

## EFFECTIVENESS OF PSYCHOLOGICAL INTERVENTIONS FOR CHILD MALTREATMENT: A META-ANALYSIS

ELIZABETH SKOWRON

*Pennsylvania State University*

DAWN H. S. REINEMANN

*University of Wisconsin—Milwaukee*

*A meta-analysis tested the effectiveness of psychological treatments for child maltreatment (CM) using weighted least squares methods (e.g., L. V. Hedges & I. Olkin, 1985). A mean effect size of  $d_+ = 0.54$  ( $SE = .03$ , 95%  $CI = .39-.69$ ) was observed, indicating that on average, treated participants were better off than 71% of those in control groups. Partitioning by type and target of outcome assessment yielded homogeneous effects within each of 5 different outcomes. Treatment effects were weakest when linked to objective behavioral observations of the family ( $d_+ = .21$ ) and strongest when associated with parent self-reported parenting attitudes*

*and behaviors ( $d_+ = .53$ ). Results of other moderator analyses are presented, along with limitations of current CM treatment research; implications for future research, practice, and social policy are discussed.*

With the federal government's creation of the National Center on Child Maltreatment (NCCAN) in the early 1970s, research on child maltreatment (CM) has dramatically increased, and a growing body of clinical theory and empirical data on this subject now exists. CM is defined as the following:

all forms of physical and/or emotional ill-treatment, sexual abuse, neglect, or negligent treatment . . . resulting in actual or potential harm to the child's health, survival, development or dignity in the context of a relationship of responsibility, trust, or power. (World Health Organization, 1999, p. 15)

CM is generally thought to encompass four types of CM: physical abuse, child sexual abuse, physical neglect, and general maltreatment (Cicchetti & Barnett, 1991). CM is a leading cause of death among children ages 1–4 in the United States (Centers for Disease Control & Prevention, 2004; U.S. Advisory Board on Child Abuse & Neglect, 1995), where an average of three children die each day from maltreatment. In 2000, approximately 879,000 victims of CM (i.e., substantiated cases) were identified in the United States alone.

In addition to physical injuries, CM also may lead to serious psychological, emotional, behavioral, cognitive (e.g., Azar & Wolfe, 1998; Beitchman, Zucker, Hood, daCosta, & Akman, 1991; Berliner & Elliott, 2002; Cicchetti & Olsen, 1990; Erickson & Egeland, 2002; Kendall-Tackett, Williams, & Finkelhor, 1993; Kolko, 2002), and neurobiological disorders (Teicher, 2002) among children. Specifically, CM has been linked with the development of internalizing disorders, such as depression, anxiety, posttraumatic stress, somatic complaints, suicidality, and externalizing disorders, in the form of aggression, acting out, anger, impulsiveness, delinquency,

---

Elizabeth Skowron, Department of Counselor Education, Counseling Psychology, and Rehabilitation Services, Pennsylvania State University; Dawn H. S. Reinemann, Department of Educational Psychology, University of Wisconsin—Milwaukee.

Portions of this article were presented at the 2002 annual meeting of the Society for Psychotherapy Research, Chicago. This research was supported by grants from the Center for Urban Initiatives and Research at the University of Wisconsin—Milwaukee and a Research Initiation Grant from the College of Education, Pennsylvania State University, to Elizabeth Skowron. Both authors contributed equally to this project. We thank Thomas A. Schmitt, Rebecca Ramczyk, Anna K. Dendy, Jacqueline Gosz, and Brian D. Keller for their assistance with literature searches and coding and Robert T. Ammerman, Sandra T. Azar, Aaron L. Pincus, Paul Priester, and Philip L. Smith for helpful comments on an earlier version.

Correspondence concerning this article should be addressed to Elizabeth Skowron, PhD, Counseling Psychology Program, Department of Counselor Education, Counseling Psychology, and Rehabilitation Services, 332 Cedar Building, Pennsylvania State University, University Park, PA 16802-3110. E-mail: eas14@psu.edu

and hyperactivity, as well as attachment disorders, sexually inappropriate behaviors, and cognitive impairment or delays (e.g., Azar & Wolfe, 1998; Berrick, 1997; Beitchman et al., 1991; Browne & Finkelhor, 1986; Finkelhor, 1990; Kaplan, Pelcovitz, & Labruna, 1999; Kendall-Tackett et al., 1993).

Because of the widespread incidence of CM and its association with a diverse array of adjustment problems, a variety of psychological treatments for CM have been developed and are currently in use. For example, of the substantiated cases of CM identified in 2000, approximately half (55%) or 483,450 clients received some form of psychotherapeutic intervention for CM (National Clearinghouse on Child Abuse & Neglect, 2000). Although, historically, treatments for CM were rarely evaluated, the last decade has seen a dramatic increase in the number of empirical studies documenting their effectiveness. However, to date it remains uncertain whether these treatments, on the whole, are effective.

Attempts at summarizing this nascent literature on CM treatments have resulted in a number of narrative reviews that have yielded conflicting reports about these treatments' effectiveness. An early qualitative review of 89 federally funded studies of CM treatments concluded that most treatment programs were largely ineffective in halting abuse, neglect, or future maltreatment, with "one third or more of the parents served . . . maltreat[ing] their children while in treatment, and over one half of the families . . . judged by staff as likely to mistreat their children following termination" (Cohn & Daro, 1987, p. 440). Subsequent reviews (e.g., Belsky, 1993; Emery, 1989; Melton & Flood, 1994; O'Donohue & Elliott, 1992; Shirk & Eltz, 1998) have concluded similarly that psychological treatments for CM are generally ineffective.

In contrast, other narrative reviews (e.g., Azar & Wolfe, 1998; Finkelhor & Berliner, 1995; Oates & Bross, 1995; Saywitz, Mannarino, Berliner, & Cohen, 2000; D. A. Wolfe & Wekerle, 1993) have concluded that some CM treatments appear promising. For example, Oates and Bross (1995) reported that most treatment programs for physically abused and neglected children appear to lead to some improvement, although many fail to assess follow-up to learn whether gains are maintained. Finkelhor and Berliner (1995) con-

cluded that many studies report improvements in sexually abused children following treatment, though few studies reviewed incorporated research designs (e.g., use of randomization, comparison/control groups, etc.) that allowed one to conclude that recovery was not simply due to the passage of time.

A single meta-analysis of psychological treatments for CM appeared recently, in which 15 studies of group treatments for child sexual abuse were summarized (Reeker, Ensing, & Elliot, 1997). A mean within-subjects effect size of 0.79 was reported, indicating that participants' functioning improved following treatment. However, because all of the studies included in their investigation used a one-group, pretest-posttest design, the size of effects reported were (a) likely inflated estimates of treatment effectiveness and (b) not comparable to conventional effect size estimates calculated for studies using independent treatment and control groups (e.g., Weisz, Weiss, Han, Granger, & Morton, 1995; Wilson & Lipsey, 2001). Because the use of no-treatment or placebo control groups (i.e., withholding treatment) has been discussed as a serious ethical concern in the case of CM treatment (Ammerman, 1998), CM researchers have been reluctant to apply experimental designs that may enable one to conclude with greater confidence that interventions account for participant improvements and to rule out other plausible explanations (i.e., statistical regression, maturation, or passage of time) for observed changes (Campbell & Stanley, 1963). In sum, wide disparity in conclusions drawn about whether or not psychological treatments for CM are effective stems from the variability in rigor of designs used (National Research Council, 1993), the particular sample of studies reviewed, the overreliance on qualitative or narrative means to analyze study effectiveness, or some combination of these.

