The Long-Term Sequelae of Childhood Sexual Abuse in Women: A Meta-Analytic Review

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The authors conducted a meta-analytic review of the relationship between a history of child sexual abuse (CSA) and psychological problems in adult women in 38 studies meeting rigorous research criteria. Across all symptoms, a significant association was found between history of CSA and adult symptomatology. Analysis of the role of moderating variables indicated the associations were stronger among subjects recruited from clinical populations. When individual symptom domains were examined, anxiety, anger, depression, revictimization, self-mutilation, sexual problems, substance abuse, suicidality, impairment of self-concept, interpersonal problems, obsessions and compulsions, dissociation, posttraumatic stress responses, and somatization all yielded significant associations with sexual abuse. These results are discussed in light of their relevance to research methodology and clinical intervention.

Early studies asserted that sexual contact between adults and children is not harmful (Bender & Blau, 1937; Bender & Grugett, 1952). In contrast, most empirical studies in the last 20 years have documented a wide variety of negative symptoms among women who were sexually abused as children.

The research paradigms used in contemporary sexual abuse studies vary tremendously, although most are based on samples of adults who are asked to retrospectively report their childhood histories of sexual events. Samples of women examined in current research include college students (e.g., Fromuth, 1986), professional women (e.g., Elliott & Briere, 1992a), psychiatric inpatients (e.g., Bryer, Nelson, Miller, & Krol, 1987), and psychotherapy clients (e.g., Tsai, Feldman-Summers, & Edgar, 1979). The methods used to gather information on child sexual abuse histories include direct clinical interviews (e.g., Herman & Hirschman, 1981), anonymous self-report surveys (e.g., Briere & Runtz, 1990), and chart reviews (e.g., Briere & Zaidi, 1989). Studies also vary in how they define child sexual abuse (CSA), especially in terms of the maximal age of the victim. Finally, methodological problems arise because many of these studies rely on retrospective recall of the victim; this process can be biased by the effects of amnesia (Briere & Conte, 1993), psychological distress (Briere, 1992b), and selective nondisclosure (Femina, Yeager, & Lewis, 1990).

As a consequence of variation in samples and methodologies, the conclusions that can be drawn from single empirical studies are constrained. For example, a study frequently cited as affirming that adult sexual abuse survivors are likely to experience sexual dysfunctions examined a sample of 10 women, all of whom were sexually molested under explicit threat of violence or force (Becker, Skinner, Abel, & Treacey, 1982). Another study frequently cited to support an association between CSA and adult sexual dysfunction was based on comparisons between a clinical group of CSA survivors and a nonclinical group of nonvictims (Tsai et al., 1979). In this research, abuse history was confounded with clinic patient status; consequently, it cannot be known whether CSA was specifically related to sexual dysfunction.

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Despite such interpretive difficulties, several qualitative reviews conclude that CSA is associated with significant negative long-term sequelae (e.g., Alter-Reid, Gibbs, Lachenmeyer, Sigal, & Massoth, 1986; Beitchman et al., 1992; Briere & Runtz, 1991; Browne & Finkelhor, 1986). But their focus, their consideration of moderating factors, and some of the conclusions they draw differ.

In response to this ambiguity, we undertook a meta-analytic review[1] of the relevant research literature to examine the magnitudes and moderators of the relationship between CSA and psychological and behavioral disturbances in women. The moderating variables selected for assessment were relevant to subject status (i.e., clinical or nonclinical) and research methodology (i.e., assessment method).

Numerous schema have been suggested to categorize the long-term sequelae associated with a history of CSA. Frequently, the negative correlates found in the research are grouped by psychiatric symptomatology or syndromes. For purposes of this analysis, however, the long-term sequelae examined in empirical CSA research have been grouped into five broad, descriptive categories or domains: affective, behavioral, identity/relational, other psychiatric sequelae, and general symptomatology.

