMI, MMPI form R, HSF, and CAQ; and investigator-authored questions. This symptom reflects bodily related distress such as headaches and pain; it also includes gastrointestinal, respiratory, and cardiovascular complaints and complaints of sleeping problems.
17. Suicidal ideation and behavior—based on the Reasons for Living Inventory (Linehan, Goodstein, Nielsen, & Chiles, 1983), the Suicide Behaviors Questionnaire (Linehan & Nielsen, 1981), and investigator-authored items.
18. Wide adjustment—based on the General Well-Being Schedule (McDowell & Newell, 1987); total or global scores from the HSCL, TSC-33 and 40, SCL-90-R, and BSI; subscales of the Comrey Personality Scales (Comrey, 1970) and the TSCS; investigator-created variables derived from combining scales of standard measures; and investigator-authored items. This factor is a general measure of psychological adjustment or symptomatology and, when derived by combining items or measures, is analogous to Jumper's (1995) "psychological symptomatology" and Neumann et al.'s (1996) "general symptomatology."

Statistical Analyses

The effect size used in this review was r , the Pearson correlation coefficient. For CSA–psychological adjustment relations, positive r s indicated 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–adjustment 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 women, or conversely, that they reported more positive reactions or effects than women.

Formulas for calculating r were taken from Rosenthal (1984, 1995). A number of studies reported results separately for different types of CSA participants (e.g., Collings, 1995; Roland, Zelhart, & Dubes, 1989; Sedney & Brooks, 1984). To make the effect sizes in these cases comparable to those in the majority of studies that compared participants with all types of CSA experiences with controls, we combined all CSA subgroups in a given study into a single CSA group and then compared this group with its control group (cf. Neumann et al., 1996).² Sample-level and symptom-level effect sizes across studies were compared and combined meta-analytically using formulas taken from Rosenthal (1984) and Shadish and Haddock (1994). Combining effect sizes involved transforming r s into Fisher Z s and then weighting the Fisher Z s by the degrees of freedom ($df = N - 3$) associated with their samples. The mean weighted Fisher Z was transformed back to a mean weighted effect size, referred to as the *unbiased effect size estimate* (r_u). This metric was used to estimate the effect size in the population and is considered to be unbiased because it weighs more heavily larger samples whose effect sizes are generally considered to be more precise population estimates (Rosenthal, 1984; Shadish & Haddock, 1994). Statistical significance of the effect size estimates was determined by computing their 95% confidence intervals; an interval not including zero indicated an effect size estimate was significant (Shadish & Haddock, 1994).

To establish interrater reliability for coding, Bruce Rind and Philip Tromovitch independently coded studies for psychological correlates, reactions, self-reported effects, family environment–CSA relations, family environment–adjustment relations, and results of statistical control. Interjudge agreement for these codings ranged from 85% to 100%; all disagreements were resolved by discussion.

Results

Definitions of CSA, Prevalence Rates, and Types of CSA

Definitions. Definitions of CSA varied from one study to the next (see the Appendix). Most studies (70%) defined sexual experiences to be CSA if a sizable age discrepancy existed between the child or adolescent and other person, regardless of the younger person's willingness to participate; 20% of the studies restricted their definition of CSA to unwanted sexual experiences only. Most studies (73%) defined CSA to include both contact and noncontact (e.g., exhibitionism) sexual experiences; 24% restricted their definition to contact experiences only.

Most studies (88%) reported specific upper age limits for children or adolescents in defining CSA. Of these studies, most

(75%) focused on middle to later adolescence with the oldest includable age for "child" usually being 16 (35%) or 17 (25%); a minority of these studies (25%) included only experiences that occurred when participants were younger than 14 or were prepubescent. Regarding age discrepancy, more than half of the studies (59%) defined sexual experiences with someone at least 5 years older to be CSA. This criterion generally applied to experiences that occurred when participants were less than 12 or 13. About a quarter of the studies (27%) also defined adolescent sexual experiences with someone at least 10 years older to be CSA. Others (17%) specified experiences with an adult, an authority figure, someone over 16, or someone older to be CSA. About a third of the studies (32%) also included in their definition peer experiences that were unwanted or forced. Fourteen percent of the studies defined sexual experiences with relatives as CSA, although this criterion generally included an age discrepancy.

Prevalence rates. For male participants, 26 samples provided data usable for estimating the prevalence rate of CSA. Of the 13,704 male participants in these samples, 14% reported sexual experiences classifiable as CSA under the various definitions. The unweighted mean prevalence was 17% ($SD = 10\%$), with a range from 3% to 37%. For female participants, 45 samples provided data that were usable for estimating the prevalence rate. Of the 21,999 women in these samples, 27% reported sexual experiences classifiable as CSA. The unweighted mean prevalence was 28% ($SD = 16\%$), with a range from 8% to 71% (see the Appendix for listing of sample-level prevalence rates).

Types of CSA. Twenty one (35.6%) of the 59 studies contained a breakdown of the types of CSA that occurred along with their frequencies. Types listed varied from study to study, including acts such as an invitation to do something sexual, exhibitionism, fondling, masturbation, oral sex, attempted intercourse, and completed intercourse. Many authors referred to this increasing level of sexual intimacy as "severity" or "seriousness." Using the reported prevalence rates of the various types of CSA from these studies, we estimated the distribution of four basic types of CSA in the college population: exhibitionism, fondling, oral sex, and intercourse. For exhibitionism, we included reports of being shown or showing sex organs in a sexual context. Researchers assessed exhibitionism by asking participants if someone had shown, exhibited, or exposed to them his or her sex organs, or if they had shown, exhibited, or

²Combination of CSA subgroups was achieved by computing a weighted mean, and by computing the "true" variance of all CSA participants. The "true" variance is the value that would have resulted from computing the variance of the scores of all CSA participants irrespective of their subgrouping. This value was obtained by (a) adding the sum of the squares of the CSA subgroups to get the within sum of squares for these subgroups, (b) calculating the between-means sum of squares for the CSA subgroups, (c) adding the within and between sum of squares to get the sum of squares total for the subgroups, and (d) dividing the sum of squares total by the number of CSA scores minus 1. Using the derived mean and variance, the CSA group was then compared with the control group. This procedure produced results that were comparable to those of most other studies that used one overall CSA group and was thus chosen over contrasting the means of the CSA subgroups with the control mean.

exposed their sex organs to the other person at the other person's request. For fondling, we included reports of sexual touching and masturbation. Researchers assessed fondling usually by asking participants if they had experienced fondling or genital touching; occasionally they included nongenital touching as examples of fondling. For intercourse we included both attempted and completed instances. Estimates were based on weighting prevalence rates by sample size across samples. Some studies reported prevalence rates for two combined types (e.g., exhibitionism and fondling) rather than reporting their rates separately. In these cases, we divided the rates evenly between the two types. Because a number of studies categorized SA participants exclusively into the most "severe" type of CSA experienced, the prevalence of less severe types is likely to be underestimated.

The top half of Table 1 shows the estimated prevalence rates in the college population for the different types of CSA for SA 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 based on reports from 3 national samples (Baker & Duncan, 1985; Laumann et al., 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. As with studies based on college samples, these studies used varying definitions of CSA (e.g., contact only vs. both noncontact and contact sex) and of types of CSA such as intercourse (i.e., completed only vs. both attempted and completed). The bottom half of Table 1 displays the estimated prevalence rates for the different types of CSA for SA persons in the general population. Comparing the college and national distributions indicates similar prevalence rates for intercourse for women; SA college men, however, show a higher rate (33%) than SA men in the general population (13%). Because inter-

course is frequently viewed as the most severe or serious type of CSA, these results imply that SA college students, especially men, do not experience less severe CSA than SA persons in the general population.

Severity or seriousness of CSA is often not only viewed as a function of the level of intimacy of the sexual act but also as a function of the closeness of the relationship between the SA person and his or her partner or abuser (e.g., Edwards & Alexander, 1992; Laumann et al., 1994). On the basis of the studies providing relationship information, we estimated the proportion of the college population that has experienced close family CSA (biological or stepparents, grandparents, older siblings) and the proportion that has experienced wider family CSA (including both close family CSA and CSA with other relatives). Estimates were performed for SA women and men separately and combined (see Table 2). Results indicate that only a small proportion of SA college students experience close family CSA (16% for women and men combined), with women experiencing it two and a half times as much (20%) as men (8%).

To provide a frame of reference, we estimated prevalence rates for SA persons in the general population based on reports from the three national samples used previously to estimate prevalence rates for different types of CSA. As is shown in Table 2, estimated prevalence rates for close and wider family CSA are similar in the college and general populations. It is important to note that estimates from the college samples do not underestimate the occurrence of close or wider family CSA relative to estimates based on national samples. This result further implies that SA college students as a group do not experience less severe CSA than SA persons in the general population.

Another commonly used indicator of severity of CSA is its frequency of occurrence (i.e., multiple occurrences are viewed as more severe than a single episode). We estimated the proportion of college students with a history of CSA who experienced more than one CSA episode using all 11 studies that provided this information. We then compared these results with national population estimates based on the same three studies of national

Table 1
Prevalence Rate Estimates of Four Types of CSA in College and National Populations

Sample/gender	<i>k</i>	<i>N</i>	Exhibitionism	Fondling	Oral sex	Intercourse ^a
College						
Female	13	2,172	32%	39%	3%	13%
Male	9	506	22%	51%	14%	33%
Combined ^b	26	2,918	28%	42%	6%	17%
National ^c						
Female	3	590	38%	67%	9%	16%
Male	3	366	25%	69%	22%	13%
Combined	6	956	33%	68%	14%	15%

Note. *k* is the number of samples and *N* is the number of sexually abused respondents in these samples on which prevalence rate estimates of types of child sexual abuse (CSA) are based. Prevalence rate estimates are weighted means of prevalences from individual samples. College estimates come from studies included in the current review; national estimates come from 3 studies of national samples (Baker & Duncan, 1985; Laumann et al., 1994; López et al., 1995).

^a In some college and national studies, intercourse included both attempted and completed acts. ^b Combined values were based on two additional studies (with a male and female sample in each) that reported only combined results. ^c For exhibitionism, only data from López et al. were reported (female: *k* = 1, *N* = 203; male: *k* = 1, *N* = 134; combined: *k* = 2, *N* = 337); for oral sex, only data from Laumann et al. and López et al. were reported (female: *k* = 2, *N* = 476; male: *k* = 2, *N* = 291; combined: *k* = 4, *N* = 767).

Table 2
Prevalence Rate Estimates of Type of Relationship Between Sexually Abused Respondents and Partners—Abusers in College and National Populations

Gender	Wider family CSA				Close family CSA			
	College ^a		National ^b		College ^c		National ^b	
	N	%	N	%	N	%	N	%
Female	2,735	37	606	34	792	20	606	15
Male	580	23	375	13	270	8	375	4
Combined	3,569	35	981	26	1,275	16	981	11

Note. Close family child sexual abuse (CSA) includes sexual relations with very close relative (e.g., biological or stepparents, grandparents, older siblings). Wider family CSA includes both close family CSA and relations with other relatives. Prevalence rate estimates are weighted means of prevalences from individual samples. College estimates come from studies included in the current review; national estimates come from three studies of national samples (Baker & Duncan, 1985; Laumann et al., 1994; Lopéz et al., 1995).

^aBased on 21, 9, and 33 samples for women, men, and combined, respectively. ^bBased on 3, 3, and 6 samples for women, men, and combined, respectively. ^cBased on 10, 6, and 19 samples for women, men, and combined, respectively.

samples used above. In the college samples, based on 11 studies with 1,195 SA participants, the weighted mean percentage that had more than one CSA experience was 46%; for the three national studies, based on 990 SA respondents, the weighted mean percentage was 52%. The unweighted mean percentages were identical in the two groups: 49% (*SD* = 11%) for the college samples and 49% (*SD* = 15%) for the national samples. These results further indicate similarity in CSA severity in the college and general populations.

Finally, force or threat of force is commonly used as an indicator of CSA severity. We estimated the proportion of SA college students whose CSA involved force or threat of force based on the 10 studies (with six male and six female samples) that provided this information. For 355 SA men in these samples, the weighted mean percentage that experienced some degree of force or threat was 23%. For 753 SA women, the weighted mean percentage experiencing some degree of force or threat was nearly twice as much (41%). Unweighted mean percentages across samples were 22% (*SD* = 21%) for men and 42% (*SD* = 26%) for women. The rather large standard deviations for the unweighted estimates suggest that these estimates should be viewed cautiously. An additional study reported that 31% of their SA students, males and females combined, experienced some degree of force or threat of force—a percentage intermediate to, and thus consistent with, the male and female estimates just presented. National population estimates were not possible in the case of force or threat of force, because none of the three studies used above provided relevant data.