Although there is little definitive information about the effectiveness of psychological treatments for CM, in contrast, considerable evidence exists to support the efficacy of individual child and family therapies for a variety of other child-identified problems. Mean effects reported in nine different meta-analyses range from 0.45 to 0.88 for effects on the basis of weighted least squares (WLS) and unweighted least squares (ULS) methods, respectively, corresponding closely to Cohen's (1988) conventions for medium (.5) and large (.8) effects. Among studies of

child therapy, Casey and Berman (1985) used a ULS method to summarize studies published from 1952 to 1983 and found an average effect of 0.71 for treatments versus placebo and no-treatment controls among children ages 12 and younger. Weisz, Weiss, Alicke, and Klotz (1987) reviewed studies published between 1958 and 1984 and reported a similar uncorrected mean effect (0.79) for treatments targeting children ages 4–18 versus no-treatment or placebo controls. Similarly, Kazdin, Bass, Ayers, and Rogers (1990) summarized studies published from 1970 to 1988 using ULS methods and reported mean effects of 0.88 and 0.77 for treatments of children ages 4–18, compared with no-treatment and placebo conditions, respectively.

More recent meta-analyses of individual child and family therapies have used WLS methods to aggregate study effects. Weisz et al. (1995) summarized studies of individual therapies for children ages 1.5–17.6 years, published between 1967 and 1993, and reported an average effect of 0.54. Two statistical reviews of the effectiveness of family therapy also used a WLS approach (Hazelrigg, Cooper, & Borduin, 1987; Shadish et al., 1993) and reported comparable effect sizes (i.e., from .45 to .50 and .36, respectively) for family treatments targeting child-identified problems.

Although sufficient numbers of methodologically sound studies exist to warrant a decisive conclusion about the general overall effectiveness of individual child and family therapies for child-identified problems, only recently have enough studies of psychological treatments for CM appeared in the literature, permitting an initial comprehensive examination of their overall effectiveness. For example, of the studies dating from 1974 to 2000 that were included in this review, over half were published in the last decade. Because, to our knowledge, no other published meta-analysis of controlled CM treatment outcome studies exists to date, the broad purpose of this study was to conduct a meta-analysis to test the effectiveness of psychological treatments for CM. Meta-analysis permits investigators to calculate and compare effect sizes across studies and can provide estimates of the differential impact of various study characteristics—such as participant characteristics, treatment characteristics, outcomes assessed, and research design quality—on the size of treatment effects observed (Hedges & Olkin, 1985). Thus, a meta-

analytic approach to summarizing CM treatment studies will provide (a) an important, initial quantitative summary of the general effectiveness of psychological treatments for CM and (b) initial information about factors associated with differential treatment effects.

We elected to conduct a comprehensive meta-analysis of treatments for all forms of CM, owing to the relatively small number of well-conducted outcome studies, general lack of consensus regarding operational definitions of different forms of maltreatment (e.g., Ammerman, 1998; Browne & Finkelhor, 1986; Emery, 1989), and evidence suggesting that one quarter to one half of maltreated children experience more than one form of abuse (U.S. Department of Health & Human Services, 2001). First, although experts in the field of CM are recommending use of consensus operationalizations of four types of CM—(a) physical abuse, (b) sexual abuse, (c) physical neglect, and (d) emotional maltreatment (see Cicchetti & Barnett, 1991; Emery & Laumann-Billings, 1998; National Research Council, 1993)—the set of treatment studies reviewed indicate that CM research varies widely in terms of clarity of CM operationalizations, assessment of abuse-specific experiences, and specificity of treatment focus. As a result, research has not yet established whether differences exist in the effectiveness of treatments for children who experience different forms of maltreatment, such as physical or sexual abuse (V. V. Wolfe, 1989). Moreover, in the case of CM, “nature conspires against scientific analysis by generating the problem of comorbidity” (Belsky, 1993, p. 413). For example, over one quarter (25.3%) of CM victims also suffered at least one other form of maltreatment and of the substantiated cases of CM identified in 1999 in the United States, 35.9% were reported for more than one form of abuse (U.S. Department of Health and Human Services, Administration on Children, Youth, and Families, 2001). Therefore, we reasoned that a comprehensive quantitative review of psychological interventions for CM broadly defined—including effect size variations among treatments targeting different forms of abuse—would be useful for consolidating what is known and not known about the effectiveness of CM interventions.

Beyond the question of general effectiveness of CM treatments, we also sought to examine whether CM treatment effects differ according to

the type and target of outcomes assessed. Research on developments in meta-analysis indicates that different outcome operationalizations account for considerable variance in treatment effect sizes (Bangert-Drowns, 1986; Durlak, 1995; Wilson & Lipsey, 2001). Likewise, primary prevention (Durlak & Wells, 1997) and individual therapy (e.g., Durlak, Fuhrman, & Lampman, 1991) for child-identified problems each have yielded different effect magnitudes, based on type of outcome assessed. Therefore, we sought to determine whether treatment effects varied by type of outcome construct assessed (i.e., child cognitive process, child personality self-report, child personality interview, parent ratings of child, teacher ratings of child, objective behavioral observations of child, parent self-report, and objective behavioral observations of family). We expected that larger effects would be associated with self-report indices of functioning.

The final purpose of this meta-analysis was to explore the potential impact of other conceptually and practically relevant study characteristics identified in previous meta-analyses (e.g., Casey & Berman, 1985; Durlak et al., 1991; Lipsey & Wilson, 1993; Weisz et al., 1995) and narrative reviews (e.g., Azar & Wolfe, 1998; Kendall & Maruyama, 1985; Rosenthal, 1995) on the size of treatment effects observed. Study characteristics were partitioned into (a) participant characteristics, (b) treatment characteristics, and (c) quality of study design. Each is briefly considered below.

## **Study Characteristics**

### *Participant Characteristics*

*Type of child maltreatment.* CM is a heterogeneous category with each form of maltreatment having unique etiological and contextual determinants, treatment approaches, and likely variations in client responsiveness to treatment. Although family dysfunction or a general maltreating environment appears to be common across various forms of CM and likely contributes to some similarities in the range and types of problems exhibited by all maltreated children, abuse-specific characteristics exist that differentiate types of CM (e.g., sexual vs. physical abuse; Kendall-Tackett et al., 1993). For example, although all forms of CM occur within a relational context (Cicchetti & Olsen, 1990), sexual abuse specifically involves exposure to traumatic sexual acts

or sexual behaviors that are nonconsensual and developmentally inappropriate, leading to child feelings of powerlessness, stigmatization, and betrayal (Berliner & Elliott, 2002; Finkelhor, 1988). Child-focused treatments for sexual abuse (e.g., Celano, Hazzard, Webb, & McCall, 1996; Cohen & Mannarino, 1996, 1997, 1998; Deblinger & Lippman, 1996; Deblinger, Steer, & Lippman, 1999; Sullivan, Scanlan, Brookhouser, Schulte, & Knutson, 1992) are designed to target thoughts and feelings about the abusive experience and perpetrator, address specific psychological symptomatology, teach coping skills, and provide education on sexual abuse, body safety, and healthy sexuality.