METHOD

Literature Search

No unpublished studies were included in this meta-analysis. Given the difficulties in establishing a database of studies with methodologies meeting the rigorous criteria for inclusion in this review using published studies, we excluded unpublished studies from our consideration. The computerized database PsychLIT (1974 to July 1992) was searched for English-language articles using combinations of the keyword descriptors *child abuse, sexual, sexual abuse, incest, early experiences,* and *human females,* with *adult* in the age-group descriptor. In addition, the bibliography of the first author's dissertation (Neumann, 1991), which reviewed this literature qualitatively, and 10 previously published qualitative reviews (Alter-Reid et al., 1986; Bachmann, Moeller, & Bennett, 1988; Beitchman et al., 1992; Briere & Runtz, 1991; Browne & Finkelhor, 1986; de Chesnay, 1985; Henderson, 1972; Kosky, 1987; Vander Mey & Neff, 1982; Wodarksi & Johnson, 1988) were manually searched to identify studies. The individual studies for the meta-analysis were acquired, and their reference sections searched to identify additional relevant articles. A total of 488 discrete citations were identified by these methods.

Criteria for Study Inclusion

The criteria for including studies in the meta-analysis were as follows:

1. Studies must have been designed to examine psychological or behavioral correlates of CSA in groups of adult females.
2. Results were based on empirical measures (as opposed to clinical impressions).
3. The comparison groups used in individual studies had to be equivalent to the CSA group in terms of clinical status, as described in more detail in the disqualifier No. 4 below.

Based upon review of the abstracts of the 488 articles identified in the literature search, 396 were excluded because they did not meet the inclusion criteria (e.g., they reported case study data only, or they were theoretical discussions, or they did not use an appropriate comparison group).

The full-length reports of the remaining 92 studies were reviewed in detail. However, a number of them were subsequently excluded for the following reasons.

1. Studies that did not report separate results for men and women (e.g., Briere, Evans, Runtz, & Wall, 1988; Felitti, 1991) were excluded because the present meta-analysis was designed to examine sequelae in women only.

2. Studies that did not report data that permitted computation of conventional effect sizes (e.g., multivariate studies without univariate results[2]) were excluded (e.g., Briere & Runtz, 1990; Charmoli & Athelston, 1988).

3. Studies that were designed to examine the incidence of sexual abuse in specific diagnostic groups were excluded for two reasons. First, we wanted to avoid bias in the meta-analytic aggregation. Clinically, specific characteristics are believed to be more prevalent among certain diagnostic groups; consequently, articles on disorders such as depression or borderline personality disorder are more likely to examine and report CSA histories in their subject samples than other studies, such as those designed to examine generalized anxiety disorder patients.[3] Second, in some research literature, CSA was ascertained as a secondary variable of interest by investigators during the course of their evaluation of a specific clinical disorder. Because data on CSA was often incidentally reported in these types of studies, it was difficult to devise a systematic method to identify them and to extract the data necessary for effect size computations.

4. Studies that failed to equate CSA and non-CSA groups on relevant subject selection variables were excluded. Specifically, studies in which CSA samples

were recruited from clinical populations but in which comparison samples were recruited from nonclinical populations were excluded, as were studies that used comparison samples composed of victimized women, such as those who had been raped (e.g., Becker et al., 1982; Herman, Russell, & Trocki, 1986; Scott & Stone, 1986). The rationale underlying this decision was that it avoided biasing the meta-analytic outcomes by over- or underestimating effect size magnitudes, respectively.

5. Studies that either: (a) provided data on individual test items or scales that did not permit aggregation according to the four domains of interest cited in the introduction; (b) reported data as reflecting child sexual *abuse* based on inquiries into childhood sexual *experiences,* which may not always meet the criteria for sexual abuse (e.g., Sedney & Brooks, 1984); or (c) reported reanalysis of data that were already included in the meta-analysis (e.g., Elliott & Briere, 1992b; Scott, 1992) were excluded.

6. Nine studies used the same subject groups but reported differing data on them (Briere, 1988; Briere & Runtz, 1986, 1989; Herman, 1981; Herman & Hirschman, 1981; Murphy et al., 1988; Peters, 1988; Saunders, Villeponteaux, Lipovsky, Kilpatrick, & Veronen, 1992; Wyatt, Guthrie, & Notgrass, 1992). Inclusion of studies drawn from the same sample would create redundancy in study-level outcomes and result in nonindependent effect sizes. Therefore, one study from each of the same-sample groups was randomly selected for inclusion (Briere & Runtz, 1989; Herman, 1981; Murphy et al., 1988; Peters, 1988).