Magnitude of the Relationship Between CSA and Psychological Adjustment

Sample-level analysis. To examine the intensity of CSA psychological effects or correlates, we first meta-analyzed the

sample-level effect sizes from the 54 samples for which these could be computed (sample-level effect sizes are listed in the Appendix).³ The resulting unbiased effect size estimate, based on 15,912 participants, was $r_u = .09$, with a 95% confidence interval from .08 to .11. Because this interval did not include zero, the obtained result was statistically significant (i.e., SA students were less well adjusted than controls). This difference in adjustment between SA and control students was small, however, according to Cohen's (1988) guidelines; in terms of variance accounted for, CSA accounted for less than 1% of the adjustment variance.

A chi-square test of the homogeneity of the sample-level effect sizes revealed that they were not homogeneous, $\chi^2(53) = 78, p < .01$. 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 ($r = .36$ in Jackson et al., 1990; $r = .40$ in Roland et al., 1989; $r = -.25$ in Silliman, 1993) with *z* scores of 2.71, 3.16, and -3.60, respectively. The Jackson et al. study included only incest cases in the CSA group, and the Roland et al. study included a large proportion of incest cases. Moreover, Neumann et al. (1996) also found the Roland et al. result to be an outlier. Measures used in these studies from which effect sizes were computed included: the SAS, BDI, RSE, and DSFI (Jackson et al., 1990); the MMPI form R (Roland et al., 1989); and the LOC and TSCS (Silliman, 1993). These measures were all used in other studies whose effect sizes were not outliers, implying that the outlying results were not a function of these measures. Removing these outliers resulted in homogeneity, $\chi^2(50) = 49.19, p > .50$, based on $k = 51$ samples, with $N = 15,635$ subjects. The recalculated unbiased effect size estimate ($r_u = .09$) and the 95% confidence interval (.08 to .11) were unchanged after rounding. The obtained small unbiased effect size estimate implies that, in the college population, the magnitude of the relationship between CSA and adjustment is small, which contradicts the assumption that CSA is associated with intense harm in the typical case.

Symptom-level analysis. Next we examined the magnitude of the relationship between CSA and adjustment at the symptom level. Table 3 presents the results of the 18 symptom-level meta-analyses. The table shows for each meta-analysis the number of independent samples (*k*), the total number of participants in these samples (*N*), the unbiased effect size estimate (r_u), the 95% confidence interval of r_u , and the homogeneity statistic (*H*) based on the chi-square test.

Initial meta-analyses yielded 8 homogeneous and 10 heterogeneous results. In an attempt to achieve homogeneity with heterogeneous sets, we examined the distribution of effect sizes within each of these sets to detect outliers, as defined previously. We removed all such deviant effect sizes and then recomputed the meta-analyses. If homogeneity was achieved in a particular set, then the search for outliers stopped for that set. Otherwise, the

³Appendixes containing other effect sizes for other analyses in the Results section (i.e., symptom-level, moderator analyses, male–female differences, family environment–CSA relations, and family environment–symptom relations) can be obtained by writing to Bruce Rind.

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Table 3
Meta-Analyses of 18 Symptoms Associated With Child Sexual Abuse From College Samples

Symptom	<i>k</i>	<i>N</i>	r_u	95% confidence interval for r_u	<i>H</i>
Alcohol	8	1,645	.07	.02 to .12	2.97
Anxiety	16 (18)	6,870 (7,365)	.13 (.13)	.10 to .15	4.62 (28.72*)
Depression	22 (23)	7,778 (7,949)	.12 (.13)	.10 to .14	25.71 (49.72*)
Dissociation	8	1,324	.09	.04 to .15	1.86
Eating disorders	10	2,998	.06	.02 to .10	9.92
Hostility ^a	5	1,497	.11	.06 to .16	11.22*
Interpersonal sensitivity	7	1,934	.10	.06 to .15	11.78
Locus of control	6	1,354	.04	-.02 to .09	1.65
Obsessive-compulsive	7	1,934	.10	.06 to .15	5.01
Paranoia	9 (10)	1,881 (2,052)	.11 (.13)	.07 to .16	10.34 (20.07*)
Phobia	5	1,497	.12	.07 to .17	8.08
Psychotic symptoms	10 (11)	2,009 (2,180)	.11 (.13)	.06 to .15	10.13 (23.84*)
Self-esteem ^b	16	3,630	.04	.01 to .07	51.31*
Sexual adjustment ^a	20	7,723	.09	.07 to .11	39.49*
Social adjustment	15 (17)	3,782 (4,332)	.07 (.09)	.04 to .10	20.37 (40.62*)
Somatization	18 (19)	4,205 (4,376)	.09 (.10)	.06 to .12	15.20 (33.21*)
Suicide	9	5,425	.09	.06 to .12	10.94
Wide adjustment	14 (15)	3,620 (3,768)	.12 (.11)	.08 to .15	18.77 (24.25*)

Note. *k* represents the number of effect sizes (samples); *N* is the total number of participants in the *k* samples; r_u is the unbiased effect size estimate (positive values indicate better adjustment for control subjects); *H* is the within-group homogeneity statistic (chi square based on $df = k - 1$). Cutting or trimming outliers was performed when effect sizes were heterogeneous in an attempt to reach homogeneity. Original numbers, before cutting or trimming, are shown in parentheses. 95% confidence intervals are based on final (cut or trimmed) distributions.

^a Cutting or trimming outliers failed to produce homogeneity; thus, only original numbers are shown.

* $p < .05$ in chi-square test.

reduced set of effect sizes was examined for new outliers, and, if found, the outliers were removed and the meta-analysis was performed again. If the set of effect sizes was still heterogeneous and no additional outliers were found, the set was considered to be heterogeneous. This procedure resulted in achieving homogeneity in 7 of the 10 initially heterogeneous sets, yielding 15 out of 18 homogeneous sets. Effect sizes remained heterogeneous only for hostility, self-esteem, and sexual adjustment. Of the 9 effect sizes removed in the 7 sets that became homogeneous, the majority came from two of the studies that contributed to the heterogeneity of effect sizes in the sample-level meta-analysis—5 from Roland et al. (1989) and 1 from Jackson et al. (1990). These six effect sizes and one additional effect size from Bendixen et al.'s (1994) female sample were removed from the upper end of their distributions. Two effect sizes were removed from the lower end of their distribution (Fishman, 1991; Fromuth & Burkhart, 1989, Southwest sample). Measures on which removed effect sizes were based in Jackson et al.'s and Roland et al.'s studies were listed previously in the sample-level meta-analysis section; Bendixen et al. and Fishman used investigator-authored items, whereas Fromuth and Burkhart used the SCL-90-R. Many studies with nonoutlying effect sizes used investigator-authored items and the SCL-90-R, implying that the outlying results were not a function of the measures used.

In Table 3, the original numbers (i.e., number of samples, number of participants in these samples, unbiased effect size estimate, and homogeneity statistic) associated with the heterogeneous results for the seven sets that became homogeneous are shown in parentheses, whereas the numbers associated with the

reduced homogeneous sets appear directly under the column headings. Removing outliers showed itself to be productive in achieving homogeneity; further, this procedure had little effect on effect size estimates, indicating that the large majority of effect size estimates can be considered to be reliable estimates of true effect sizes in the college population. The unbiased effect size estimates for all 18 symptoms were small according to Cohen's (1988) guidelines. The effect size estimates ranged from $r_u = .04$ to .13. Despite these small values, all effect size estimates, except for one (locus of control), were statistically significantly greater than zero, as is indicated by their 95% confidence intervals. These findings indicate that, for all symptoms but one, CSA participants as a group were slightly less well adjusted than control participants. The small magnitude of all effect size estimates implies that CSA effects or correlates in the college population are not intense for any of the 18 meta-analyzed symptoms.

Moderator Analyses

Semipartial correlational analysis. To examine whether the variability in sample-level effect sizes could be accounted for by moderator variables, we performed multiple regression analyses. We focused on the sample-level rather than symptom-level effect sizes because of the substantially larger sample-level data set, which is more appropriate for multiple regression analysis. As in other meta-analyses (e.g., Oliver & Hyde, 1993), we performed multiple regression specifically to obtain correlations between each moderator and the effect sizes while controlling for other

moderators, because of the possibility that the moderators were confounded. We focused on semipartial correlations. This moderator analysis was based on a weighted multiple regression procedure, using a weight of $N - 3$ for each sample, which represents the reciprocal of the variance for an effect size r , thereby producing the best linear unbiased estimate (cf. Hedges, 1994); this approach is consistent with the use of unbiased effect size estimates. The sample-level effect sizes were regressed on the three variables that were coded for each sample: level of contact (0 = both noncontact and contact sex, 1 = contact sex only), level of consent (0 = willing and unwanted sex, 1 = unwanted sex only), and gender (0 = male, 1 = female). Examining the relationship of gender with the effect sizes was done to address the issue of gender equivalence. As discussed previously, it is widely believed that contact sex is more severe or serious than noncontact sex; therefore, it was of interest to test whether this factor would account for variability in effect sizes. Finally, it was expected that unwanted sex would be associated with larger effect sizes; hence, level of consent was examined as a moderator. Results from this analysis regarding level of consent and level of contact are likely to be conservative (i.e., their relationship with the effect sizes may be underestimated) because the first level of each variable overlaps with the second level (e.g., willing and unwanted sex overlaps with unwanted sex only). Also entered into the regression equation were two two-way interactions: Contact \times Gender and Consent \times Gender. The Contact \times Consent and Contact \times Consent \times Gender interactions were not included because no male samples consisted exclusively of cases of unwanted contact sex and only one female sample consisted exclusively of unwanted contact sex. Finally, because outliers can skew correlational results, we excluded from the multiple regression analysis the three outliers identified previously in the sample-level meta-analysis. Four studies containing both men and women were also excluded, because they did not report results separately for the two genders.

The regression model was marginally significant, $F(5, 41) = 2.09, p = .09$. Significance tests of predictors were based on adjusting their standard errors to obtain a correct model for multiple regression involving effect sizes (see Hedges, 1994). Three predictors were significantly related at the .05 level to the effect sizes: consent, gender, and the Consent \times Gender interaction. The other two predictors, contact and Contact \times Gender, were not related. The semipartial correlations between these latter two predictors and the effect sizes were, respectively, $sr(41) = .15$ and $-.13$ (two-tailed $ps > .30$). A second regression model was run, eliminating the two nonsignificant predictors in the previous model. This new model was statistically significant, $F(3, 43) = 3.18, p = .03$; all three predictors were significantly related to the effect sizes at the .05 level. The semipartial correlations between the effect sizes and the predictors of consent, gender, and Consent \times Gender were, respectively, $sr(43) = .33, .38$, and $-.36$ (all two-tailed $ps < .05$). These results indicate that unwanted sex and being female were each associated with poorer adjustment. These results have to be qualified, however, because of the significant Consent \times Gender interaction.

Contrast analyses. To investigate the Consent \times Gender interaction, effect sizes for each of the different levels of consent

and gender were meta-analyzed separately, and then contrast analyses were performed comparing the unbiased effect size estimates between the different levels of each moderator. Next, effect sizes within each of the four Consent \times Gender combinations were meta-analyzed separately, and then contrast analyses between unbiased effect size estimates in appropriate combinations were performed. This procedure follows the model of a main effects and then simple effects analysis in an analysis of variance (ANOVA). The contrast analyses were based on the formula presented by Rosenthal (1984) and used weighted Fisher Z transformations of the effect sizes. Within each of the two sets of Fisher Zs being compared in a given contrast analysis, the weight of a Fisher Z was its degrees of freedom (i.e., $N - 3$) divided by the sum of degrees of freedom for all Fisher Zs in that set. Weights in the first set were positive, whereas those in the second were negative. This weighting method resulted in a statistic (i.e., normal deviate z) that is equivalent to Hedges's (1994) between-groups heterogeneity statistic (i.e., Q_{BET} , distributed as χ^2) for testing differences between two sets of effect sizes, in that the square of z is equal to the χ^2 value.

Table 4 presents the results of the four meta-analyses across the different levels of gender and consent. Effect sizes were homogeneous in all four groups and unbiased effect size estimates were all significantly greater than zero, as is indicated by the 95% confidence intervals that did not contain zero. The contrast between the female ($r_u = .10$) and male ($r_u = .07$) unbiased effect size estimates, based on 14,578 participants, was nonsignificant, $z = 1.42, p > .10$, two-tailed. The contrast between the unwanted sex ($r_u = .10$) and all levels of consent ($r_u = .10$) unbiased effect size estimates was also nonsignificant, $z = .03, p > .10$. These nonsignificant main effects are attributable to the Consent \times Gender interaction, which is described next.