In contrast, conceptual models of physical abuse focus on the maladaptive family environment, history of family violence, parental attitudes and beliefs regarding discipline, distorted expectations of children, use of aversive child management strategies, and parental deficits in anger control and emotion regulation (Azar & Wolfe, 1998). Treatment for child physical abuse (e.g., Kolko, 1996a, 1996b; Moore, Armsden, & Gogerty, 1998; Wolfe, Edwards, Manion, & Koverola, 1988) is frequently parent- and/or family-focused and may consist of parent training in child behavior management techniques, changing distorted or irrational beliefs that may lead to physical abuse, training in anger control and stress reduction strategies (Azar & Wolfe, 1998), parent-child interaction training, and abuse-focused family treatment (Kolko, 2002). In sum, etiological differences exist among different forms of CM and have led to the development of different treatment interventions. These interventions target unique, abuse-specific dimensions of experience and use various methods to assess outcome. Thus, we summarized the proportion of existing studies that provided clear operational definitions of the primary type of CM targeted and then calculated and reported descriptive data on size of treatment effects associated with each form of CM in order to provide some initial information about potential variations in treatment effects by type of maltreatment.

*Mandated treatment.* Maltreating parents as a group are not likely to self-refer to treatment (Azar & Twentymann, 1986; Conger, 1982). Instead, these families typically come to the attention of treatment providers following the involvement of child protective service agencies. Some evidence suggests that court-involved families

benefit slightly more from family therapies than those not involved with the legal system (Shadish et al., 1993). Given this, we explored whether treatments in the current sample were differentially effective for clients who were mandated to attend treatment versus those who opted to participate voluntarily.

### *Treatment Characteristics*

*Treatment modality and orientation.* A variety of psychological treatments have been designed to reduce the incidence of maltreatment and to address the broad range of abuse-related problems in child victims and their families. Likewise, some studies (e.g., Casey & Berman, 1985; Weisz et al., 1987) have concluded that behavior therapies outperform nonbehavioral interventions for child-focused problems. Given the diversity of treatments developed for maltreated children and their families (Azar & Wolfe, 1998; Kolko, 1998) and possible associated variations in effect size (e.g., Weisz et al., 1995), we sought to examine whether treatment effects differed by treatment modality (i.e., individual, group, family, milieu, or multicomponent approaches) or across behavioral and nonbehavioral forms of interventions.

### *Quality of Study Design*

*Type of comparison group.* Treatment effects have been shown to systematically vary with type of control group (e.g., no treatment, placebo, or alternate treatment) involved. Studies comparing treatments with placebos generally show smaller effect sizes than those that use a no-treatment control condition (Wilson & Lipsey, 2001). For example, among 15 meta-analyses of adult psychotherapy, average effect sizes for psychotherapies compared with placebo and no-treatment controls were 0.48 versus 0.82, respectively (Lambert & Bergin, 1994). Accordingly, we tested the relative effectiveness of CM treatments versus no-treatment, placebo control, and standard case management conditions.

*Study design.* Until recently, the rigor of research designs applied to studying treatment of CM has lagged noticeably behind that of research on psychotherapy of other child and family disorders (Ammerman, 1998; Kolko, 1998; National Research Council, 1993). Though experimental designs are becoming more common, small sam-

ple sizes can limit the statistical power needed to detect differences among treated and untreated groups, and lack of long-term follow-up prevents investigators from drawing conclusions regarding maintenance of treatment gains (Ammerman, 1998; Kolko, 1998; National Research Council, 1993). Limited use of reliable and valid assessment instruments along with the creation of insufficiently developed and piloted tests to assess CM treatment outcomes contribute to the general impression that "the child maltreatment literature is weak" (Ammerman, 1998, p. 125). However, recent advances in CM research have led to the emergence of published controlled clinical trials that include such elements as random assignment of participants, use of standardized outcome measures, larger sample sizes, and measures of follow-up (Azar & Wolfe, 1998; Kolko, 1998). Therefore, we examined whether size of CM treatment effects covaried with quality of study design in the current sample of studies.

Although we were also interested in exploring whether other treatment characteristics (i.e., therapist experience, attrition rates, and use of treatment manuals) and client characteristics (i.e., gender, age, and ethnicity) covaried with effect size estimates, too few studies reported these data to permit assessment of their impact on treatment effects. Despite these limitations, the recent increase in the quantity and quality of CM treatment outcome studies has made it possible for a meta-analytic review of the literature that provides initial information about the general effectiveness of CM treatments and explores potential moderators that influence treatment outcomes.

## **Method**

### *Data Collection*

We conducted a search for articles published from 1974 through the end of 2000. Three methods were used to identify relevant studies: computerized searches of existing databases (i.e., PsycINFO, PsycLIT, Academic Search Elite), manual searches, and a review of references from studies identified through database search and narrative reviews of CM treatment research (e.g., Azar & Wolfe, 1998; Kolko, 1992; Shirk & Eltz, 1998; V. V. Wolfe, 1989; D. A. Wolfe & Wekerle, 1993). In addition, 4 years of the Dissertation Abstracts International between 1974 and 2000 were randomly selected as representa-

tive of the body of unpublished research (e.g., Durlak & Wells, 1997) and hand searched for relevant unpublished treatment studies. As such, the effect sizes reported herein should be considered reasonable estimates of the magnitudes of treatment effects for studies conducted to date.

### *Description of Studies*

Treatment studies were included in this review if they met the following criteria: (a) participants were referred for CM, physical abuse, sexual abuse, and/or physical neglect; (b) a treatment must have been compared with a control group drawn from the same population; (c) results had to be reported in sufficient detail to permit calculation or estimation of effect sizes; and (d) the study must have appeared in an English-language publication. Studies of the effectiveness of therapies for adults with childhood histories of abuse/neglect were beyond the scope of this review. Likewise, this review did not include prevention studies, as several excellent reviews of the effectiveness of preventive interventions for CM exist (e.g., Cox, 1998; Daro & Donnelly, 2002; Davis & Gidycz, 2000; MacLeod & Nelson, 2000). In addition, those studies using one-group, pretest-posttest designs (e.g., Kruczek & Vitanza, 1999; Lanktree & Briere, 1995; Lutzker & Rice, 1987) and  $N = 1$  studies (e.g., Roesler, Savin, & Grosz, 1993; Wolfe, St. Lawrence, Graves, Brehony, Bradlyn, & Kelly, 1982) were excluded from this study because of incompatibility of effect sizes.

Of the studies identified for possible inclusion, seven used comparison/control groups composed of nonmaltreated or "normal" children. Four of these studies were included in our analyses because they contained a second comparison group composed of maltreated children (i.e., Elmer, 1986; Fantuzzo et al., 1988; Fantuzzo et al., 1996; Sankey, Elmer, Halechko, & Schulberg, 1985). A few studies provided insufficient data in the article to calculate or estimate effect sizes (e.g., De Luca, Boyes, Grayston, & Romano, 1995; Moore et al., 1998), so the authors of these studies were contacted to obtain the necessary data.