Thus, the computations in this review have been carried out with a sample of 38 studies. They are listed in the Appendix and identified with an asterisk. In domain-level analysis, however, the single study that reported the most representative measure of the domain of interest was retained for inclusion, with the restriction that multiple measures of the same variables obtained from the same subject samples could not be included in the same analysis. The 43 studies used in the domain-level calculations are identified in the appendix with a dagger (†).

We undertook a meta-analytic review of the relevant research literature to examine the magnitudes and moderators of the relationship between CSA and psychological and behavioral disturbances in women.

Definition of Child Sexual Abuse

Most studies examined in the meta-analysis defined CSA as involving physical contact of a sexual nature, ranging from fondling to intercourse (n = 26). Six included noncontact sexual abuse (e.g., forced observation of nudity; Roland, Zelhart, & Dubes, 1989). The remaining five studies did not specify contact or noncontact.

In studies that reported subject age at the time of abuse, the upper age cutoff for considering the sexual abuse to have occurred in childhood ranged from 11 to 17 years old. In a number of studies (n = 16), subject age at the time of abuse was not specified empirically, but rather the abuse was described subjectively as occurring during childhood. In these studies, the age difference between victim and perpetrator was defined. It ranged from 3 to 10 years.

For ease of exposition, we refer to two groups in the subsequent sections: the CSA-positive group (CSAP), composed of women who reported sexual victimization during childhood or adolescence, and the CSA-negative group (CSAN), composed of women who did not report such histories.

Effect Size Computations[4]

In most studies, either the CSAP and CSAN group means and standard deviations, or statistical test values based on comparisons of them, were reported. These data were used to construct the effect sizes. Effect sizes (g-values) based on means and standard deviations were constructed by subtracting the CSAN group mean from that of the CSAP group mean and dividing by their pooled standard deviation. Effect sizes based on statistical test values contrasting the CSAN and CSAP groups were performed using procedures described by Rosenthal (1991). Prior to meta-analysis, bias in individual effect sizes extracted from each study was reduced by applying the procedures recommended by Hedges and Olkin (1985, p. 100) to convert g-values to d-values.

In other studies, however, data from multiple groups were reported, and in these instances the following decisions were made prior to deriving the effect sizes:

1. In single studies that examined both clinical and nonclinical CSAP and CSAN groups, such as that reported by Feinauer (1988) in which clinical CSAP,

clinical CSAN, and nonclinical CSAN women were examined, data from the clinical CSAP and clinical CSAN groups were used to construct the effect size. Similarly, Tsai et al. (1979) presented data for three groups: clinical CSAP, nonclinical CSAP, and nonclinical CSAN. In this instance, the latter two groups were used to calculate effect sizes.

2. In studies that reported data based on subgroups of CSAP subjects, we combined the data for the CSAP subgroups prior to computing effect sizes. Specifically, Finkelhor, Hotaling, Lewis, & Smith (1989) reported data for CSA that involved intercourse and for CSA that did not involve intercourse; Roland et al. (1989) reported data for subjects who were sexually abused by their fathers or stepfathers and those who were sexually abused by other people; Alexander & Lupfer (1987) reported data on victims of father or stepfather perpetrators, of all other family perpetrators, and of extrafamilial perpetrators; Bagley (1991) provided data on women whose CSA histories varied in duration (less or more than 1 week). In each of these instances, effect sizes were derived by contrasts between the CSAN and the multiple CSAP groups.

3. Three studies (Bryer et al., 1987; Surrey, Swett, Michaels, & Levin, 1990; Swett, Cohen, Surrey, Compaine, & Chavez, 1991) provided data on four groups: adult women who had experienced childhood physical abuse only, CSA only, child physical and sexual abuse, and no CSA. In these studies, the CSA-only group and the CSA and child physical abuse group were compared to the combined child physical abuse only and the nonvictim group. In addition, Peters (1988) presented data for contact and noncontact child sexual abuse groups. Again, we combined the noncontact group with the nonvictim data for the meta-analysis. The rationale underlying this decision to combine the physical abuse victims and noncontact sexual abuse victims with the nonvictims in the CSAN group was that in most primary-level studies, victims of these types of abuse were included in the nonvictim group. By adhering to the procedures used in most primary-level studies (i.e., considering all cases where CSA did not occur to be "not abused"), the present meta-analysis avoids increasing variance among effect sizes induced by using different nonvictim groups. Note, however, that the effect sizes derived by contrasting such groups could be attenuated relative to those that would be obtained if CSAN groups consisted solely of nonabused individuals.