Table 5 presents the results of the four meta-analyses for the four different Consent \times Gender combinations. Effect sizes were homogeneous in all four groups. The unbiased effect size estimate for men with all types of consent ($r_u = .04$) was not significantly different from zero. All other unbiased effect size

Table 4
Meta-Analyses of Sample-Level Effect Sizes Assessing CSA-Adjustment Relations in College Students for Each Level of Gender and Consent

Moderator and level	<i>k</i>	<i>N</i>	r_u	95% CI	<i>H</i>
Gender					
Male	14	2,947	.07	.04 to .11	17.05
Female	33	11,631	.10	.08 to .12	23.83
Consent ^a					
All types	35	11,320	.10	.08 to .11	30.12
Unwanted	12	3,258	.10	.06 to .13	12.78

Note. *k* represents the number of effect sizes (samples); *N* is the total number of participants in the *k* samples; r_u is the unbiased effect size estimate (positive values indicate better adjustment for control participants); 95% CI is the 95% confidence interval for r_u ; *H* is the within-group homogeneity statistic (chi square based on $df = k - 1$). All sets of effect sizes were homogeneous.

^a All types of consent included both willing and unwanted child sexual abuse (CSA); unwanted CSA includes unwanted experiences only.

Table 5
*Meta-Analyses of Sample-Level Effect Sizes Assessing
 CSA-Adjustment Relations in College Students for Each
 Gender × Consent Combination*

Gender and consent ^a	<i>k</i>	<i>N</i>	<i>r_u</i>	95% CI	<i>H</i>
Male					
All types	10	1,957	.04	-.00 to .09	9.29
Unwanted	4	990	.13	.07 to .19	3.08
Female					
All types	25	9,363	.11	.09 to .13	14.50
Unwanted	8	2,268	.08	.04 to .12	8.23

Note. *k* represents the number of effect sizes (samples); *N* is the total number of participants in the *k* samples; *r_u* is the unbiased effect size estimate (positive values indicate better adjustment for control participants); 95% CI is the 95% confidence interval for *r_u*; *H* is the within-group homogeneity statistic (chi square based on *df* = *k* - 1). All sets of effect sizes were homogeneous.

^a All types of consent included both willing and unwanted child sexual abuse (CSA); unwanted CSA includes unwanted experiences only.

estimates, however, were significantly greater than zero. For men, the contrast between the unwanted sex (*r_u* = .13) and all types of consent (*r_u* = .04) effect size estimates, based on 2,947 participants, was statistically significant, *z* = 2.16, *p* < .05, two-tailed, indicating that the association between CSA and adjustment problems was stronger for men when the CSA was unwanted than when it included all levels of consent. For women, the analogous contrast between the unwanted sex (*r_u* = .08) and all levels of consent (*r_u* = .11) effect size estimates, based on 11,631 participants, was nonsignificant, however, *z* = -1.03, *p* > .10, two-tailed. For unwanted sex only, the contrast between the female (*r_u* = .08) and male (*r_u* = .13) unbiased effect size estimates, based on 3,258 participants, was nonsignificant, *z* = -1.21, *p* > .10, two-tailed. Finally, for all types of consent, the contrast between the female (*r_u* = .11) and male (*r_u* = .04) effect size estimates, based on 11,320 participants, was statistically significant, *z* = 2.51, *p* < .02, two-tailed.

These results help clarify the significant Consent × Gender interaction found in the multiple regression analysis. Adjustment was associated with level of consent for men, but not for women. Noteworthy is the finding that SA men in the all-levels-of-consent group were unique in terms of not differing from their controls in adjustment. Because all levels of consent corresponds to social and legal definitions of CSA, these results imply that, in the college population, the association between CSA and adjustment problems is not equivalent for men and women. If the definition of CSA is restricted to unwanted sex only, however, then these results imply a gender equivalence between men and women in the association between CSA and adjustment problems.

Simple correlations. In a further attempt to explain variability in sample-level effect sizes, we examined the association between several additional factors and the sample-level effect sizes (the three outliers were not included in these analyses). Associations were computed using weighted correlational analyses (weights were *N* - 3 for each sample). We coded all studies for method of assessment (e.g., face-to-face interview vs. questionnaire), type of institution (e.g., public vs. private),

sampling strategy (e.g., a convenience sample of introductory psychology students vs. a broader sample of students obtained by random or pseudorandom sampling), mean age of students at time of assessment, the maximum age for a "child" in the study's definition of CSA, and whether the study was published. No method variance in assessment emerged because all studies were based on questionnaires. Similarly, type of institution did not show itself to be useful for correlational analysis because nearly all studies were conducted at state universities. For sampling strategy, we categorized studies into two groups: ones that used convenience samples of students (usually psychology or sociology) and ones that used wider samples that included students in nonsocial science courses or that were based on random or pseudorandom sampling of all students at the school. Of the 38 studies for which sampling strategy could be coded, 25 were of the first type and 13 were of the second. Sampling strategy was not related to effect sizes, *r*(36) = .16, *p* > .30, two-tailed. Regarding age of students, if CSA has early effects that diminish over time, or if it has delayed effects that emerge only as students get older, then a significant correlation between mean age of students in the sample and effect sizes would be expected (the range of mean ages in the samples went from 18.0 to 26.6 with an overall mean age of 20.8). The correlation, however, was nonsignificant, *r*(36) = .01, *p* > .90, two-tailed. Similarly, maximum age of "child" in the study's definition of CSA was not related to the effect sizes, *r*(44) = -.05, *p* > .70, two-tailed. The relationship between whether a study was published and the sample-level effect sizes was marginally significant, *r*(49) = .25, *p* = .08, two-tailed. The 27 samples with published results had a slightly larger unbiased effect size estimate (*r_u* = .11) than that of the 24 samples whose results were unpublished (*r_u* = .08).

Moderators concerning aspects of the CSA experience. Studies were inconsistent in providing statistics on aspects of the CSA experience (e.g., force, penetration) that might affect adjustment among SA participants. We examined all studies to search for such moderators and found five types that were reported in at least two studies: force, penetration, duration, frequency, and incest. Additionally, several studies examined moderators that were composite measures that combined two or more of the moderators just listed. Some researchers provided correlations between a moderator and self-reported reactions or effects; other researchers provided correlations between a moderator and symptoms among SA participants. We meta-analyzed separately the moderator-reaction-effect and moderator-symptom relations for the different types of moderators when results for both types of relations were available (we considered individually the results from the studies examining composite moderators). In the case of moderator-symptom relations, if a study provided correlations between a given moderator and more than one symptom, then all of these correlations were averaged using Fisher *Z* transformations to create a single moderator-symptom relation for that study. Some studies reported only beta weights; these values were used as effect size estimates. A number of studies reported only that the relation was nonsignificant or that it was significant; in these cases, following recommended procedures by meta-analysts (e.g., Rosenthal, 1984), we set the effect size to zero in the former case and to the appropriate value corresponding to *p* = .05, two-tailed, in the

second case. Because most effect size assignments were of the former type, some of the unbiased effect size estimates are likely to be underestimates of the moderator-symptom relations.

Table 6 provides summaries of the meta-analyses of the moderator-outcome relations. As shown in the table, only 3 of the 10 moderator-outcome relations reached statistical significance. The presence of force was associated with more negative reactions and self-reported effects; the magnitude of this relation was medium, $r_u = .35$. Incest (i.e., close familial CSA) was associated with both symptoms, $r_u = .09$, and negative reactions-self-reported effects, $r_u = .13$; the magnitudes of these relations were small. Notably, force was unrelated to symptoms, and penetration was unrelated to either outcome. Frequency (i.e., number of CSA episodes) and duration (i.e., length of CSA involvement) were also not related to outcome.

The table also displays recalculated unbiased effect size estimates (shown in parentheses next to original estimates) in cases where one or more effect sizes were estimated. These new effect size estimates were computed using only the known effect size values. The statistical significance of these recalculated values changed in only one case. Symptoms associated with penetration became statistically significant (95% confidence interval = .02 to .30). This result, however, should be viewed with caution, because it is based on the removal of more than half the effect sizes for this outcome, all of which were nonsignificant.

Five studies examined composite measure-symptom relations. In one, a composite measure of paternal incest, force, and penetration was associated with poorer adjustment (Edwards & Alexander, 1992). Composite measure-symptom relations in the other four studies, however, were nonsignificant. In these studies, the composite measures consisted of incest, frequency,

force, and genital contact (Greenwald, 1994); type of CSA and frequency (Smolak, Levine, & Sullins, 1990); extent of physical contact and invasiveness of the sex (Mandoki & Burkhart, 1989); factors such as invasiveness, duration, and frequency (Cole, 1988). The inconsistency in results and in composition of the composite measures makes it difficult to draw conclusions concerning the composite measure-symptoms relations. Future research is required to address this issue by systematically documenting which combinations of moderators are reliably associated with symptoms.

Self-Reported Reactions to and Effects From CSA

To examine further whether CSA is an equivalent experience for males and females, we compared the genders in terms of their self-reported reactions to and effects from CSA. If a basic property of CSA is that it is an equivalent experience for males and females, then it follows that correlates of this experience (e.g., self-perceptions of negativity and harmfulness) should be similar for men and women in the college population. These subjective self-reports were also useful for addressing the assumption that harmful effects are pervasive and intense in the population of persons with a history of CSA.

Retrospectively recalled immediate reactions. Fifteen studies presented data on participants' retrospectively recalled immediate reactions to their CSA experiences that were classifiable as positive, neutral, or negative. Table 7 presents the reaction data separately for 10 female and 11 male samples. Some authors reported the number of participants who reported positive, neutral, or negative reactions; others reported the number of experiences reported to be positive, neutral, or negative. We

Table 6
Meta-Analyses of Relations Between Aspects of the Child Sexual Abuse Experience and Outcome in Sexually Abused College Students

Moderator and outcome	<i>k</i>	Est.	<i>N</i>	r_u	95% CI	<i>H</i>
Duration						
Reactions-effects	4	1 ^a	473	-.03 (-.04)	-.12 to .06	1.70
Symptoms	2	0	82	.21	-.01 to .41	0.84
Force						
Reactions-effects	7	2 ^b	694	.35 (.40)	.28 to .41	29.70*
Symptoms	4	1 ^a	295	.11 (.14)	-.01 to .24	1.71
Frequency						
Reactions-effects	3	2 ^a	328	-.02 (-.09)	-.13 to .09	0.49
Symptoms	3	0	174	.08	-.07 to .23	0.53
Incest						
Reactions-effects	4	0	394	.13	.03 to .22	4.73
Symptoms	9	1 ^a	572	.09 (.11)	.01 to .17	15.20
Penetration						
Reactions-effects	2	0	253	-.03	-.15 to .10	0.30
Symptoms	7	4 ^a	594	.05 (.16)	-.03 to .13	4.32

Note. *k* represents the number of effect sizes (samples); Est. is the number of effect sizes that had to be estimated because statistics were not provided or were inadequate; *N* is the total number of participants in the *k* samples; r_u is the unbiased effect size estimate (positive values indicate worse reactions or poorer adjustment for participants who experienced greater degrees of the moderator); values in parentheses after some r_u s represent unbiased effect size estimates based on only known (i.e., nonestimated) *r*s; 95% CI is the 95% confidence interval for r_u based on both known and estimated *r*s; *H* is the within-group homogeneity statistic (chi square based on $df = k - 1$).

^a Estimated effect sizes set at $r = 0$. ^b Estimated effect sizes based on $p = .05$, two-tailed.

* $p < .05$.

Table 7
*Retrospectively Recalled Immediate Reactions of College Students
 to Their Child Sexual Abuse Experiences*

Study	Women (%)				Men (%)			
	Positive	Neutral	Negative	N	Positive	Neutral	Negative	N
Brubaker, 1991	22	18	60	50	—	—	—	—
Brubaker, 1994	10	17	73	99	—	—	—	—
Condy et al., 1987	—	—	—	—	58	14 ^a	28	50
Finkelhor, 1979	7	27	66	119 ^b	n/a	n/a	38	23
Fischer, 1991	5	n/a	n/a	39	28	n/a	n/a	18
Fishman, 1991	—	—	—	—	27	43	30	30 ^b
Fromuth, 1984	28	12	60	130 ^b	—	—	—	—
Fromuth & Burkhart, 1989	—	—	—	—	60	28	12	81
Goldman & Goldman, 1988	17	16	68	188 ^b	39	32	30	40 ^b
Landis, 1956	2	16	82	493 ^b	8	39	54	183 ^b
Long & Jackson, 1993	4	28 ^a	69	137	—	—	—	—
O'Neill, 1991	10	6	84	83 ^b	43	9	48	46 ^b
Schultz & Jones, 1983	28	19	52	122 ^b	69	24	7	67 ^b
Urquiza, 1989	—	—	—	—	39	27	33	51
West & Woodhouse, 1993	—	—	—	—	45	29	26	58
Totals	11	18	72	1,421	37	29	33	606

Note. Dashes indicate that participants of a given gender were not included in the study. n/a indicates information not available. Totals include only samples for which all 3 reaction types are given. Total percentages are weighted by sample size; total *N*s reflect a combination of number of experiences and number of participants. Percentages do not sum exactly to 100 because of rounding.