A total of 21 studies examining 25 viable treatments for CM met our inclusion criteria. For the purposes of this investigation, all nonindependent, published studies were combined and treated as single studies (e.g., Cohen & Mannarino, 1996, 1997; Deblinger & Lippman, 1996;

Deblinger et al., 1999; Kolko, 1996a, 1996b). In addition, three studies (Deblinger & Lippman, 1996; Deblinger et al., 1999; Fantuzzo et al., 1988; Kolko, 1996a, 1996b) examined the effectiveness of two or more viable treatments versus comparison/control group. For example, Deblinger and her colleagues (1996, 1999) compared the efficacy of (a) child, (b) parent, and (c) parent-child-treatments with a community case management condition for child sexual abuse victims experiencing posttraumatic stress disorder. Fantuzzo et al. (1988) examined peer and adult treatments for withdrawn, maltreated children and contrasted findings with a placebo control. Kolko (1996a, 1996b) compared (a) individual child and parent cognitive-behavioral therapy and (b) family systems therapy with routine CPS case management for maltreating families. Because of issues of nonindependence of the data, effect sizes calculated by comparing each treatment performance to the controls were averaged across the treatments within each study to produce a single effect size per study. However, because these three studies included treatments administered using different modalities (i.e., individual vs. family therapies), effects associated with each viable treatment were included only in the test of variability in effect sizes associated with different treatment modalities. Across the 21 studies reviewed, the total number of cases included was 964, yielding a mean study sample size of 46 cases.

### *Selection of Variables and Coding of Studies*

Each study was coded along 62 dimensions that characterized participants (i.e., type of abuse targeted), treatments (i.e., modality, type of control group), type and target of outcome assessed, and quality of study design. For example, client age, gender, and ethnicity were coded. Treatments investigated were coded for theoretical orientation (i.e., behavioral or nonbehavioral), modality (i.e., individual, group, family, milieu, or combination), and experience level of therapists. Outcome variables were coded by type and target of assessment measure, yielding (a) child cognitive process (e.g., intellectual ability, cognitive processing), (b) child personality self-report (e.g., self-esteem, depression, anxiety), (c) parent self-report (e.g., parenting attitudes and behaviors, including abuse-specific attitudes and behaviors and other symptoms), (d) parent ratings of child

(e.g., internalizing and externalizing disorders), (e) teacher ratings of child (e.g., internalizing and externalizing disorders, classroom disruption, social skills), (f) objective behavioral observations of child (e.g., diagnostic interviews such as K-SADS-E, aggression, inappropriate sexual behaviors), and (g) objective behavioral observations of family (e.g., parent-child interactions). Control groups were coded as (a) no-treatment/wait list ( $n = 6$ ), (b) placebo ( $n = 5$ ), or (c) standard case management ( $n = 10$ ) conditions. Studies were also coded for quality of research design used (i.e., random assignment to treatment conditions, use of at least one standardized outcome measure, treatment sample greater than 50, and follow-up assessment; e.g., Durlak et al., 1991; Lipsey & Wilson, 1993).

### *Reliability of Coding Procedures*

Cohen's (1960) coefficient kappa was used to assess interrater reliability. Kappa is considered a conservative estimate because it reflects the proportion of agreement between raters, after adjusting for chance agreement (Tinsley & Weiss, 2000). Four research assistants were provided 8–12 hr of instruction and trained to a high level of agreement ( $\kappa = .85$ ) prior to beginning to code treatment studies. Following coding, kappas were calculated on 33% of the sample to assess coding reliability. Mean interrater agreement across pairs of coders ranged from  $\kappa = .62$  for outcome assessment variables to  $\kappa = .77$  for participant characteristics. Kappas ranging from .61 to .80 are considered "substantial" (Landis & Koch, 1977, p. 159).

### *Analysis of Effects*

Posttest differences between treatment and comparison groups were expressed using the standardized effect size,  $d_o$  (Cohen, 1988). Effects were calculated so that positive scores indicate that the treatment group outperformed the control group, whereas negative scores specify that the control group showed greater improvement. When information was not available to calculate effect sizes, they were estimated whenever possible on the basis of statistics that were reported using procedures outlined in Shadish, Robinson, and Lu (1999). In addition, when calculating effect sizes for studies in which "no effect" or "no significant effect" was reported for

a particular outcome, we followed the common practice of assigning a conservative effect size estimate of zero (e.g., Shadish et al., 1993; Weisz et al., 1995). Research has shown that use of conservative estimates may decrease effect size estimates by about 10–20% when compared with use of liberal estimates (e.g., deleting all effects for which "no effect" was reported) but that no differences in substantive conclusions seem to emerge (Shadish, 1992; Shadish et al., 1993).

Using guidelines developed by Hedges and Olkin (1985), we applied a weighted least squares approach to the analyses of effects. Comparisons between treatment and control groups were calculated using the standardized effect size,  $d_o$ , which was then corrected for small sample bias (Hedges & Olkin, 1985, Eq. 5.10). These unbiased effects ( $d$ ) were calculated because of the small sample sizes observed in this body of studies. It should be noted that the unbiased  $d$  differs only slightly from  $d_o$  as total sample size increases. Each unbiased  $d$  was then weighted by the inverse of its sampling error variance in all statistical analyses, where  $d_+$  represents the weighted unbiased effect size (Hedges & Olkin, 1985, Eq. 6.6 and 5.15), in order to more accurately estimate true population effects.

First, a single effect,  $d$ , was calculated for each study. Multiple effects were averaged to create an overall  $d_+$  per study. Treatment effects were calculated separately for each outcome construct identified. When a study reported several treatment effects for one of the eight outcome constructs, those effects were aggregated into a single effect per outcome construct assessed in that study. The same process was repeated for each of the other outcome constructs measured in each study. Next, we tested whether the effects reported for each study were homogeneous or similar in magnitude (Hedges & Olkin, 1985; Rosenthal & Rubin, 1986). Essentially, if a group of treatment effects are found to be homogeneous, it is reasonable to conclude that they represent a single population of effects, such as "CM treatment effects." If, however, there is significant heterogeneity in the overall group of effects, it suggests that two or more distinct subgroups of effects are present and further tests are needed to determine whether significant relationships exist between particular study characteristics (e.g., type of outcome measures, client characteristics, treatment characteristics, etc.) and size of treatment effects (Durlak, 1995). To be able to con-

clude within reason that a particular study characteristic (e.g., type of outcome measure such as parent self-report, behavioral observation, or child self-report) accounted for this variability in treatment outcomes, two additional criteria were required to be present: (a) significant heterogeneity of effects across groups and (b) significant homogeneity of effects within groups. Taking the study characteristic of "outcome construct," for example, we could conclude that significant relations between outcome construct assessed and size of treatment effects would exist if (a) effects associated with different outcome measures (e.g., parent self-report vs. behavioral observation) were heterogeneous and (b) effects within each of the groups of outcome measures are homogeneous. The  $Q$  statistic was calculated to test for homogeneity of effects across the sample of studies.  $Q$  shares a chi-square distribution and has  $(k - 1)$  degrees of freedom, where  $k$  represents the number of treatment comparisons.