4. Murphy et al. (1988) examined mutually exclusive subgroups consisting of child, adolescent, and adult sexual assault victims, victims of multiple assault (who had experienced sexual abuse/assault both as children or adolescents and as adults), and nonvictims. In this study, the CSAP group used for the meta-analysis was composed of members of the child, adolescent, and multiple subgroups and the CSAN group was composed of the adult victim and the nonvictim groups.

Domains of Dysfunction

Five domains were used to categorize long-term negative sequelae associated with CSA: affective, behavioral, identity/relational, other psychiatric sequelae, and general symptomatology. Three measures were used to index the affective domain: anger, anxiety, and depression. Five measures were used to index the behavioral domain: revictimization, self-mutilation, sexual problems, substance use, and suicidality. Two measures were used to index the identity/relational domain: interpersonal problems and self-concept impairment. The psychiatric domain was indexed by four types of symptoms: dissociation, obsessions and compulsions, somatization, and traumatic stress responses; specific measures of these were adapted from the *Diagnostic and Statistical Manual of Mental Disorders* (American Psychiatric Association, 1987). A single index of general symptomatology was defined as consisting of summary measures, such as the overall Global Severity Index score on the SCL 90-R.

Several studies provided measures of domains segregated according to whether they were acute or chronic. For example, Briere & Runtz (1988) provided data for both acute and chronic dimensions of the symptoms they investigated. Similarly, Stein, Golding, Siegel, Burnham, and Sorensen (1988) and Saunders et al. (1992) provided data on current and lifetime psychiatric diagnoses. In all of these cases, effect sizes were derived from acute or current symptoms measurements only.

In addition, some studies provided more than one measure relevant to the assessment of the same domain. For example, Tsai et al. (1979) reported data on a variety of sexual behaviors (sexual dissatisfaction, frequency of orgasms, number of sexual partners, sexual responsiveness to current partner, and quality of close relationships with males). For such studies, effect sizes were calculated using the measure we believed to be most representative of the domain of interest. Hence, because sexual problems functioned as one index of the behavioral domain, the effect size was computed using the measure based on sexual dissatisfaction in Tsai et al.'s (1979) work.

Moderators

Eight moderator variables[5] were examined. Three of them were of substantive interest in relation to CSA: (a) age group at the time of assessment (≤ 30, ≥ 31),

(b) population from which the sample was recruited (clinical, nonclinical, or mixed), and (c) locus of abuse (intrafamilial or mixed). The other five moderators were relevant to the research methodologies of the studies: (a) year of publication (before 1988, 1988, or after), (b) sample size (≤ 50, 51-150, 151-500, ≥ 501), (c) publication form (journal article or book chapter), (d) method of assessment (questionnaire and/or interview, or chart review), and (e) type of statistic used (parametric, nonparametric, or mixed). In addition, demographic data reported in each study were tabulated.

Analysis of Effect Sizes

Two sets of analyses were performed. In the first, each of the 38 studies was represented by a single effect size, and the substantive and methodological study moderators were examined. To derive a single effect size for each study, the individual *g*-values for each relevant measure acquired within single studies were averaged, and then the average *g* was converted to *d* (Hedges & Olkin, 1985) prior to meta-analysis. The categorical models used to assess moderators[6] and the homogeneity of effect sizes[7] were evaluated using methods described by Hedges and Olkin (1985). In the second set of analyses, all effect sizes from single studies that were relevant to the five domains under investigation (i.e., affective, behavioral, identity/relational, other psychiatric sequelae, and general symptomatology) were computed, and these individual *g*-values were converted to *d* prior to meta-analysis.