^a Includes mixed reactions. ^b Indicates number of experiences. Otherwise, *N* indicates number of participants.

therefore treated reports of numbers of participants as numbers of experiences (i.e., one participant equals one experience) so as to be able to combine results. Overall, 72% of female experiences, but only 33% of male experiences, were reported to have been negative at the time. On the other hand, 37% of male experiences, but only 11% of female experiences, were reported as positive. These overall percentages were obtained by weighting the percentages of each sample by their sample size (only samples in which all three reaction-types were reported were combined).

These results indicate that males and females did not react to CSA at the time it occurred in an equivalent manner. The partial results reported by Finkelhor (1979) and Fischer (1991) are consistent with the overall results. Also consistent with these results are those obtained by Haugaard and Emery (1989), who reported mean retrospectively recalled immediate reactions based on a 7-point scale (1 = *very positive*; 7 = *very negative*). The mean rating for men was 3.38, indicating a neutral to somewhat positive overall reaction, and the mean rating for women was 5.83, indicating an overall negative reaction. Aside from gender differences, the results show that reactions were highly variable, rather than being exclusively negative. Assuming that retrospectively recalled immediate reactions are associated with later adjustment—a relation that was found by Long and Jackson (1993) in their study using a college sample—these results imply that resulting harm is not prevalent, at least for men, in the college population.

Current reflections. Seven female and three male samples contained reports of positive, neutral, and negative current reflections (i.e., current feelings) about CSA experiences. Results were similar to retrospectively recalled immediate reactions,

with 59% of 514 female experiences being reported as negative compared with 26% of 118 male experiences. Conversely, 42% of current reflections of male experiences, but only 16% of female experiences, were reported as positive. In addition to these results, Haugaard and Emery (1989) reported mean current reflections based on a 7-point scale (1 = *very positive*; 7 = *very negative*). The mean rating for men was 3.95, indicating neutral overall current reflections, and the mean rating for women was 5.82, indicating current reflections that were negative overall. These data further point to the nonequivalence of male and female CSA experiences and imply that harmful effects may not be prevalent.

Self-reported effects. In eight studies, comprising 11 samples, participants were asked whether their CSA experiences had affected them. In some studies, effects pertained to participants' adult sex lives or their sexual attitudes (Condy et al., 1987; Fishman, 1991; Fritz et al., 1981; Landis, 1956). In other studies, questions about effects covered more general topics, for example, amount of stress (Fischer, 1991), effects on one's overall life (Fishman, 1991), still feeling troubled (Hrabovey, 1987), time to recover and damage to emotional development (Landis, 1956), how long they were affected (Nash & West, 1985), and lasting effects (West & Woodhouse, 1993). Table 8 provides the results of participants' responses to these questions.

For men, self-reported negative effects on their current sex lives or attitudes were uncommon. In the five studies providing data regarding these perceived effects, rates of negative sexual effects ranged from 0.4% of participants to 16%, with an unweighted mean rate of 8.5%. For women, self-reported negative effects were also in the minority; only two samples provided relevant data, with rates of 2.2% and 24%, yielding an un-

Table 8
Self-Reported Effects of Child Sexual Abuse Experiences on College Students

Study	Gender	N	Type of effect	Response
Condy et al., 1987	M	51	Adult sex life	Good = 37%; none = 28%; mixed = 9%; bad = 16%
Fischer, 1991	F	54	Stress then or now	No stress then or now = 7%; mean stress now = 3.00 on 1-10 scale
Fischer, 1991	M	24	Stress then or now	No stress then or now = 21%; mean stress now = 2.12 on 1-10 scale
Fishman, 1991	M	30 ^a	Overall life	Positive = 17%; neutral = 57%; negative = 27%
			Current sex life	Positive = 24%; neutral = 63%; negative = 13%
Fritz et al., 1981	F	42	Current sex life	Problems = 24%
Fritz et al., 1981	M	20	Current sex life	Problems = 10%
Hrabowy, 1987	F	107	Troubled over it now	Minimal or trouble free = 75%; moderately = 20%; very = 5%
Landis, 1956	F	531 ^a	Time to recover	No shock = 25%; little/no = 17%; days to years = 51%; never = 4%
			Damage to emotional development	None = 66%; temporary = 30%; permanent = 3%
			Effect on sex attitudes	None = 70%; temporary = 26%; permanent = 2.2%
Landis, 1956	M	215 ^a	Time to recover	No shock = 68%; little/no = 10%; days to years = 22%; never = 0%
			Damage to emotional development	None = 81%; temporary = 19%; permanent = 0%
			Effect on sex attitudes	None = 80%; temporary = 17%; permanent = 0.4%
Nash & West, 1985	F	50	How long affected	Not at all/weeks = 52%; months = 16%; year/+ = 10%; still = 22%
West & Woodhouse, 1993	M	67	Lasting effects	"Only . . . one or two" out of 67 of a sexual nature

Note. M = male; F = female.

^a Indicates number of experiences. Otherwise, N indicates number of participants.

weighted mean of 13.1%. One study (Landis, 1956) also provided rates of temporary negative effects on sexual attitudes: 17% for men and 26% for women.

Self-reports of lasting negative effects of a general nature for men were also uncommon. About a quarter of male participants reported lasting negative effects in one study, but none reported lasting effects in the other two studies asking this question—in one of these latter studies (West & Woodhouse, 1993), 1 or 2 participants reported lasting negative effects of a sexual, rather than general, nature. Landis (1956) reported that only a minority of his male participants perceived themselves to have been temporarily adversely affected. Fischer (1991) found that the mean amount of stress that men reported they felt now as a result of their CSA was low. Fischer found that her female participants who experienced CSA reported a somewhat higher mean but were still on the low end of the scale. In other female samples, Hrabowy (1987) found that only 5% of her participants reported currently being very troubled over their CSA experiences; another 20% reported being moderately troubled. Landis found that fewer than 1 in 20 of his female participants with CSA experiences reported that they never recovered or that they suffered permanent damage to their emotional development. Nash and West (1985) found that 1 in 5 of their CSA participants reported still being affected. Landis reported that about two thirds of his female CSA participants felt themselves to have been temporarily affected. Nash and West found that half of their CSA participants perceived themselves to have been affected for a little or no time, while another quarter were affected for a longer, but temporary, period of time.

The overall picture that emerges from these self-reports is that (a) the vast majority of both men and women reported no negative sexual effects from their CSA experiences; (b) lasting general negative effects were uncommon for men and somewhat more common for women, although still comprising only a minority; and (c) temporary negative effects were more common,

reported by a minority of men and a minority to a majority of women. These data imply that, in the college population: (a) CSA affects males and females differently; (b) lasting negative effects are not prevalent; and (c) when negative effects occur, they are often temporary, implying that they are frequently not intense. These findings are inconsistent with the assumption that CSA has the properties of gender equivalence, prevalence, and intensity in terms of harmful effects.

Comparing male versus female reactions and self-reported effects via meta-analysis. In three meta-analyses, we examined the size of sex differences in (a) retrospectively recalled immediate reactions, (b) current reflections, and (c) self-reported effects of CSA. Studies included in these analyses consisted of both male and female samples. In the case of Risin and Koss (1987), who reported on male participants, and Wisniewski (1990), who reported on female participants, all participants came from the same pool (a random sample of 32 U.S. colleges and universities, designed to be representative of the entire U.S. college population). In two other cases, we combined results from separate studies that used different samples. The first case was Fromuth (1986) and Fromuth and Burkhart (1989), and the second case was Nash and West (1985) and West and Woodhouse (1993). Combining appeared to make sense because the same principal researchers were responsible for each set of studies (Fromuth and West, respectively), and the samples were drawn from nearly the same geographic areas, although at different times. In most cases, comparisons were made between the proportion of men who reported negative reactions or effects and the corresponding proportion of women. In the case of Haugaard and Emery (1989), comparisons were based on contrasting mean reaction ratings of men and women. Positive effect sizes indicated that women reported proportionately more negative reactions or effects, or had a higher mean negative response, than males. Table 9 presents the results of the meta-analyses.

Table 9
*Meta-Analyses for Male Versus Female Reactions to
 and Self-Reported Effects From Child
 Sexual Abuse in College Samples*

Measure ^a	<i>k</i>	<i>N</i>	<i>r_u</i>	95% CI	<i>H</i>
Reactions then	10	2,965	.31	.28 to .34	30.70*
Reactions now	3	424	.34	.25 to .42	2.13
Self-reported effects	4	835	.22	.15 to .28	1.12

Note. *k* represents the number of effect sizes for a given meta-analysis; *N* is the total number of participants in a given meta-analysis; *r_u* is the unbiased effect size estimate (positive *r_u* indicates more negative reactions or effects for women); *H* is the within-group homogeneity statistic (chi square).

^a *Reactions then* refers to retrospectively recalled immediate reactions; *reactions now* refers to current reflections.

* *p* < .05 in chi-square test.

In the case of retrospectively recalled immediate reactions, Risin and Koss (1987) and Wisniewski (1990) presented percentages of participants who responded to their CSA experiences with fear, guilt, anger, depression, or feelings of being victimized. Each item was measured on a 5-point scale whose values were 1 = *not at all*; 2 = *a little*; 3 = *somewhat*; 4 = *quite*; and 5 = *very*. We averaged the proportion of men and women across the 5 items who reported anything from "a little" to "very" to compare the proportions of each sex who made negative reports. The meta-analysis, based on 10 effect sizes that ranged from *r* = .21 to .52, yielded a medium unbiased effect size estimate, *r_u* = .31, in which women reported significantly more negative immediate reactions than men (indicated by the 95% confidence interval). The effect sizes were heterogeneous, however. The meta-analysis of current reflections, based on 3 effect sizes ranging from .24 to .38, also yielded a medium unbiased effect size estimate, *r_u* = .34, in which women's current reflections concerning their CSA experiences were significantly more negative than those of males (indicated by the 95% confidence interval). These effect sizes were homogeneous.

For the self-reported effects, effect sizes were derived as follows: contrasting 21% of men with no stress then or now with 7% of women for Fischer (1991); contrasting 10% of men with current sex problems reported to have resulted from the CSA with 24% of women (Fritz et al., 1981); for Landis (1956), averaging the effect sizes for self-reports of time to recover, damage to emotional development, and effects on sexual attitudes (in each case, proportions of men and women reporting any negative effects at all were contrasted); and for Nash and West (1985) and West and Woodhouse (1993), the proportions of women and men reporting lasting negative effects were contrasted. The meta-analysis, consisting of four effect sizes ranging from *r* = .16 to .30, yielded a small to medium unbiased effect size estimate, *r_u* = .22, indicating that women reported significantly more negative effects than men (indicated by the 95% confidence interval). The effect sizes were homogeneous.

The results of these three meta-analyses imply that, in the college population, men and women with experiences classifiable as CSA feel very differently about them and perceive very different effects from them. The assumption that CSA is an

equivalent experience for men and women in the population of persons who experience CSA is unsupported by these results.

Family Environment

Analyses of the CSA-symptom relations indicated that college students with a history of CSA were, on average, slightly less well adjusted than college students without such a history. The question arises as to whether these relations were causal in nature. That CSA usually or inevitably causes harm is a basic assumption of many mental health care workers and child abuse researchers. The self-reported effects data, however, do not support this assumption. Nevertheless, self-reports by themselves cannot be taken as firm evidence for or against the role of CSA in causing harm, because people are frequently unaware of the causes of their behavior or current states when causal relations are ambiguous or complex (cf. Nisbett & Wilson, 1977). Therefore, we addressed the issue of causation further by considering family environment. Research using clinical samples has consistently shown that family environment and CSA are confounded (e.g., Beitchman et al., 1991), which weakens the argument that CSA-symptom relations in these samples are causal. We 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.

Family environment-CSA relations. Each study that assessed family environment factors was coded for type of factor, gender, number of participants used to compute the comparison statistic, and the comparison statistic itself—the effect size *r* was computed from this statistic. Once all the family environment factors had been listed, Bruce Rind and Philip Tromovitch attempted to classify them into a smaller number of distinct categories. Results were compared, and discrepancies were resolved by discussion. Six general categories emerged: nonsexual abuse and neglect, adaptability, conflict and pathology, family structure, support and bonding, and traditionalism.

The effect sizes for each family environment category were meta-analyzed, as shown in Table 10. For all 6 categories, the effect size estimates were statistically significant, indicated by

Table 10
*Meta-Analyses of Six Family Environment Factors
 as a Function of CSA Status*

Family factor	<i>k</i>	<i>N</i>	<i>r_u</i>	95% CI	<i>H</i>
Abuse and neglect	5	1,098	.19	.13 to .25	2.36
Adaptability	3	976	.13	.06 to .19	20.38*
Conflict or pathology	9	4,906	.14	.12 to .17	0.74
Family structure	4	3,803	.09	.06 to .12	6.54
Support or bonding	13	3,288	.13	.09 to .16	36.46*
Traditionalism	5	836	.16	.09 to .22	8.26

Note. *k* represents the number of effect sizes (samples); *N* is the total number of participants in the *k* samples; *r_u* is the unbiased effect size estimate; 95% CI is the 95% confidence interval for *r_u*; *H* is the within-group homogeneity statistic (chi square). A positive *r_u* indicates better family adjustment or functioning in the control than child sexual abuse (CSA) group.