## Results

Table 1 lists the 21 studies included in the meta-analysis, along with descriptions and average effect sizes,  $d$ . As shown, the majority of studies focused on the maltreated child and parent(s) as targets of intervention. Two studies only focused on treatment for maltreating parents (i.e., Burch & Mohr, 1980; Whiteman, Fanshel, & Grundy, 1987), and four others only targeted the maltreated child (i.e., Fantuzzo et al., 1988, 1996; Sullivan et al., 1992; Verleur, Hughes, & de Rios, 1986). Study sample sizes ranged from a high of  $n = 90$  (Deblinger & Lippman, 1996; Deblinger et al., 1999) to  $n = 16$  (Wolfe, Sandler, & Kaufman, 1981). Several studies were noteworthy for their use of random assignment to groups, at least one standardized outcome measure, and assessment of follow-up (i.e., Cohen & Mannarino, 1996, 1997; Deblinger & Lippman, 1996; Fantuzzo et al., 1996; Kolko, 1996a, 1996b; Moore et al., 1998; Wolfe et al., 1988).

Child participants averaged 6.28 ( $SD = 4.25$ ) years of age. The majority of studies ( $n = 12$ ) primarily targeted general CM or multiple victimization, whereas 7 studies focused primarily on treatment of child sexual abuse, 1 chiefly targeted physical abuse, and 1 other examined physical neglect. With respect to questions about comorbidity among types of CM, of the general maltreatment interventions, 2 of 12 studies re-

ported treating several cases of comorbid physical and sexual abuse (e.g., Kolko, 1996b; Moore et al., 1998), whereas 4 other studies did not specify the type or types of abuse treated. Among treatments for child sexual abuse, 3 listed specific exclusionary criteria (e.g., Cohen & Mannarino, 1996, 1997, 1998; Deblinger & Lippman, 1996; Deblinger et al., 1999), though it was unclear whether children who experienced multiple types of abuse (i.e., sexual abuse and physical abuse or neglect) were excluded from the study because comorbid abuse was not among the criteria for exclusion. Over half (57.1%,  $n = 12$ ) of studies involved random assignment of participants to treatment or control groups, and 5 of 21 studies reported follow-up data. Investigators provided treatment to mandated clients in 6 studies versus 12 studies conducted with volunteer clients, whereas client status (i.e., voluntary or mandated) was unclear in 3 studies. Twenty studies located their treatment approach within a theoretical orientation: that is, 12 studies examined a total of 16 behavior/cognitive-behavioral treatments (including 3 treatments in Deblinger & Lippman, 1996, 1999; 2 treatments in Fantuzzo et al., 1988; and 2 treatments in Kolko, 1996a, 3, 1996b), 3 studies examined nonbehavioral treatments (e.g., psychodynamic, humanistic, developmental-ecological), and 5 tested combination treatments.

The mean number of effects calculated for each study was  $M = 11.14$  ( $SD = 20.34$ ), on the basis of the use of  $M = 4.81$  ( $SD = 5.54$ ) instruments to assess treatment outcome. Effect sizes that exceeded more than 3 standard deviations from a study's mean effect were eliminated as outliers. Results revealed an overall WLS  $d_+ = .54$ ,  $SE = .03$ , 95% confidence interval (CI) = .39 to .69, for CM treatments in this sample, indicating that on average, 71% of treated participants were better off at posttreatment than the average untreated participant. A binomial effect size display (Rosenthal & Rubin, 1982) was calculated by translating mean differences into percentages of improved cases in treatment versus control groups. Results yielded a 64% probability of improvement for clients in the treatment conditions, whereas those in the control groups had a 36% rate of improvement. In other words, treatment increased the "improvement" rate for participants by 28%.

As shown in Table 1, the highest average effect per study ( $d = 1.54$ ) was reported for psychotherapy with sexually abused, deaf adolescents in

TABLE 1. Psychological Treatments of Child Maltreatment : Descriptive Information and Unbiased Effect Sizes (*d*) by Study

Study	Participants ( <i>N</i> )	Presenting problem	Design	Control group	Modality	Study <i>d</i>	Standardized outcome	Follow-up
1. Bagley & LaChance, 2000	11.5 y.o. girls & family w/offending parent (57)	2	1	1	2, multicomponent	1.01	Y	N
2. Burch & Mohr, 1980	abusing parents (31)	4	1	3	1, group	0.66	N	N
3. Celano et al., 1996	8–12 y.o. girls & nonoffending female caregivers (32)	2	2	2	1, multicomponent	0.33	Y	N
4. Cohen & Mannarino, 1996, 1997	3–6 y.o. preschool children & nonoffending parent (67)	2	2	2	1, individual (child & parent)	0.39	Y	Y
5. Cohen & Mannarino, 1998	7–15 y.o. children & nonoffending parent (49)	2	2	2	1, individual (child & parent)	0.15	Y	N
6. Culp et al., 1987	3.1 y.o. children & parents (68)	4	1	1	2, multicomponent	0.87	Y	N
7. Culp et al., 1991	4–6 y.o. children & parents (34)	4	1	1	2, multicomponent	0.65	Y	N
8. Deblinger et al., 1996, 1999	7–13 y.o. children & nonoffending parents (90)	2	2	3	(a) 1, individual—parent (b) 1, individual—child, (c) 1, conjoint (parent & child)	0.45 (a) 0.43 (b) 0.47 (c) 0.46	Y	Y
9. Elmer, 1986	infants & parents (62)	4	1	3	3, multicomponent	-0.18	Y	Y
10. Fantuzzo et al., 1988	withdrawn, 3–5 y.o. children (36)	4	2	2	(a) 1, individual w/peer, (b) 1, w/adult	0.42 (a) 0.83 (b) 0.00	Y	N
11. Fantuzzo et al., 1996	withdrawn, 3–5 y.o. children (20)	4	2	2	1, individual w/peer	0.87	Y	Y
12. Gaudin et al., 1990	families (51)	3	2	3	3, multicomponent	0.97	N	N
13. Kolko, 1996a & 1996b	8.6 y.o. children & parents (54)	4	2	3	(a) 1, individual (parent & child) (b) 1, family	0.17 (a) 0.28 (b) 0.07	Y	Y
14. Moore et al., 1998	1–2 y.o. children & family (48)	4	2	3	3, multicomponent	0.71	Y	Y
15. Sankey et al., 1985	infants & parents (40)	4	1	3	3, multicomponent	-0.06	Y	Y
16. Sullivan et al., 1992	12–16 y.o., hearing-impaired adolescents (72)	2	1	1	3, multicomponent	1.54	Y	N
17. Szykula & Fleischman, 1985	3–12 y.o. children & family (48)	4	2	3	1, family	0.28	N	N
18. Verleur et al., 1986	13–17 y.o. girls (30)	2	1	3	—, group	0.93	Y	N
19. Whiteman et al., 1987	parents of 10.5 y.o. children (24)	4	2	3	1, group	0.45	N	N
20. Wolfe et al., 1981	2–10 y.o. children & parents (16)	1	1	3	1, multicomponent	0.30	Y	Y
21. Wolfe et al., 1988	1–5 y.o. children & mothers (30)	4	2	2	1, multicomponent	0.35	Y	Y

Note. For presenting problems, 1 = child physical abuse; 2 = child sexual abuse; 3 = child physical neglect; 4 = general maltreatment. For design, 1 = nonequivalent control group; 2 = randomized true experiment. For type of control group, 1 = no treatment or wait list control; 2 = placebo control; 3 = case management. For treatment modality, 1 = behavioral; 2 = nonbehavioral; 3 = combination/electic. A “Y” for standardized outcome indicates the use of at least one standardized measure of outcome. A dash indicates that data were not reported. y.o. = year old; w/ = with; Y = Yes; N = No.

residential placement (Sullivan et al., 1992), whereas the lowest average treatment effect ( $d = -.18$ ) was associated with a residential treatment program for deprived infants (Elmer, 1986). Table 2 displays the unbiased effect sizes,  $d$ , associated with each treatment investigated.