RESULTS

Study-Level Outcomes

The 38 studies investigated a total of 2,774 CSAP and 8,388 CSAN subjects. Aggregation of them yielded a mean weighted effect size of $d = +.37$. This indicates that there is a small to medium (Cohen, 1988) relationship between a history of CSA and negative long-term sequelae. The 95% confidence interval (CI, +.33 to +.41) excludes zero, which indicates that the association between CSA history and negative long-term sequelae can be considered statistically significant.

There was significant heterogeneity in these effect sizes ($Q_w^8 = 62.36$, $df = 37$, $p < .01$). Removal of one study (Roland, Zelhart, & Dubes, 1985) significantly reduced heterogeneity, yielding a mean weighted effect size *d* of +.36 (95% CI, +.31 to +.40; $Q_w = 39.02$, $df = 36$, ns). Moderator analyses were undertaken based on this set of 37 studies.

TABLE 1: Summary of Substantive Study Moderators

Variable and Class	k[a]	d[b]	*95% CI*[c]	Q_b[d]
Age (years)[e]				.01
30 or below	15	+.39	+.31 to +.47	
≥31	10	+.40	+.33 to +.47	
Source of recruitment				9.40*
Clinical	17	+.50	+.40 to +.61	
Nonclinical	18	+.32	+.27 to +.37	
Mixed	2	+.43	+.20 to +.67	
Locus of abuse				1.73
Intrafamilial	8	+.47	+.29 to +.69	
Mixed	29	+.35	+.30 to +.39	

NOTE: Thirty-seven studies were included in this analysis. Degrees of freedom vary depending on the number of categories.
a. *k* denotes number of studies.
b. *d* denotes mean weighted effect size.
c. CI denotes confidence interval.
d. Q_b denotes between-class effect.
e. Twelve studies did not provide usable data.
*$p < .01$.

Substantive study moderators. Table 1 shows results of study comparisons based on subject age, source of recruitment, and locus of abuse. There were no significant effect-size differences based on age. For source of recruitment, studies that selected subjects from among clinical groups obtained higher effect sizes than those studies that recruited subjects from among nonclinical groups; mixed samples yielded intermediate values. There were no significant differences based on locus of abuse as assessed by these studies.

Methodological study moderators. Table 2 shows results of comparisons based on year and form of publication, sample size, type of statistic used, and assessment method. None of these were significant, although there was a tendency for studies that examined less than 50 subjects to yield comparatively high mean effect sizes ($Q_b^9 = 6.61$, $df = 3$, $p = .08$), compared to studies that examined larger numbers of subjects.

Domain-Level Outcomes

Table 3 shows the mean effect sizes for the affective, behavioral, identity/relational, psychiatric, and general symptomatology domains. The mean effect sizes range from $d = +.32$ for self-concept impairment to $d = +.67$ for revictimization. All 95% confidence levels excluded zero, and by this criterion the results for measures of all domains can be considered statistically significant.

DISCUSSION

This meta-analysis indicates a significant relationship between CSA and psychological distress and dys-

TABLE 2: Summary of Methodological Study Moderators

Variable and Class	k[a]	d[b]	*95% CI*[c]	Q_b[d]
Median year of publication	1988			.36
< 1988	17	+.33	+.25 to +.42	
≥ 1988	20	+.36	+.31 to +.42	
Sample size (Total *n*)				6.61
≤ 50	5	+.71	+.41 to +1.02	
51-150	17	+.40	+.29 to +.50	
151-500	9	+.35	+.26 to +.45	
≥ 501	6	+.33	+.28 to +.39	
Publication form				.42
Journal article	33	+.35	+.31 to +.40	
Book chapter	4	+.42	+.23 to +.60	
Type of statistic used				3.97
Parametric	14	+.40	+.34 to +.47	
Nonparametric	20	+.31	+.25 to +.37	
Mixed	3	+.37	+.21 to +.53	
Assessment method				1.49
Questionnaire and/or interview	30	+.35	+.30 to +.39	
Chart review	7	+.46	+.29 to +.62	

NOTE: Degrees of freedom vary depending on the number of categories.
a. *k* denotes number of studies.
b. *d* denotes mean weighted effect size.
c. CI denotes confidence interval.
d. Q_b denotes between-class effect.