* *p* < .05 in chi-square test.

the 95% confidence intervals. The unbiased effect size estimates ranged from $r_u = .09$ to $.19$, with a weighted mean $r = .13$. Effect sizes were homogeneous in 4 of the 6 categories. Only adaptability and support-bonding were heterogeneous. The positive values of the effect size estimates imply that college students with a history of CSA come from more problematic home environments than control students, implying that CSA and family environment are confounded in this population.

Family environment-symptom relations. The confounding of CSA and family environment raises the possibility that CSA may not be causally related to symptoms in the college population or may be related in a smaller way than uncontrolled analyses have indicated. To address this issue, we examined the relationship between family environment and symptoms. All studies providing statistics assessing the relationship between these two factors were coded. For each study, effect sizes were computed for all family environment-symptom relations. Additionally, for each study, a study-level effect size was computed; this value represents the mean effect size based on Fisher Z transformations of all family environment-symptom relations in that study. A series of symptom-level meta-analyses and a study-level meta-analysis were then performed.

Table 11 provides the results of the meta-analyses of the symptom-level and study-level effect sizes. Symptoms that

had only one effect size were not meta-analyzed. The effect sizes ranged from $r = .04$ to $.49$. All effect size estimates based on two or more effect sizes were significantly greater than zero, as indicated by their 95% confidence intervals. Five of the seven effect sizes based on single samples were significantly greater than zero. In the majority of cases, effect size estimates were based on a small number of samples and the effect sizes used to derive these estimates were heterogeneous. This latter finding is not surprising, given the heterogeneous collection of family environment measures for any given symptom. These estimates should therefore be viewed with caution. Nevertheless, with the exception of two measures based on single samples, the effect sizes were generally medium in size, in contrast to the CSA-symptom and CSA-family environment effect sizes, which were generally small. The study-level effect size estimate was $r_u = .29$, indicating an overall medium association between family environment and symptoms. In terms of variance accounted for, family environment outperformed CSA in explaining symptoms by a factor of 9. These results imply that, in the college population, family environment is a more important predictor of symptoms than is CSA (see below for a discussion of the statistical validity of comparing CSA-symptom and family environment-symptom relations).

Statistical control. Results of the three sets of analyses just presented (i.e., meta-analyses of the relationships between CSA and symptoms, CSA and family environment, and family environment and symptoms) are consistent with the possibility that the small but statistically significant CSA-symptom associations found in the studies reviewed may have been spurious. This possibility is suggested by the logic of semipartial correlational analysis, or equivalently, hierarchical regression analysis (Keppel & Zedeck, 1989). These analyses are useful for determining whether a significant relationship between two variables remains significant after controlling for extraneous factors. The necessary conditions for a significant relationship to be reduced to nonsignificance are as follows: (a) the independent variable (e.g., CSA) is related to the dependent variable (e.g., symptoms), (b) the independent variable is related to a third variable (e.g., family environment), (c) the third variable is related to the dependent variable, and (d) the significant relation between the independent and dependent variables is rendered nonsignificant when the third variable is statistically controlled for. The analyses presented above demonstrate that the first three of these conditions were generally satisfied. Further, the finding that the mean correlation between CSA and symptoms ($r = .09$) was somewhat smaller than that between CSA and family environment ($r = .13$), which in turn was substantially smaller than that between family environment and symptoms ($r = .29$), suggests that many significant CSA-symptom relations might be reduced to nonsignificance with statistical control. To address this possibility directly, we coded all studies that employed statistical control (see Table 12).

Coding involved recording for each study the type of statistical control used, the number of symptoms whose relationships with CSA were controlled for, the number of significant CSA-symptom relations before statistical control, and the number

Table 11
Meta-Analyses of Symptoms as a Function of Family Environment Factors

Symptoms	<i>k</i>	<i>N</i>	r_u	95% CI	<i>H</i>
Alcohol	1	383	.04	-.06 to .14	—
Anxiety	3	788	.34	.28 to .40	19.80
Depression	5	1,279	.38	.33 to .43	22.28*
Dissociation	1	251	.49	.39 to .58	—
Eating disorders	4	822	.21	.15 to .28	10.05*
Hostility	1	383	.15	.05 to .25	—
Interpersonal sensitivity	2	634	.32	.24 to .38	20.25
Locus of control	1	383	.07	-.03 to .17	—
Obsessive-compulsive	2	634	.27	.20 to .34	4.02*
Paranoia	1	383	.16	.06 to .26	—
Phobia	1	383	.18	.08 to .28	—
Psychotic symptoms	1	383	.22	.12 to .31	—
Self-esteem	5	1,354	.26	.20 to .30	37.13*
Sexual adjustment	2	337	.23	.13 to .33	0.24
Social adjustment	3	653	.41	.35 to .47	20.50*
Somatization	2	634	.22	.15 to .29	12.59*
Suicide	2	634	.26	.18 to .33	1.41
Wide adjustment	4	992	.31	.25 to .37	12.95*
Study level	13	2,846	.29	.26 to .33	62.56*

Note. *k* represents the number of effect sizes (samples); *N* is the total number of participants in the *k* samples; r_u is the unbiased effect size estimate (positive values indicate greater degrees of symptoms are associated with poorer family functioning); 95% CI is the 95% confidence interval for r_u ; *H* is the within-group homogeneity statistic (chi square)—dashes indicate *H* was not computed because only one sample was involved. Meta-analyses were performed when $k > 1$. Study-level effect sizes are mean effect sizes, based on Fisher Z transformations, of all symptom-family environment relations in a given study.

* $p < .05$ in chi-square test.

Table 12
Results of Statistical Control on CSA-Symptoms Relations

Study	Type of control	N	Significant results		
			Before	After	% reduction
Brubaker, 1991	Separated categories	1	1	0	100
Cole, 1988	Hierarchical regression	5	3	0	100
Collings, 1995	ANCOVA	10	8	6	25
Fromuth & Burkhart, 1989, mw	Hierarchical regression	13	6	6	0
Fromuth & Burkhart, 1989, se	Hierarchical regression	13	0	0	—
Fromuth, 1986	Hierarchical regression	13	4	1	75
Gidycz et al., 1995	Path analysis	3	0	0	—
Greenwald, 1994	Hierarchical regression	1	0	0	—
Harter et al., 1988	Path analysis	2	1	0	100
Higgins & McCabe, 1994	Hierarchical regression	2	2	0	100
Lam, 1995	Multiple regression	3	0	0	—
Long, 1993	Multiple regression	2	1	0	100
Pallotta, 1992	ANCOVA	13	6	0	100
Yama et al., 1992	ANCOVA	2	2	1	50
Totals		83	34	14	59 ^a

Note. N indicates the number of symptom measures whose relation to child sexual abuse (CSA) status was examined (or was intended to be by the study authors) by using statistical control. "Before" indicates the number of relations significant before applying statistical control; "After" indicates the number of significant relations after applying statistical control; and "Reduction" indicates the percentage of significant relations that became nonsignificant after statistical control—dashes indicate that percentage reduction was not computed because all results were initially nonsignificant. ANCOVA = analysis of covariance; mw = Midwest; se = Southeast.

^a Based on the percentage of total significant relations that became nonsignificant after control. The unweighted percentage of reduction was 83%.

of significant CSA-symptom relations after statistical control.⁴ Table 12 displays the results of this coding. In the last column the percentage of reduction from before to after statistical control is provided. Statistical control was used in 13 studies with 14 samples—in some cases control was not used because nonsignificant correlations between symptoms and family environment obviated this procedure, although the researchers had planned to use statistical control; these samples are included in this analysis. In all cases but one (i.e., Brubaker, 1991), statistical control involved using statistical procedures such as hierarchical regression or analysis of covariance (ANCOVA). Brubaker (1991) imposed control by separating her participants into mutually exclusive categories (i.e., no abuse, CSA only, psychological abuse only, physical abuse only, followed by combinations of these abuse types). This deconfounding procedure has been used recently by other researchers examining noncollege samples, who have shown that when CSA is isolated, its negative correlates tend to shrink considerably or disappear (e.g., Eckenrode, Laird, & Doris, 1993; Ney et al., 1994).

Of 83 CSA-symptom relations, 34 (41%) were significant before statistical control. Only 14 (17%) remained significant after statistical control. It is important to note that, within any given study, multiple CSA-symptom relations were not independent, because they were based on the same sample. It may therefore be more appropriate to use only one result per study (e.g., percentage of reduction) to evaluate the effects of statistical control. Using this approach, the overall reduction from statistical control was 83% (as opposed to the 59% reduction using dependent relations). One additional study, not shown in

the table and not included in the above analysis, also used statistical control (Wisniewski, 1990). This study was based on 3,187 female college students drawn from 32 colleges and universities that were fairly representative of all institutions of higher learning in the United States. Unlike the other studies using statistical control, which held extraneous factors constant for all participants (with or without CSA) in a single analysis, Wisniewski conducted four separate analyses using path analysis, one for each separate group of participants (i.e., no CSA, nonincest CSA, incest CSA, and nonincest CSA with adult revictimization). For all CSA participants, she constructed a CSA severity score that reflected the degree of felt victimization from and negative reactions to the CSA. Results of her analyses revealed that CSA did not contribute to current adjustment for nonincest or incest CSA participants and contributed to only a small degree (β weight = .02) in the case of incest with adult revictimization subjects. Wisniewski found that other factors, particularly family violence, best explained current adjustment.

Results from studies using statistical control supplement the analyses of the intercorrelations among CSA, symptoms, and family environment. They provide direct evidence that the majority of significant CSA-symptom relations examined in the college samples may have been spurious. These results imply that significant CSA-symptom relations in studies based on

⁴ It would have been preferable to code and examine effect sizes before and after statistical control, rather than the number of (non) significant relations. Because of inadequate reporting of the statistics that resulted from statistical control, this procedure could not be used.

college samples cannot be assumed to represent effects of CSA. Although the results of the analyses of statistical control, as well as analyses of the CSA-symptom-family environment relations, do not prove that CSA-symptom relations are spurious in the college population, they specifically do not support the assumption that a basic property of CSA is that it causes psychological injury.

Statistical validity. In comparing CSA-symptom and family environment-symptom relations, as well as statistically controlling for family environment when assessing CSA-symptom relations, several statistical issues may relate to the validity of these analyses. It is possible that the CSA-symptom association may be underestimated relative to the family environment-symptom association. First, often unstandardized measures of CSA may have less reliability than measures of family environment. Lower reliabilities translate into attenuated correlations (Glass & Hopkins, 1996; Hunter & Schmidt, 1994). Second, CSA is usually measured as a dichotomous variable (i.e., present or absent), whose distribution tends to be skewed with a strong mode in the absent category. Low base rates for a category of interest (e.g., CSA) can attenuate correlations (Glass & Hopkins, 1996; Rosenthal & Rosnow, 1991). Further, the artificial dichotomization of an independent variable (e.g., CSA) can also attenuate correlations (Glass & Hopkins, 1996; Hunter & Schmidt, 1994).

Regarding the first point, although most studies on CSA have not assessed the reliability of their measures of CSA, several have, all of which were based on college samples. Messner et al. (1988) reported that 2-week test-retest reliabilities for characteristics of CSA experiences (e.g., duration, frequency, age of onset) were all greater than .69. Long and Jackson (1993) reported that 2-week test-retest reliabilities for emotional reactions to CSA at the time it occurred ranged from .70 to .96, with a mean of .83. Pallotta (1992) reported that 2-week test-retest reliabilities for CSA characteristics (e.g., duration, age of onset) ranged from .93 to 1.00, with a mean of .97. She also reported corresponding reliabilities for negative family environment characteristics, with a mean of .90. Koss and Gidycz (1985) reported that 1-week test-retest agreement on a measure of unwanted sexual experiences since age 14 was 93%. These results point to acceptable reliabilities for measures of CSA, which are comparable to reliabilities for family environment measures—for example, 8-week test-retest reliabilities on the Family Environment Scale have ranged from .68 to .86 (Cole, 1988). Furthermore, the reliability results from the first three of the studies just discussed are especially relevant, because their measures of CSA were modified versions of Finkelhor's (1979) measure; about half of the studies in the current review used modifications of Finkelhor's measure. Thus, support for acceptable reliability extends to a sizable portion of the studies under review.