First, homogeneity of effect sizes across all 21 studies was tested. As expected, results of the chi-square analysis were significant,  $\chi^2(20, N = 21) = 39.21, p < .01$ , indicating significant heterogeneity of effect sizes. In other words, when the unit of analysis was average effect size per study, the significant differences in treatment effect sizes indicated that at least two or more distinct subgroups of treatment effects were present in this sample of studies. Therefore, we tested for differences in effect sizes associated with type of outcome constructs assessed. Those outcome constructs represented by fewer than 10 effect sizes within and across studies were dropped from remaining analyses, yielding six outcome constructs per study that were considered: (a) child cognitive performance, (b) child self-report, (c) parent self-report, (d) parent ratings of child, (e) behavioral observation of child, and (f) behavioral observation of family. Effect sizes were homogeneous at  $p < .05$  for five of the six outcome constructs examined:  $d_+ = .28, Q_w(6) = 11.11$ , for child cognitive performance;  $d_+ = .44, Q_w(7) = 13.81$ , for child self-report;  $d_+ = .30, Q_w(3) = 3.20$ , for behavioral observation of child;  $d_+ = .53, Q_w(6) = 9.38$ , for parent self-report; and  $d_+ = .21, Q_w(1) = 0.15$ , for behavioral observation of family. Heterogeneity within effects was observed only for parent ratings of child:  $d_+ = .42, Q_w(8) = 16.32$ . Effect sizes, standard errors, and 95% confidence intervals associated with each of the outcome constructs are shown in Table 2. As shown, 95% confidence intervals did not include zero for any of the six categories of outcomes: child cognitive

performance, child self-report, parent ratings of child, behavioral observations of child, parent self-report, and behavioral observations of family.

Next, tests of treatment moderators were conducted to provide some initial benchmarking for the field. These analyses examined relations between study characteristics (i.e., type of CM targeted, mandated vs. volunteer, treatment modality, theoretical orientation, type of control group, and quality of study design) and size of treatment effects. First, differences in effect sizes were observed by type of control group: Significant homogeneity within groups  $Q_w(18) = 21.51$ , and heterogeneity across groups ( $Q_b = 17.70, n = 3$ ) occurred. Specifically, treatment effects were larger when compared with no-treatment control groups ( $d_+ = .99, SE = .14, n = 6$ ), than when contrasted with either placebo ( $d_+ = .38, SE = .16, n = 5$ ) or case management groups ( $d_+ = .35, SE = .10, n = 10$ ). Treatment effects also varied as a function of theoretical orientation ( $Q_b = 9.41, n = 3$ ), though effects were homogeneous only within the group of nonbehavioral treatments ( $d_+ = .87, SE = .17, Q_w(2) = .64$ ) and group of behavioral treatments ( $d_+ = .40, SE = .10, Q_w(11) = 1.70$ ), but not homogeneous in the group of mixed/combination treatments ( $d_+ = .59, SE = .13, Q_w(4) = 27.46$ ). Post hoc analysis indicated that length of treatment was significantly different for behavioral treatments ( $M = 2.9$  months,  $SD = 1.2$ ) versus nonbehavioral treatments ( $M = 13.5$  months,  $SD = 9.1$ ):  $F(1, 9) = 12.63, p = .006$ . That is, nonbehavioral treatments were considerably lengthier treatments than were the behavioral interventions.

Correlations between treatment orientation and quality of study design were not significant:  $r = -.27, p = .32$ . Variations in treatment effects by type of CM were observed. Because of the small number of studies that only examined treatments

TABLE 2. Weighted Mean Effect Sizes by Outcome Construct

Outcome construct	$N^a$	Effect size ( $n$ ) <sup>b</sup>	$SE$	95% confidence interval	Fail-safe $N_s^c$
Child cognitive process	7	.28 (22)	.07	.11-.42	3
Child personality self-report	8	.44 (43)	.05	.34-.54	10
Parent ratings of child behavior	9	.42 (34)	.06	.30-.54	10
Behavioral observations of child	4	.30 (13)	.10	.10-.50	2
Parent self-report	7	.53 (42)	.05	.43-.63	12
Behavioral observations of family	2	.21 (14)	.08	.05-.37	0

<sup>a</sup> Number of studies. <sup>b</sup> Number of effects calculated per outcome construct. <sup>c</sup> Number of additional studies with null results needed to reduce mean effect size to 0.2.

for child physical abuse ( $n = 1$ ) and physical neglect ( $n = 1$ ), we contrasted effect sizes associated with treatments for primary sexual abuse ( $n = 7$ ) and treatments for general CM ( $n = 12$ ; e.g., Burch & Mohr, 1980). Effects for child sexual abuse interventions ( $d_+ = .69$ ,  $SE = .09$ ,  $n = 7$ ) were larger compared with interventions for general CM ( $d_+ = .40$ ,  $SE = .12$ ,  $n = 12$ ),  $Q_b(1) = 6.26$ ,  $p > .05$ . Though effects were significantly different by primary type of abuse treated, surprisingly, group effects were homogeneous for general maltreatment interventions,  $Q_w(11) = 14.40$ ,  $p < .05$ , and heterogeneous for sexual abuse treatments,  $Q_w(6) = 18.55$ ,  $p > .05$ , meaning that significant variability existed among the group of effects for treatment of child sexual abuse. No other statistically significant relations between study characteristics and CM treatment effects emerged in these follow-up analyses. Variations in treatment effect sizes did not emerge for treatment modality: individual ( $d_+ = .39$ ,  $SE = .13$ ,  $n = 8$  treatments), group ( $d_+ = .69$ ,  $SE = .16$ ,  $n = 3$  treatments), family ( $d_+ = .28$ ,  $SE = .11$ ,  $n = 3$  treatments), and multilevel ( $d_+ = .64$ ,  $SE = .11$ ,  $n = 11$  treatments). No significant differences in magnitude of effects were observed between mandated ( $d_+ = .70$ ,  $SE = .14$ ,  $n = 6$ ) and voluntary ( $d_+ = .49$ ,  $SE = .10$ ,  $n = 11$ ) treatments. In addition, no relationship was observed between treatment effectiveness and quality of study design:  $r = .03$ ,  $p = .89$ , suggesting that studies characterized by use of more rigorous research methods produced neither consistently larger nor smaller treatment effects. However, research design quality was found to covary with year of publication:  $r = .61$ ,  $p < .01$ , indicating that recently published studies used more rigorous methodological designs.

To be able to examine the likelihood of whether our review of published studies of psychological treatments for CM may have overestimated the true size of treatment effects because of publication bias, we calculated fail-safe  $N$ s. Using Orwin's (1983) formula, we calculated that an additional 36 treatment studies with null findings would be required to reduce the overall mean treatment effect to 0.2—Cohen's (1988) estimate for a small effect size. Next, fail-safe  $N$ s were calculated for effects associated with each of the six outcome categories. As shown in Table 2, the number of studies with null findings that would be needed to reduce the mean treatment effect to 0.2 for each outcome was lower than 36,

ranging from 12 studies for effects associated with parent self-reports to 0 studies for behavioral observations of the family.