TABLE 3: Summary of Domain-Level Effect Sizes

Measure	k[a]	d[b]	*95% CI*[c]
Affective Domain			
Anger	8	+.39	+.25 to +.51
Anxiety	11	+.40	+.34 to +.47
Depression	24	+.41	+.36 to +.46
Behavioral Domain			
Revictimization	5	+.67	+.50 to +.84
Self-mutilation	3	+.42	+.19 to +.64
Sexual problems	16	+.36	+.30 to +.42
Substance abuse	14	+.41	+.31 to +.51
Suicidality	8	+.34	+.24 to +.44
Identity/Relational Domain			
Self-concept impairment	10	+.32	+.32 to +.47
Interpersonal problems	10	+.39	+.22 to +.46
Psychiatric Domain			
Dissociation	5	+.39	+.32 to +.47
Obsessions and compulsions	7	+.34	+.22 to +.46
Somatization	9	+.34	+.24 to +.45
Traumatic stress responses	4	+.52	+.44 to +.59
General Symptomatology			
Index of general symptomatology	11	+.46	+.40 to +.52

NOTE: Forty-three studies were included in this analysis.
a. *k* denotes number of studies.
b. *d* denotes mean weighted effect size.
c. CI denotes confidence interval.

function in adult women. The mean effect sizes indexing the association between CSA and dysfunctions in the affective, behavioral, identity/relational, psychiatric, and general symptomatology domains were small to medium in magnitude. There was, however, some variation in the magnitudes of the effect sizes associated with distinct operational measures, both within and across domains (see Table 3). The similarity in the magnitudes of these effect sizes across domains is noteworthy, because the impacts of sexual abuse have been thought to be harder to detect using generic measures of psychiatric symptoms or disorders, such as the MMPI, SCL-90-R, and structured clinical interviews based upon *DSM-III-R* criteria (Briere & Runtz, 1989).

Significance in the meta-analytic aggregation of studies increases the likelihood that the effects reported here are real, as opposed to artifacts of methodology, sample variation, or measurement error that might occur in single studies (Briere, 1992b). Thus, the current analysis suggests that across methodologies, samples, and measures, CSA is a general risk factor for the development of later psychological disturbance, at least for adult women. In addition, the results suggest that certain problems may be associated especially (although not necessarily uniquely) with a sexual abuse history, such as traumatic stress and revictimization.

It should be noted that although these findings indicate an association between CSA and psychological distress and dysfunction, we cannot be certain that this relationship is causal (i.e., that CSA causes suicidality, substance abuse, etc.). There are at least two major impediments to inferring causality from these results: "third variable" concerns and the limitations of retrospective data per se.

Some of the associations found in the present study may be epiphenomena of sexual abuse. That is, some abuse-associated symptoms may arise from other factors (third variables) correlated with both sexual abuse and symptomatology, such as antecedent or concurrent variables that include other forms of child maltreatment, or the general effects of a negative family environment (Briere, 1992b). Unfortunately, some studies do not separate sexual abuse from physical or psychological abuse or neglect for evaluation. In view of the definitions of the control groups used in many of the primary-level studies and our decision to adhere to them for this meta-analysis, the mean effect sizes reported here might have been attenuated relative to those that would have been obtained if the

control groups were composed solely of nonabused individuals.

In addition, most of the studies in the current sample did not consider the role of family dynamics as potentially moderating variables. Thus, these issues could not be addressed in the current analysis. Yet, it is a common clinical hypothesis that the familial context of child abuse is important, and that intrafamilial sexual abuse effects differ from those of extrafamilial abuse (Courtois, 1988; Harter, Alexander, & Neimeyer, 1988). In a similar vein, some studies suggest that abuse perpetrated by parents or immediate family members is more harmful than abuse by those less intimately involved in child rearing, such as extended family members (Briere & Elliott, 1993; Russell, 1986; Tsai et al., 1979). Although they question current approaches to controlling family effects in sexual abuse research, Briere and Elliott (1993) cite instances where abuse-symptom relationships remain in some form despite third variable control (e.g., Briere & Runtz, 1990; Elliott & Edwards, 1991; Peters, 1988).