The second issue concerns attenuating effects from low base rates. The more the split between CSA and control participants deviates from 50-50, the greater the attenuation in the CSA-symptom association will tend to be (cf. Rosenthal & Rosnow, 1991). This attenuation is quite small for a 27-73 split (e.g., female CSA), but it is somewhat larger for a 14-86 split (e.g., male CSA). However, 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 by .03 (based on formulas provided by Rosenthal & Rosnow, 1991), indicating that adjusted effect size estimates are still small in magnitude and are considerably smaller than the family environment-symptom effect size estimate of $r_u = .29$. From an empirical point of view, it is noteworthy that, in the current review, base rates were not positively related to effect size estimates, $r(48) = -.04$, $p > .70$, two-tailed, contrary to expectations that they would be.

Finally, the relevance of artificial dichotomization to the CSA variable is weakened by the fact that CSA has generally been conceptualized as a categorical rather than continuous variable (i.e., one experiences CSA or one does not). Nevertheless, despite this common conceptualization of CSA, several researchers have attempted to construct continuous measures of CSA and have used these measures to compare CSA with family environment in terms of their relative contribution to adjustment variance (e.g., Cole, 1988; Wisniewski, 1990). Wisniewski's severity score of CSA discussed previously is one example. For nonincestuous SA students who were not revictimized as adults, a path analysis revealed that family violence was related to current levels of emotional distress ($\beta = .13$), whereas CSA was not ($\beta = -.02$). Likewise, for incestuous CSA, family violence ($\beta = .27$) was related to emotional distress, but CSA was not ($\beta = -.01$). Cole constructed a severity index for CSA (composed of factors such as degree of invasiveness), which can also be viewed as a continuous measure of CSA. She found that CSA did not explain adjustment variance above and beyond that explained by various family environment factors in a hierarchical regression. It is important to note that a continuous measure for physical abuse, constructed similarly to the severity index for CSA, was entered along with CSA in the last step of the analysis; this family environment factor, but not CSA, accounted for additional adjustment variance. Results from these studies in which CSA was constructed to be continuous are consistent with results from studies in which CSA was treated dichotomously in terms of pointing to family environment, rather than CSA, as a significant contributor to current adjustment.

In sum, 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. The foregoing discussion suggests that reliability is not problematic and that attenuation due to low base rates is of very low magnitude because effect size estimates were small to begin with. In a similar vein, attenuation due to dichotomization, if artificial, would also be of very low magnitude because of the small effect size estimates that were obtained (cf. Glass & Hopkins, 1996). Empirically, low base rates were not associated with lower effect size estimates, and CSA was relatively unimportant compared with family environment when CSA was treated as a continuous variable. These considerations support the validity of comparing CSA-symptom and family environment-symptom relations and of assessing CSA-symptom relations when controlling for family environment. Nevertheless, precise, as opposed to relative, estimates of the contributions of CSA and family environment to adjustment may be somewhat problematic because of the possi-

bility of low magnitude attenuations of CSA-symptom relations.

Discussion

Commonly expressed opinions, both lay and professional, have implied that CSA possesses four basic properties: causality (it causes harm), pervasiveness (most SA persons are affected), intensity (harm is typically severe), and gender equivalence (boys and girls are affected equally). Qualitative and quantitative literature reviews of CSA have offered mixed conclusions regarding these properties but have suffered from various shortcomings. Problems in qualitative reviews have generally included sampling bias (i.e., overreliance on clinical and legal samples), subjectivity, and imprecision. Quantitative reviews have included larger proportions of nonclinical and nonlegal samples, reduced subjectivity, and increased precision and indicate that the intensity of CSA effects or correlates is of low magnitude in the general population. These reviews, however, have offered less clarification regarding issues of causality, pervasiveness, and gender equivalence. To address the shortcomings of the qualitative and quantitative reviews, we reviewed the CSA literature based on college samples. The advantages of this literature were (a) it contains the largest set of studies conducted on nonclinical and nonlegal populations; (b) it offers the most extensive database on moderating influences (e.g., family environment), useful for examining the issue of causality; (c) it provides a large number of male samples, facilitating gender comparisons; and (d) it provides a large database on self-reported reactions and effects, enabling examination of the pervasiveness of negative outcomes.

Review of the college samples revealed that 14% of college men and 27% of college women reported events classifiable as CSA, according to the various definitions used. Results from the college data do not support the commonly assumed view that CSA possesses the four basic properties outlined previously. CSA was associated with poorer psychological adjustment across the college samples, but the magnitude of this association (i.e., its intensity) was small, with CSA explaining less than 1% of the adjustment variance. Further, this small association could not be attributed to CSA for several reasons: (a) family environment was confounded with CSA, (b) family environment predicted adjustment problems better than CSA by a factor of nine, and (c) statistical control tended to eliminate significant relations between CSA and adjustment. Results also revealed that lasting negative effects of CSA were not pervasive among SA students, and that CSA was not an equivalent experience for men and women. These results imply that, in the college population, CSA does not produce pervasive and intensely negative effects regardless of gender. Therefore, the commonly assumed view that CSA possesses basic properties regardless of population of interest is not supported. These findings are consistent with Constantine's (1981, p. 238) conclusion that CSA has "no inbuilt or inevitable outcome or set of emotional reactions" associated with it. It is important to add that analysis at the population level estimates the typical case and therefore obscures individual cases. That is, the findings of the current review should not be construed to imply that CSA never causes intense harm for men or women—clinical research has well

documented that in specific cases it can. What the findings do imply is that the negative potential of CSA for most individuals who have experienced it has been overstated.

The validity of using studies based on the college population to assess characteristics of CSA in the general population is of particular concern. Objections to such an approach have included claims that SA college students may be too young for symptoms to appear, typically experience less severe forms of CSA and consequently are less harmed, or are better able to cope with their experiences than persons in the general population (e.g., Briere, 1988; Jumper, 1995; Pallotta, 1992). Evidence from the current review of similarities in CSA between the college and general populations, however, contradicts these views. Compared with SA persons in national samples, SA college students experienced intercourse, close family CSA, and multiple incidents of CSA just as often, and the overall prevalence of CSA was not lower in the college samples. 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, $r_u = .10$ for women. Thus, college students do not appear to present fewer symptoms, experience less severe CSA, or show better coping. Against claims that college students may be too young for symptoms to manifest, Neumann et al. (1996) found that persons under 30 years of age and over 30 years of age did not differ in CSA-adjustment relations, and age also failed to moderate CSA-adjustment relations in the current review. These results demonstrate the relevance of college data to CSA in the broader population and point to the value of using the college data to evaluate the commonly assumed properties of causality, pervasiveness, intensity, and gender equivalence.⁵

The Four Assumed Properties of CSA Revisited

Gender Equivalence

The gender differences found in current adjustment, retrospectively recalled immediate reactions, current reflections, and self-reported effects demonstrate that the experience of CSA is not comparable for men and women, at least among those who go on to attend college. The relation between CSA and adjustment problems was generally stronger for women than men. Two thirds of male CSA experiences, but less than a third of female CSA experiences, were reported not to have been negative at the time. Three of every eight male experiences, but only one of every 10 female experiences, were reported to have been positive at the time. Patterns for current reflections about these events were similar. The magnitude of gender differences in self-

⁵ Despite all the empirically based similarities between the college and national populations, it is tempting to speculate that certain differences exist. Persons with extremely harmful CSA episodes may be unable to attend college or remain there once they have begun. In this way, surveys of college students may miss extreme cases of CSA, limiting the generalizability of findings from the college population. Nevertheless, the results of the current review, while not demonstrating equivalence between the two populations, strongly suggest that the gulf between them is narrow, and much narrower than child abuse researchers have generally acknowledged.

reported effects was virtually identical in the college samples in the current review ($r_u = .22$) and in the national samples ($r_u = .23$) examined by Rind and Tromovitch (1997), which lends further support to the relevance of the college data to the general population.

A number of researchers have commented on differences in male and female reactions to CSA. Schultz and Jones (1983) noted that men tended to see these sexual experiences as an adventure and as curiosity-satisfying, whereas most women saw it as an invasion of their body or a moral wrong. Fritz et al. (1981) made nearly identical observations. West and Woodhouse (1993), comparing their male sample with Nash and West's (1985) female sample, observed that women's remembered reactions at the time were "predominantly of fear, unpleasant confusion, and embarrassment . . . [while men's] remembered reactions were mostly either indifference, tinged perhaps with slight anxiety, or of positive pleasure, the latter being particularly evident in contacts with the opposite sex" (p. 122). These gender differences in reactions to CSA experiences are consistent with more general gender differences in response to sex among young persons. For example, boys and girls report very different reactions to their first experience of sexual intercourse (Sorensen, 1973), with girls predominantly reporting negative reactions such as feeling afraid, guilty, or used, and boys predominantly reporting positive reactions such as feeling excited, happy, and mature. These differences are likely due to an interaction between biologically based gender differences and social learning of traditional sex roles (Fischer & Lazerson, 1984). Researchers (e.g., Kinsey et al., 1948; Sorensen, 1973) have repeatedly reported that boys are more sexually active than girls, masturbate more frequently, and require less physical stimulation for arousal. Social norms tend to encourage sexual expression in adolescent boys but have traditionally emphasized romance and nurturance in girls (Fischer & Lazerson, 1984). Thus, it is unsurprising that men and women should show similar differences in their reactions to CSA.

It is important to add that men and women may react differently to CSA experiences because they tend to experience different kinds of CSA. For example, Baker and Duncan (1985) commented that girls in their national survey in Great Britain may have found their CSA experiences to be more damaging than boys did because they had more intrafamilial CSA and experienced CSA at younger ages. In the current review, college men and women also 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.

It is important to note that the separate meta-analyses of the four Gender \times Consent combinations revealed a stronger association between CSA and adjustment problems for women than for men when all levels of consent were considered, but not when unwanted sex only was contrasted. These findings suggest that some types of CSA (e.g., unwanted experiences) are equivalent between the genders, but that other types (e.g., willing) may not be. The overall difference between male and female college students in the CSA-adjustment relationship is not surprising, because men experienced coercion less frequently than women. The CSA-adjustment results, however, reflect both the effects of CSA and of confounding variables. For this reason, the self-reported reactions and effects data are

valuable as direct measures of impact. These data point to gender nonequivalence but must be qualified because of potential biases in recalling past events. Nevertheless, the two sets of analyses converge to suggest that when using current sociolegal definitions for CSA, which include both unwanted and willing experiences, men and women are not equivalent in their reactions and outcomes.

Causality

Two approaches were used to examine whether poorer adjustment for CSA students compared with control students reflected the effects of CSA. First, examination of the interrelations among CSA, adjustment, and family environment revealed that weighted mean effect sizes for CSA-adjustment, CSA-family environment, and family environment-adjustment relations were $r_u = .09$, $.13$, and $.29$, respectively. The finding that family environment was confounded with CSA and explained nine times more adjustment variance than did CSA is consistent with the possibility that the CSA-adjustment relation may not reflect genuine effects of CSA. Second, analysis of studies that used statistical control further supported the possibility that many or most CSA-symptom relations do not reflect true effects of CSA, because most CSA-adjustment relations became nonsignificant under statistical control.

Some researchers (Briere, 1988; Briere & Elliott, 1993) have questioned the validity of statistically controlling for family environment when examining CSA-adjustment relations, arguing that such analyses may be invalid when the control variable (e.g., family environment) is unreliable, the sample size is small, the causal relationship between the control and CSA variables is unknown, or the sample underrepresents abuse severity. These concerns do not appear to be problematic in the current review. Whether measured by standard instruments or by author-written items, family environment was reliably related to adjustment. Sample sizes were not small in the studies using control ($M = 309$, $SD = 173$). Regarding the direction of causality, Ageton's (1988) national sample showed that family problems preceded, rather than followed, CSA. Burnam et al. (1988), using the same large community sample as Stein et al. (1988), found that SA persons tended to be symptomatic both before and after experiencing CSA. These researchers noted that a third variable such as family or community environment might have been responsible for both the CSA and the adjustment problems. Pope and Hudson (1995) detailed the potential role of third variables in accounting for obtained CSA-adjustment associations (e.g., genetic factors can both predispose individuals to adjustment problems and make them vulnerable to CSA events). CSA may be most likely to cause family dysfunction when it is incestuous; when it is extrafamilial, however, then family dysfunction may contribute to CSA by making children vulnerable to this experience (Briere & Elliott, 1993).⁶ In clini-

⁶ It is important to note that, under certain circumstances, extrafamilial CSA may be likely to affect adversely family functioning, as in cases where CSA episodes become known to the family and to the police. In this situation, tension may arise in the family, representing secondary consequences of the CSA (cf. Baumann, 1983). Most commonly, however, CSA episodes do not come to the attention of the family or police; for example, Laumann et al. (1994), in their national probability sample, found that only 22% of their SA respondents ever told anyone. Addition-

cal studies, which often include high proportions of patients with incestuous CSA, causality is therefore more problematic. In the college samples, however, close family CSA was the exception, not the rule. Only 16% of SA students had close family CSA; the percentage of cases of paternal incest is even lower because the overall value includes sibling incest. These considerations do not prove causal direction in the college population but suggest that in most cases the direction is more likely to go from family environment to CSA. Finally, the college samples did not underrepresent abuse severity. Compared with the general population, as indicated by studies based on national samples, SA students experienced as much intercourse, close family CSA, and multiple episodes of CSA; moreover, college students were just as likely to have experienced CSA as persons in the general population. Briere's arguments seem most appropriate for clinical samples with large proportions of incest cases. In this situation, Briere's (1988, p. 84) argument that "abuse without family dysfunction may have little construct validity" may be applicable; in the general population and in the college population, however, this argument is less valid. These considerations support the validity of using statistical control in the studies under review.