Finally, average effect sizes at follow-up tentatively indicate that—at least in a few circumstances—treatment gains may be maintained at 3 months ( $d = .30$ ,  $n = 2$  studies reporting), 6 months ( $d = .16$ ,  $n = 3$ ), 1 year ( $d = .50$ ,  $n = 2$ ), 2 years ( $d = .69$ ,  $n = 1$ ), and 12 years posttreatment ( $d = .49$ ,  $n = 1$ ). Because of the paucity of follow-up data, these should be viewed cautiously.

## Discussion

Results of this meta-analysis suggest that psychological treatments for CM yielded improvements among participants, compared with waitlist, placebo, or community case management control groups drawn from the same population. Specifically, after intervention, treated CM clients appeared to be functioning better than 71% of their nontreated counterparts. The average weighted effect was  $d_+ = .54$ , which is considered a medium effect size (Cohen, 1988). When placed in the context of other meta-analyses of psychological treatments for child-identified problems, this set of CM interventions appears about as effective as those reviewed in other published meta-analyses of child, adolescent (Casey & Berman, 1985; Durlak et al., 1991; Kazdin et al., 1990; Weisz et al., 1987; Weisz et al., 1995), and family therapies (Hazelrigg et al., 1987; Shadish et al., 1993). Moderate differences in the estimated magnitude of treatment effects across these meta-analyses are likely due in part to the data analytic strategies (i.e., ULS vs. WLS) used. The ULS approach typically yields larger effects because of the assumption of homogeneity of variance across individual effect sizes (though in meta-analysis, the assumption is typically violated because of differences in variances of effect sizes across studies; Hedges & Olkin, 1985). In contrast, a WLS approach adjusts for heterogeneity of variance across individual studies in the meta-analysis, typically producing lower mean effect sizes than meta-analyses that use a ULS model. Keeping in mind the differences in analytic procedures applied across the various meta-analyses of child and family treatment outcomes, we find that overall results appear to provide some initial support for use of these psychological treatments for children and

families who have experienced some form of CM.

### *Moderators of Treatment Effectiveness*

*Operationalization of treatment outcomes.* As shown in Table 2, treatment effects systematically varied by type, target, and source of outcome construct. In general, larger effects were associated with parent self-reports ( $d_+ = .53$ ), child self-reports ( $d_+ = .44$ ), and parent ratings of child ( $d_+ = .42$ ), whereas objective behavioral observations of the family yielded smaller treatment gains ( $d_+ = .21$ ). These findings are consistent with a growing body of research demonstrating that different operationalizations of treatment outcome account for considerable variance in treatment effects (Weisz et al., 1995; Wilson & Lipsey, 2001). At present, use of comprehensive assessments that include behavioral observations in addition to parent and child reports is still rare in CM treatment studies (Ammerman, 1998). Although self-reported indices of improvement may be better designed to access the internal, covert experiences of clients, among CM treatment studies, there exists a historical overreliance on use of self-report and parent reports of child behaviors to document outcome (Fantuzzo & Twentyman, 1986). Parent reports tend to be skewed and biased among these families (Reid, Kavanagh, & Baldwin, 1987), and the very nature of CM as a family systems problem leaves parents susceptible to providing socially desirable responses indicating that they and their children have improved as a result of treatment (Ammerman, 1998). Note that when the targets of change were observer-rated (i.e., behavioral improvements in children or parent-child interactions), treatment gains were smaller. Although evidence of positive behavioral change may be more difficult to obtain, behaviorally based evidence (i.e., reductions in reabuse and recidivism rates) is necessary to conclude with confidence that psychological treatments for CM are effective. However, given that behavioral observation measures may fail to capture other important internal or covert abuse-related symptoms that may not easily lend themselves to use of objective observational methods (e.g., child internalizing problems; Weisz et al., 1995), self-report methodologies should be used within multidimensional assessments of CM intervention effectiveness.

Taken together, these findings point to the need for investigators to use a multi-trait, multi-method, and multi-source approach to assessment of CM treatment outcomes to better elucidate treatment and participant characteristics that are associated with greater intervention effectiveness. CM researchers are encouraged to utilize multi-source and multi-method assessment protocols, in order to evaluate change from multiple perspectives (e.g., child and parent self-report, trained observer, therapist, teachers, and significant others/family members) using a variety of methods (e.g., surveys, interviews, behavioral observations, and school/academic records), focusing on multiple targets in the family system (e.g., child victims, siblings, nonoffending parents, perpetrator in cases of intrafamilial abuse, and the family system). Although no single study could include all of these dimensions, future CM treatment studies need to incorporate at least two different methods and sources of measurement. For example, a study of family systems treatment for intrafamilial child physical abuse could include assessments of both parent (or parents) and child, through parent report of self and child functioning, direct child assessment, and observer ratings of parent-child interactions. In addition, future summaries of CM treatment research would be enhanced by including documentation in original studies about the following: basic demographic information for treated family members; incidents of reabuse; exits and reentries of family members (i.e., victims and perpetrators) into/out of the home before, during, and after treatment; nature of family involvement with the criminal justice system; all targets of treatment; and in cases of intrafamilial abuse, information-tracking status of and treatment for perpetrators.

*Type of maltreatment.* Treatments for child sexual abuse yielded slightly larger effects ( $d_+ = .69$ ) than those for general CM ( $d_+ = .40$ ), yet significant within-group variance among the sexual abuse treatment effects rendered these differences moot. These findings indicate that other critical moderators of child sexual abuse treatment effectiveness, such as length of treatment, severity of abuse, and/or abuse comorbidity, need further identification. CM researchers (e.g., Ammerman, 1998; Emery & Laumann-Billings, 1998; Oates & Bross, 1995; Shirk & Eltz, 1998) have lamented the fact that CM treatment studies often do not contain sufficient information re-

garding these potentially critical moderators of treatment. For example, studies that target one primary type of maltreatment often fail to report any information on possible co-occurrence of other types of abuse (Shirk & Eltz, 1998), although just as comorbidity of psychological disorders is the norm during childhood, comorbidity among types of CM experienced is commonplace (Belsky, 1993; Cicchetti & Olsen, 1990; U.S. Department of Health & Human Services, 2001). Studies that purport to examine a specific type of abuse frequently contain subsets of children who have experienced other forms of victimization (Shirk & Eltz, 1998), making it difficult to determine the extent to which various forms of treatment are differentially effective for different types of maltreatment. Similarly, data collected in the current meta-analysis indicate that until more thorough assessments of presenting problems and types of CM experienced are consistently made, conclusions that may be drawn regarding the effectiveness of interventions for specific forms of maltreatment will be limited.