Ultimately, the causality hypothesis must be tested through longitudinal research, such as that of Egeland and colleagues (e.g., Egeland, 1989; Egeland & Erickson, 1987; Egeland & Faber, 1984). This research group has followed a cohort of abused children from very early in life into (thus far) mid-childhood, documenting a number of negative psychological symptoms in these subjects as they mature. Additional longitudinal research is required to replicate Egeland et al.'s relative concurrence with the cross-sectional findings reported here. If their data are representative, CSA might be a critical and etiological agent involved in some or all of the associations detected in this meta-analysis.

A second constraint on this meta-analysis was the retrospective nature of the original data. As a result, potential problems associated with recall bias cannot be ruled out. This may manifest as two relatively opposite phenomena. Not only is it possible that some individuals who report a given form of abuse in childhood misrepresent some or all of their childhood experience (Rich, 1990), it is also possible that some proportion of those denying CSA (i.e., those in the nonabused comparison group) actually were abused as children and either do not remember the event (Briere & Conte, 1993; Williams, 1992) or have chosen to deny it when questioned by researchers (Femina et al., 1990).

These results must also be interpreted with caution in light of the exclusion of unpublished studies from the sample. As Lipsey and Wilson (1993) suggest, the published literature may give an inflated effect size when compared with effect sizes drawn from the published and unpublished literature, due to editorial and submission bias. Future research can address this suggestion, including unpublished versus published status as a moderating variable.

A number of substantive and methodological study variables were considered in this analysis. Of the former, which include age, source of recruitment, and locus of abuse, only source of recruitment moderated outcomes: sexually abused women in treatment appear to have more severe psychological difficulties than do abused women in nonclinical samples, although it should be emphasized that in both groups, the meta-analytic outcomes were significant. This might be expected, given that people seeking mental health services usually experience significant psychological distress. On the other hand, sexual abuse effects might "get lost" (or at least become attenuated) in the context of the generalized psychological symptomatology present in both abused and nonabused subjects. The current data suggest, in this regard, that CSA effects in clinical groups are of sufficient magnitude to overshadow the nonspecific symptomatology of comparison subjects.

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Another implication of the recruitment moderator effect concerns the external validity of CSA research findings. Because clinical studies typically show greater impacts of CSA than do nonclinical ones, it may not be appropriate to infer from the former alone that CSA is invariably associated with a given symptom or difficulty. For example, it might be that self-mutilation is virtually unrelated to CSA abuse in general population samples, yet is a powerful predictor of abuse history in clinical groups. More generally, this finding reinforces Finkelhor and Browne's (1985) contention that clinical research may tend to magnify the impact of CSA by identifying profound effects that do not necessarily generalize to nonclinical abuse victims.

None of the tests of the methodological study moderators investigated (year of publication, sample size, publication form, statistic used, and assessment method) attained significance in the current analysis. Such findings are consistent with the possibility that in the CSA literature, these methodological differences between studies may not strongly influence what findings are obtained. The stringent inclusion criteria for studies in this analysis should be noted, however, in that elimination of many studies may have restricted the range of methodological variations that might otherwise have been associated with study outcomes.

Investigation of the symptom domains indicated that anxiety, anger, depression, revictimization, self-mutilation, sexual problems, substance abuse, obsessions and compulsions, dissociation, post-traumatic stress responses, and somatization are all associated with CSA. The relationship between history of CSA and revictimization and post-traumatic stress responses was particulary marked. However, few studies investigating these symptom areas were included in this meta-analysis, due to the stringent exclusionary criteria employed. Therefore, generalization from these results must be limited. Further research is needed to clarify and confirm our findings.

The clinical implications of this meta-analysis reside in two areas: those relevant to prevention of psychological disorders and those regarding clinical intervention. To the extent that sexual abuse does, in fact, confer the various psychological difficulties identified here, it is possible that preventing CSA or intervening early in cases where CSA has occurred would decrease the incidence and prevalence of certain psychological symptoms and disorders in our society. Second, if there is an abuse-symptom relationship, one possible approach to psychotherapy is to directly address the abuse and its developmental reverberations (Briere, 1992a; McCann & Pearlman, 1990). Such an approach might facilitate new treatment responses among individuals with disorders and dysfunctions that have heretofore been viewed as refractory to treatment.