Aside from validity issues, however, the statistical control analyses do not rule out causality for several reasons. First, in a minority of cases, CSA-symptom relations remained significant after statistical control. Second, when nonsignificance did result from statistical control, low power rather than a zero effect may have been responsible. Third, a small minority of students with a history of CSA did report self-perceived lasting harm, implying genuine negative effects of CSA for these persons. Fourth, for male participants, unwanted CSA was associated with greater symptomatology. If unwanted CSA had been contrasted with willing CSA only, instead of a combination of unwanted and willing CSA, then consent would likely have moderated CSA-symptom relations more strongly. These results suggest that unwanted CSA does have negative effects, although confounding variables must still be considered. Despite these caveats, the current results imply that the claim that CSA inevitably or usually produces harm is not justified.

The finding that family environment is more important than CSA in accounting for current adjustment in the college population is consistent with the results of several recent studies using participants from noncollege populations (e.g., Eckenrode et al., 1993; Ney et al., 1994). Eckenrode et al. categorized children and adolescents obtained from a large representative community sample in a small-sized city in New York state into six groups: not abused, CSA, physical abuse, neglect, CSA and neglect, and physical abuse and neglect. They found that SA children and adolescents performed as well in school as non-abused controls in all areas measured, including standardized test scores, school performance, and behavior. Neglect and physical abuse, on the other hand, were associated with poorer performance and more behavior problems. Ney et al. (1994) sepa-

rated their mostly clinical sample of children and adolescents into categories of CSA, physical abuse, physical neglect, verbal abuse, emotional neglect, and combinations of these. They found that the combination of abuse that correlated most strongly with adjustment problems was physical abuse, physical neglect, and verbal abuse. In the top 10 worst combinations, verbal abuse appeared seven times, physical neglect six times, physical abuse and emotional neglect five times each, whereas CSA appeared only once.

The greater importance of nonsexual negative childhood experiences in explaining later adjustment was clearly demonstrated in a study of a large, representative sample of female college students throughout the United States. Wisniewski (1990) used path analyses to assess the relative contributions of CSA and family environment to current adjustment. She concluded that the data did not support CSA "as a specific explanation of current emotional distress [but instead are] best interpreted as supportive of other factors such as family violence . . . as having the greatest impact" (p. 258). Other researchers who used college samples and used statistical control reached similar conclusions regarding the role of family violence, rather than CSA, in explaining current adjustment problems (e.g., Higgins & McCabe, 1994; Pallotta, 1992). One reason CSA may have been overshadowed by other childhood experiences such as verbal and physical abuse in explaining adjustment is that participants may have experienced the latter type of events more frequently than CSA. Nevertheless, the results from these studies highlight the relatively greater importance of family environment compared with CSA in accounting for adjustment problems—a point that has been ignored or underemphasized in much of the CSA literature to date.

Pervasiveness and Intensity of Negative Effects or Correlates

Self-reported effects from CSA revealed that lasting psychological harm was uncommon among the SA college students. Perceived temporary harm, although more common, was far from pervasive. In short, the self-reported effects data do not support the assumption of wide-scale psychological harm from CSA. This conclusion is further suggested by students' self-reported reactions. The finding that two thirds of SA men and more than one fourth of SA women reported neutral or positive reactions is inconsistent with the assumption of pervasive and intense harm. It is not parsimonious to argue that boys or girls who react neutrally or positively to CSA are likely to experience intense psychological impairment. To argue that positive or neutral reactions are consistent with intense harm, it seems logical to first demonstrate that negative reactions are consistent with intense harm. However, the magnitude of the CSA-adjustment relation was small for women, despite the reporting of negative reactions by a majority of SA women. This low intensity finding for generally negative CSA experiences is inconsistent with an expectation of intense harm from nonnegative CSA experiences.

Moderators

Multiple regression analyses showed that the intensity of the relationship between CSA and adjustment varied reliably as a function of gender, level of consent, and the interaction of these

ally, it should be noted, because of its salience, the revelation, or even fear of revelation, of CSA events may inflate a SA person's perception of negative aspects of family environment, particularly in retrospective measures.

two factors. It is noteworthy that neither the level of contact nor the interaction between gender and level of contact was related to intensity. These latter results failed to provide support for the common belief that contact sex is more harmful than noncontact sex or that contact sex for girls is especially harmful. These conclusions, however, should be viewed cautiously because of the overlapping nature of the two levels of the contact variable (i.e., contact only versus contact and noncontact sex). This same caveat applies to consent because its two levels (unwanted versus willing and unwanted) were overlapping as well. The finding that most women (72%) reacted negatively to their CSA at the time it occurred implies that most of this CSA was unwanted and that the overlap between the two levels of consent was high. Thus, even though consent did not moderate intensity for women, a true difference as a function of consent may have been obscured. The finding that level of consent did moderate intensity for men is consistent with less overlap between the two levels of consent for men, because the majority of men (67%) reacted nonnegatively at the time. Importantly, CSA was not related to adjustment for men in the willing and unwanted level of the consent variable.

In separate moderator analyses, we examined how aspects of the CSA experience moderated self-reported reactions and effects, as well as symptoms. Although these results should be viewed cautiously because they were usually based on a small number of samples, we found that only force and incest moderated outcomes. The largest relation occurred between force and self-reported reactions or effects, but force was unrelated to symptoms. Incest moderated both symptoms and self-reported reactions and effects. Penetration, duration, and frequency did not moderate outcomes. The near-zero correlation between penetration and outcome is consistent with the multiple regression analysis finding that contact sex did not moderate adjustment. This result provides empirical support for Finkelhor's (1979, p. 103) observation that our society's view of intercourse as the most damaging form of CSA is "a well-ingrained prejudice" unsupported by research. Composite measures consisting of various combinations of moderators (e.g., incest, force, penetration) showed no association with symptoms in four of five studies that constructed such measures. This finding is consistent with Laumann et al.'s (1994) failure to find an association between their composite variable (consisting of penetration, number of older partners—abusers, relatedness of partner—abuser, frequency of contacts, age when having contacts, duration of contacts) and adjustment for SA respondents in their study of a U.S. national sample. It is important to note, however, that these nonsignificant results may be attributable to the additive nature of the composite variables. Composites based on two-way or higher order interactions of moderators might have been more likely to yield significant results, particularly if the interactions included incest and force.

Child Sexual Abuse as a Construct Reconsidered

In light of the current findings, it is appropriate to reexamine the scientific validity of the construct of CSA as it has been generally conceptualized. In most studies examined in the current review, CSA was defined based on legal and moral, rather than empirical and phenomenological, criteria. This approach

may form a defensible rationale for legal restrictions of these behaviors, but is inadequate and may be invalid in the context of scientific inquiry (Okami, 1994). In science, *abuse* implies that particular actions or inactions of an intentional nature are likely to cause harm to an individual (cf. Kilpatrick, 1987; Money & Weinrich, 1983). Classifying a behavior as abuse simply because it is generally viewed as immoral or defined as illegal is problematic, because such a classification may obscure the true nature of the behavior and its actual causes and effects.

The history of attitudes toward sexuality provides numerous examples. Masturbation was formerly labeled "self-abuse" after the 18th century Swiss physician Tissot transformed it from a moral to a medical problem (Bullough & Bullough, 1977). From the mid-1700s until the early 1900s the medical profession was dominated by physicians who believed that masturbation caused a host of maladies ranging from acne to death (Hall, 1992; Money, 1985), and medical pronouncements of dangerousness were accompanied by moral tirades (e.g., Kellogg, 1891). This conflation of morality and science hindered a scientifically valid understanding of this behavior and created iatrogenic victims in the process (Bullough & Bullough, 1977; Hall, 1992; Money, 1985). Kinsey et al. (1948) argued that scientific classifications of sexual behavior were nearly identical with theological classifications and the moral pronouncements of English common law in the 15th century, which were in turn based on medieval ecclesiastic law, which was itself built on the tenets of certain ancient Greek and Roman cults and Talmudic law. Kinsey et al. noted that "[e]ither the ancient philosophers were remarkably well-trained psychologists, or modern psychologists have contributed little in defining abnormal sexual behavior" (p. 203). Behaviors such as masturbation, homosexuality, fellatio, cunnilingus, and sexual promiscuity were codified as pathological in the first edition of the American Psychiatric Association's (1952) *Diagnostic and Statistical Manual of Mental Disorders*. The number and variety of sexual behaviors labeled pathological has decreased, but mental health professionals continue to designate sexual behaviors as disorders when they violate current sexual scripts for what is considered acceptable (Levine & Troiden, 1988). This history of conflating morality and law with science in the area of human sexuality by psychologists and others indicates a strong need for caution in scientific inquiries of sexual behaviors that remain taboo, with child sexual abuse being a prime example (Rind, 1995).

As discussed previously, abuse implies that harm is likely to result from a behavior. The results for SA male college students, using this scientific conceptualization of abuse, highlight the questionable validity of the construct CSA as defined and used in the studies examined in the current review. For these male college students, 37% viewed their CSA experiences as positive at the time they occurred; 42% viewed these experiences as positive when reflecting back on them; and in the two studies that inquired about positive self-perceived effects, 24% to 37% viewed their CSA experiences as having a positive influence on their current sex lives. Importantly, SA men across all levels of consent (i.e., both willing and unwanted experiences) did not differ from controls in current psychological adjustment, although SA men with unwanted experiences only did, implying that willingness was associated with no impairment to psychological adjustment. The positive reports of reactions and effects,

along with normal adjustment for willing participants, are scientifically inconsistent with classifying these male students as having been abused. Their experiences were not associated with harm, and there appears to be no scientific reason to expect such an association (i.e., predicting psychologically harmful effects from events that produced positive reactions lacks face validity). On the other hand, a minority of SA men did report retrospectively recalled negative reactions, negative current reflections, and negative self-perceived effects; moreover, unwanted CSA was associated with adjustment problems. Assuming that negative reactions were associated with unwanted CSA, the term *abuse* may be scientifically valid for the latter students. Combining positive and negative responders into a single category of abuse may incorrectly suggest harm for the former and simultaneously dilute harm for the latter (Bauserman & Rind, 1997).

Some researchers have questioned their original definitions of sexual abuse after assessing their results. For example, Fishman (1991) borrowed from Finkelhor's (1979) definition to classify sexual abuse of boys mostly on the basis of age discrepancies (i.e., sex between a boy of 12 or less and someone at least 5 years older, or between a boy aged 13 to 16 with someone at least 10 years older), stating that age differences implied sufficient discrepancy in developmental maturity and knowledge to indicate victimization. He found that SA men in his study did not differ from controls on measures of adjustment and reported a wide range of reactions to and effects from their CSA experiences (mostly positive or neutral). In-depth interviews confirmed and elaborated the quantitative findings, leading Fishman to question his original assumptions. He noted that the men's stories altered his universal beliefs about the impact of inappropriate sexual experiences on children, and stated that "to impose a confining definition onto someone's experience does nothing to alter the realities of that experience for the person" (pp. 284–285). Fishman concluded by arguing for the use of language of a more neutral nature rather than labels such as abuse, victim, and molestation—in short, for use of empirical and phenomenological criteria in conceptualizing early sexual relations, rather than legal or moral criteria.

The foregoing discussion does not imply that the construct CSA should be abandoned, but only that it should be used less indiscriminately to achieve better scientific validity. Its use is more scientifically valid when early sexual episodes are unwanted and experienced negatively—a combination commonly reported, for example, in father–daughter incest.⁷ In general, findings from the current review suggest that sociolegal definitions of CSA have more scientific validity in the case of female children and adolescents than for male children and adolescents, given the higher rate of unwanted negative experiences for women. Nevertheless, as Long and Jackson (1993) argued, because some women perceive their early experiences as positive, do not label themselves as victims, and do not show evidence of psychological impairment, it is important for researchers to be cautious in defining abuse for both men and women in attempts to validly examine the antecedents and effects of these experiences.

Summary and Conclusion

Beliefs about CSA in American culture center on the viewpoint that CSA by nature is such a powerfully negative force

that (a) it is likely to cause harm, (b) most children or adolescents who experience it will be affected, (c) this harm will typically be severe or intense, and (d) CSA will have an equivalently negative impact on both boys and girls. Despite this widespread belief, the empirical evidence from college and national samples suggests a more cautious opinion. Results of the present review do not support these assumed properties; CSA does not cause intense harm on a pervasive basis regardless of gender in the college population. The finding that college samples closely parallel national samples with regard to prevalence of CSA, types of experiences, self-perceived effects, and CSA–symptom relations strengthens the conclusion that CSA is not a propertied phenomenon and supports Constantine's (1981) conclusion that CSA has no inbuilt or inevitable outcome or set of emotional reactions.

An important reason why the assumed properties of CSA failed to withstand empirical scrutiny in the current review is that the construct of CSA, as commonly conceptualized by researchers, is of questionable scientific validity. Overinclusive definitions of abuse that encompass both willing sexual experiences accompanied by positive reactions and coerced sexual experiences with negative reactions produce poor predictive validity. To achieve better scientific validity, a more thoughtful approach is needed by researchers when labeling and categorizing events that have heretofore been defined sociolegally as CSA (Fishman, 1991; Kilpatrick, 1987; Okami, 1994; Rind & Bauserman, 1993).

One possible approach to a scientific definition, consistent with findings in the current review and with suggestions offered by Constantine (1981), is to focus on the young person's perception of his or her willingness to participate and his or her reactions to 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 experienced negative reactions to it, then *child sexual abuse*, a term that implies harm to the individual, would be valid. Moreover, the term *child* should be restricted to nonadolescent children (Ames & Houston, 1990). Adolescents are different from children in that they are more likely to have sexual interests, to know whether they want a particular sexual encounter, and to resist an encounter that they do not want. Furthermore, unlike adult–child sex, adult–adolescent sex has been commonplace cross-culturally and historically, often in socially sanctioned forms, and may fall within the "normal" range of human sexual behaviors (Bulough, 1990; Greenberg, 1988; Okami, 1994). A willing encounter between an adolescent and an adult with positive reactions on the part of the adolescent would then be labeled scientifically as *adult–adolescent sex*, while an unwanted encounter with negative reactions would be labeled *adolescent sexual abuse*. By drawing these distinctions, researchers are likely to achieve

⁷ Two of the three outliers identified in the sample-level meta-analysis involved samples consisting largely of incest cases (Jackson et al., 1990; Roland et al., 1989). The CSA experiences of these women, associated with relatively large effect sizes, may capture more accurately the essence of abuse in a scientific sense—that is, more persuasive evidence of harm combined with the likely contextual factors of being unwanted and perceived negatively.

a more scientifically valid understanding of the nature, causes, and consequences of the heterogeneous collection of behaviors heretofore labeled CSA.

Finally, it is important to consider implications of the current review for moral and legal positions on CSA. If it is true that wrongfulness in sexual matters does not imply harmfulness (Money, 1979), then it is also true that lack of harmfulness does not imply lack of wrongfulness. Moral codes of a society with respect to sexual behavior need not be, and often have not been, based on considerations of psychological harmfulness or health (cf. Finkelhor, 1984). Similarly, legal codes may be, and have often been, unconnected to such considerations (Kinsey et al., 1948). In this sense, the findings of the current review do not imply that moral or legal definitions of or views on behaviors currently classified as CSA should be abandoned or even altered. The current findings are relevant to moral and legal positions only to the extent that these positions are based on the presumption of psychological harm.

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(Appendix follows)

Appendix
 Definitions of Child Sexual Abuse (CSA), Prevalence Rates, and Sample-Level Effect Sizes (i.e., Psychological Correlates)
 in Studies Using College Student Samples

Study	Gender	Operational definition of CSA ^a	Prevalence ^b		Sample-level effect sizes ^c	
			N	CSA	N	r
Alexander & Lupfer (1987)	F	U C (not specified), relative	586	25%	431	.02
Bailey & Gibbons (1989)	F	a ? self-labeled as "sexually molested"	294	13%	294	.04
Beckman & Burns (1990)	F	a b <12 w/ "adult"	198	10%	182	.04
Bendixen, Muus, & Schei (1994)	F	U b <18	510	19%	510	.15
Bendixen, Muus, & Schei (1994)	M	U b <18	486	3%	486	.08
Bergdahl (1983)	F	a b <18 w/ "adult"	430	36%	355	.11
Briere & Runtz (1988a, 1988b, 1990)	F	a C <15 w/ 5+	278	15%	224	.12
Brubaker (1991)	F	a C <16 w/ 5+	284	18%	155	.13
Brubaker (1994)	F	a C <16 w/ 5+	400	25%	—	.10 ^d
Cole (1988)	F	a C <18 w/ 5+; unwanted peer	2,740	21%	128	—
Cole (1988)	M	a C <18 w/ 5+; unwanted peer	2,279	17%	—	—
Collings (1995)	M	U b <18	284	29%	284	.17
Condy et al. (1987)	M	a C <16 w/ 5+ or 16 or over	359	16%	—	.17
Edwards & Alexander (1992)	F	a C <16 w/ 5+ or force; 16-18 w/ 10+ or unwanted	103	44%	97	.14
Everill & Waller (1995)	F	U b <18	69	71%	69	.09
Finkelhor (1979, 1984)	F	a b <13 w/ ≥16; 13-16 w/ 10+ (relative or unwanted)	530	19%	536	.11
Finkelhor (1979, 1984)	M	a b <13 w/ ≥16; 13-16 w/ 10+ (relative or unwanted)	266	9%	260	.12
Fischer (1991)	F	a b <puberty w/ 4+	325	19%	—	.12
Fischer (1991)	M	a b <puberty w/ 4+	327	7%	—	.12
Fishman (1991)	M	a b <13 w/ 5+; 13-16 w/ 10+ or unwanted	148	18%	148	-.04
Fritz, Stoll, & Wagner (1981)	F	a C <puberty w/ "post-adolescent"	540	8%	—	.11
Fritz, Stoll, & Wagner (1981)	M	a C <puberty w/ "post-adolescent"	412	5%	—	.11
Fromuth (1984, 1986)	F	a b <13 w/ 5+ or 16 or over; 13-16 w/ 10+	482	22%	383	.08
Fromuth & Burkhardt (1989) [MW]	M	a b <13 w/ 5+ or 16 or over; 13-16 w/ 10+	253	15%	249	.11
Fromuth & Burkhardt (1989) [SE]	M	a b <13 w/ 5+ or 16 or over; 13-16 w/ 10+	329	13%	324	.00
Gidycz et al. (1993)	F	a b <13 w/ 5+ or 16 or over; 13-16 w/ 10+	857	54%	831	.15
Gidycz, Hanson, & Layman (1995)	F	a b <14	796	59%	159	.11
Goldman & Goldman (1988)	F	a b <13 w/ 5+; 13-16 w/ 10+	603	28%	—	.11
Goldman & Goldman (1988)	M	a b <13 w/ 5+; 13-16 w/ 10+	388	9%	—	.11
Greenwald (1994)	F	a b <17 w/ 5+; ≥17 w/ 10+; unwanted; intrafamilial	214	41%	214	.03
Greenwald (1994)	M	a b <17 w/ 5+; ≥17 w/ 10+; unwanted; intrafamilial	213	32%	213	-.09
Harter, Alexander, & Neimeyer (1988)	F	a C <18 w/ relative who was 5+	1,066	13%	85	.16
Hatfield (1988)	M	a C <14 w/ 5+	213	12%	213	.06
Haugaard & Emery (1989)	F	a b <16 w/ 5+ or 16 or over	672	12%	186	.11 ^e
Haugaard & Emery (1989)	M	a b <16 w/ 5+ or 16 or over	420	5%	—	.16
Higgins & McCabe (1994)	F	a b <13 w/ 5+; 13-18 w/ 10+	199	24%	199	.05
Hrabowy (1987)	F	U b <16	383	28%	383	.36
Jackson et al. (1990)	F	a C <18 w/ relative who was 5+	40	n/a	40	.08
Kinzl et al. (1994, 1995)	F	a b <18, unwanted peer, sex now considered abuse	202	22%	201	.08

Appendix (continued)

Study	Gender	Operational definition of CSA ^a	Prevalence ^b		Sample-level effect sizes ^c	
			N	CSA	N	r
Klein-Trull (1990)	F	a C <17 w/ 5+ or unwanted	271	11%	58	.19
Lam (1994)	F	a b <14 w/ adult or unwanted	264	18%	260	.01
Landis (1956)	F	a b through adolescence w/ older partner	1,029	35%	—	— ^d
Landis (1956)	M	a b through adolescence w/ older partner	467	30%	—	— ^d
Long (1993)	F	a b <13 w/ 3+, authority figure	305	18%	305	.10
Long & Jackson (1993)	F	a b <13 w/ 5+; 13-16 w/ 10+	137	n/a	—	— ^d
Maggio (1984)	F	a b <17 w/ 6+	244	35%	477	-.02 ^e
Maggio (1984)	M	a b <17 w/ 6+	233	36%	—	— ^d
Moor (1992)	F	U b <17 w/ 5+	437	18%	321	.16
Nash & West (1985)	F	U b <17 w/ 5+	92	54%	—	— ^d
O'Neill (1991)	F	a b <13 w/ 5+, 13-16 w/ 10+	365	17%	—	— ^d
O'Neill (1991)	M	a b <13 w/ 5+, 13-16 w/ 10+	206	17%	—	— ^d
Pallotta (1992)	F	a C <13 w/ 5+; 13-16 w/ 10+; <17 unwanted relative	275	20%	257	.13
Peters & Range (1995)	F	U b <12 w/ 5+	135	20%	266	.11 ^f
Peters & Range (1995)	M	U b <12 w/ 5+	131	8%	—	—
Pizzolo (1990)	F	a b <13 w/ 5+; 13-16 w/ 10+; relative, unwanted peer	308	23%	298	.13
Predieri (1992)	M	a b <13 w/ 5+; 13-16 w/ 8+; unwanted peer	557	6%	62	.05
Preuss (1988)	F	a b <13 w/ 5+; 13-17 w/ 10+; force	402	50%	402	.05
Preuss (1988)	M	a b <13 w/ 5+; 13-17 w/ 10+; force	288	20%	288	.04
Rau (1994)	M	U b <12 w/ 5+; 12-16 w/ 10+	60	n/a	60	.10
Rew, Esparza, & Sands (1991)	F	U b <18 w/ older partner	111	50%	111	.11
Rew, Esparza, & Sands (1991)	M	U b <18 w/ older partner	160	23%	160	.21
Risin & Koss (1987)	M	a b <13 w/ 5+; 13 w/ 8+; unwanted peer	2,922	7%	—	— ^d
Roland, Zellhart, & Dubes (1989)	F	U b <pubescence w/ 5+	171	30%	171	.40
Sarbo (1985)	F	a b <12 w/ ≥16; <16 w/ relative ≥16; unwanted	154	40%	154	.02
Sarbo (1985)	M	a b <12 w/ ≥16; <16 w/ relative ≥16; unwanted	112	22%	112	.05
Schultz & Jones (1983)	F	a b <12 w/ 16 or over	n/a	n/a	—	— ^d
Schultz & Jones (1983)	M	a b <12 w/ 16 or over	n/a	n/a	—	— ^d
Sedney & Brooks (1984)	F	a b "while growing up"	301	17%	102	.15
Silliman (1993)	F	? ? (not specified)	66	n/a	66	-.25
Smolak, Levine, & Sullins (1990)	F	a b <13 w/ 5+; 13-16 w/ 10+	298	23%	269	.12
Urquiza (1989)	M	a C <18 w/ 5+	2,016	17%	88	.16
West & Woodhouse (1993)	M	a b <11 w/ 16 or over; 11-16 w/ 18 or over	182	37%	—	— ^d
White & Strange (1993)	F	U b <17 w/ 18 or over who was 5+	131	14%	105	-.01
Wisniewski (1990)	F	a b <14 w/ 5+; unwanted	3,187	29%	3,187	.11
Yama et al. (1992, 1993)	F	a b <13 w/ 5+; 13-16 w/ 10+	420	10%	156	.21
Zetzer (1991)	F	U b <18; relative	338	64%	338	.02

^a In column 1: U = only unwanted sex included in definition; a = all types of sex, unwanted and willing, included. In column 2: C = only physical contact sex included in definition; b = both contact and noncontact experiences included. Next, upper age of "child" is given first; then, minimum age of other person is given (e.g., w/ 5+ means with someone at least 5 years older); last, other conditions for CSA are given. ^b Ns are number of subjects used for prevalences; may be different from effect size Ns. Under CSA, percentage of sample with CSA experiences is provided. ^c Ns are number of subjects in analyses of psychological correlates of CSA; rs are the sample-level effect sizes. "Reaction data only" indicates data were available only for self-reported reactions or effects. ^d Dashes indicate that an effect size for psychological correlates was not computable—only data for self-reported reactions or effects were provided. ^e Represents N and r for female and male students combined; results were not reported separately, thus the dashes on the next row.