Three general weaknesses of the reviewed literature can be addressed by future researchers with little difficulty. First, studies should more routinely examine potential moderators of abuse-symptom relationships. Variables that have not been examined with enough frequency to allow meta-analytic assessment include family environment, relationship between victim and perpetrator, victim's age at onset and termination of abuse, disclosure of abuse and others' response to it, duration and frequency of abuse, use of force, and presence of vaginal or anal penetration.

Second, future research should address more fully the wide range of potential abuse effects. Although the results of this meta-analysis point to the importance of specific problems, including revictimization, self-mutilation, dissociation, and post-traumatic stress, these variables were only examined in a relatively small number of studies. Other problems, such as eating disorders, impaired self-reference, personality disorders, and altered sexual arousal patterns have been associated with CSA in the clinical literature and a few empirical studies, but at present, these have not received the kind of research attention devoted to more classic difficulties, such as depression and anxiety. If, as some clinicians suggest, these behavior patterns are especially correlated with CSA, future meta-analyses may find even more persuasive evidence of an abuse-symptomatology relationship.

Third, future research should report demographic characteristics of groups (i.e., race/ethnicity, socioeconomic status, marital status, religious background, and sexual orientation). Preliminary data (Russell, Schurman, & Trocki, 1988) suggest that these are important categories that might moderate abuse-symptom relationships, and they are worthy of intensive investigation.

In conclusion, the present study suggests that the assumption of many in the child abuse field has merit: CSA is related to a variety of symptomatic outcomes in adulthood. Only further theory-based research—using more sophisticated methodologies and outcome measures—will resolve the issue of whether such outcomes arise from CSA or merely correlate with it.

APPENDIX

Items marked with an asterisk (*) indicate studies included in the meta-analysis. Those marked with a dagger (†) indicate studies included in the domain-level analysis. More complete information on short entries can be found in the main reference list.

*†Alexander & Lupfer (1987).
*†Bagley (1991).
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NOTES

1. Meta-analytic reviews use statistical procedures to combine the results of empirical studies relating one variable to another. To conduct a meta-analytic review, the relationships between variables are rigorously defined, relevant sources of data are gathered from the extant literature, and the summary data from source reports are then converted into a standardized statistic, the effect size. The effect sizes are then aggregrated across studies, and this composite effect size is tested to see whether it differs significantly from zero. This determination is based upon examination of the 95% confidence interval drawn around each mean. If the confidence interval includes zero, it may be concluded that no relationship exists between the dependent and independent variables. A test is also conducted to see whether the studies consistently find the overall result. A meta-analysis that achieves statistically significant results indicates that studies investigating a relationship between two variables consistently find results of the same direction and magnitude (Johnson, 1989).

2. Multivariate studies examine group differences on more than one dependent variable. Univariate studies examine group differences on a single variable.

3. Previous investigations have reported relationships between childhood sexual abuse and borderline personality disorder diagnosis (Briere & Zaidi, 1989; Herman, Perry, & van der Kolk, 1989), multiple personality disorder diagnosis (Coons & Milstein, 1986), eating disorders (Steiger & Zanko, 1990), patients with chronic pelvic pain (Walker et al., 1988), women experiencing premenstrual syndrome (Paddison, Gise, Lebovitz, Strain, & Cirasole, 1990), prostitution (Bagley & Young, 1987), and substance abuse (Rosehnow, Corbett, & Devine, 1988).

4. Effect sizes in this study are standard scores representing the difference between CSAN and CSAP groups on the variable of interest expressed in standard deviation units (d).

5. In this study, moderating variables are variables that influence the impact of the relationship of childhood sexual abuse and psychological and behavioral disturbances in women.

6. Categorical model testing is used to explain variablility among effect sizes by differences in study qualities.

7. Homogeneity refers to the consistency of effect sizes in a data set and is measured by the statistic Q.

8. Q_w is a measure of within groups variability.

9. Q_b is a measure of between groups variability.

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