Further, we suspect that severity of maltreatment and resulting symptoms are other important moderators of CM treatment effectiveness (e.g., Emery & Laumann-Billings, 1998). Because CM is composed of a diverse set of experiences and is not a psychological disorder in and of itself, those who participate in treatment are likely to display diverse symptom patterns (i.e., severity of maltreatment and type and severity of symptoms), regardless of similarity of maltreatment events, with some children not appearing to evidence any significant psychopathology (Finkelhor & Berliner, 1995; Saywitz et al., 2000). Future CM intervention studies must carefully specify the severity of victimization experiences, types of maltreatment experienced, and severity of symptomatology (e.g., Barnett, Manly, & Cicchetti, 1993) in order to facilitate use of more advanced methods to evaluate individual differences in the effectiveness of CM treatments (e.g., attribute-by-treatment interaction designs; Shoham-Salomon & Hannah, 1991; Snow, 1991; hierarchical linear modeling; Bryk & Raudenbush, 1992) and to assist treatment providers who are increasingly being required to triage their services in order to be cost-effective and survive in the age of managed care.

*Type of control group.* Consistent with prior meta-analyses of general child (Casey & Berman, 1985; Kazdin et al., 1990; Weisz et al., 1987,

1995) and adult therapies (Lambert, Weber, & Sykes, 1993), larger treatment effects were observed when CM treatments were compared with no-treatment/wait-list controls ( $d_+ = .99$ ) than with either placebo ( $d_+ = .38$ ) or case-management ( $d_+ = .35$ ) controls. Among the sample of treatment studies reviewed here, the majority ( $n = 10$ ) involved typical community case management control conditions, whereas only 6 studies used a no-treatment or wait-list control condition and 5 used a placebo control. Indeed, ethical and legal considerations often preclude the use of no-treatment or placebo control groups in CM treatment studies, such as when children in control conditions remain in continued danger and experience an extended duration of suffering or when child protective services mandate treatment (Ammerman, 1998). Given that a growing number of empirical studies have documented the effectiveness of several treatments for CM, control group conditions that withhold delivery of therapeutic interventions to maltreated children and their families may no longer be ethically or legally defensible. Along these lines, the National Institute of Mental Health (Hyman & Shore, 2000) recently released a position paper on the use of control groups in clinical trials for depressive and bipolar disorders:

National Institute of Mental Health sponsored trials should include placebo conditions when alternative designs would not provide replicable and generalizable results. Trial designs, however, should minimize risk to individuals insofar as they can . . . for studies in which suffering is extreme, and where safe and effective treatments are available, ethical questions will occur. (pp. 690–691)

Thus, although it may not be feasible to use traditional no-treatment control groups with CM populations, treatment researchers may randomly assign participants to an alternative psychological treatment whose effectiveness has been empirically established or use a community case management condition that is more thoroughly documented. Use of such options would represent improvements over many existing studies that tend to evaluate a single CM treatment by assessing participants pre- and posttreatment, using no form of comparison/control group or a comparison group characterized by involvement in community services that are vaguely described and/or not under investigator control (Ammerman, 1998). In sum, continued enhancements to the development of viable comparison/control groups are

needed to allow for more rigorous evaluation of the effectiveness of CM treatments, while maintaining ethical and legal standards of care.

Finally, relative to other forms of CM, treatments for child sexual abuse have been the focus of a greater proportion of existing treatment outcome studies to date. Although national estimates suggest that roughly 10% of maltreated children are sexually abused (NCCAN, 2000), just over 30% of the studies included in this sample investigated treatments for primary child sexual abuse. By comparison, roughly two thirds of maltreated children experience neglect (physical, emotional, educational, and/or medical), yet only one study of treatment for primary child neglect met criteria for inclusion in this meta-analysis.

*Therapy orientation.* Although the small number of nonbehavioral interventions reviewed here ( $n = 3$ ;  $d_+ = .87$ ) yielded larger treatment effects than did behavioral treatments ( $n = 12$ ;  $d_+ = .40$ ), post hoc analyses revealed that length of treatment covaried significantly with theoretical orientation. Specifically, among studies reporting data on length of treatment for CM (i.e.,  $n = 8$  behavioral and  $n = 3$  nonbehavioral treatments), nonbehavioral interventions (i.e., interpersonal/humanistic/psychodynamic) averaged about 1 year whereas behavioral treatments were only about 3 months long. Although these findings should be considered preliminary because of the small number of studies in question, one leading explanation for differences in treatment effects by therapy orientation is that the few nonbehavioral treatments investigated to date were more effective because participants received *more* treatment than did those participants who received behavioral interventions. To better understand whether treatment gains are a function of length rather than theoretical orientation, it would be helpful to learn whether brief nonbehavioral treatments (i.e., brief interpersonal therapy or time-limited dynamic psychotherapy) developed for CM would also show effect sizes smaller than longer term (1+ year) CM treatments and on par with the effects associated with the behavioral treatments included here. Such findings would lend support to the notion that dosage is predictably related to gains in CM treatment (see also Howard, Kopta, Krause, & Orlinsky, 1986). Other plausible explanations (e.g., systematic variation in severity of presenting problems, multiple subtypes of

abuse experienced, and therapist skill level) may exist as well, yet inconsistent reporting of these and other potentially important moderators of treatment effectiveness (i.e., age, gender, ethnicity, family income, parent alcohol/substance abuse, and child cognitive development level; Erickson & Egeland, 2002; Kolko, 2002) precluded analysis of their role in predicting differential treatment outcomes. More definitive conclusions regarding the relationship between theoretical orientation, treatment length, and outcomes await publication of additional methodologically sound CM treatment studies that provide detailed reports of sample characteristics.

*Treatment modality.* Other exploratory analyses indicated that effects did not vary on the basis of modality of treatments used (e.g., individual, group, family, milieu, or multilevel forms of treatment). Similarly, treatment outcomes associated with individual versus group therapies (Casey & Berman, 1985; Weisz et al., 1987) or for parent- versus child-focused treatments (Casey & Berman, 1985) appear equally effective, suggesting that participants may have some flexibility in choosing their preferred mode of CM treatment, as each form appears to lead to positive treatment gains. It would be interesting to learn whether some choice in modality would increase the likelihood of engagement in, compliance with, and completion of treatment—often-problematic issues for professionals working with CM families (Kolko, 1998).

*Mandated versus voluntary treatment.* Treatment effects did not appear to vary as a function of whether or not participants were mandated for treatment, suggesting that both mandated and voluntary forms of CM treatment as represented in these studies may be similarly effective. This finding was somewhat surprising, as it would seem logical to expect that there would be differences in improvement rates among those mandated for treatment versus participants who volunteered to receive services. Yet, in practice, the circumstances in which families are targeted for CM treatment may always involve some element of involuntariness or the experience of coercion, and, frequently, resistance is high (Azar & Wolfe, 1998). Thus, the mandated versus voluntary distinction may be a difficult one to discern. Although the extent to which treatment is mandated or voluntary may impact adults' responses to treatment, it also may be salient for children who, in essence, are required to attend treatment by

important adults in their lives (Friedberg & McClure, 2002; Kendall, 2000).

*Study design.* We observed that design quality covaried with year of publication, with more recently published studies applying more rigorous methodological designs. Nonetheless, more rigorously designed studies that involved random assignment, standardized outcome measures, larger samples, and follow-up assessment did not yield larger (or smaller) treatment gains than those studies using less rigorous designs. Although the quality of treatment outcome study designs has important implications for the internal and external validity of results obtained, recent methodological analyses of psychotherapy effectiveness studies (i.e., Wilson & Lipsey, 2001) suggest that variations in design quality result neither in consistent over- or underestimates of treatment effects but operate instead as sources of random error.

*Follow-up treatment effects.* Though a small number of follow-up assessments provided preliminary evidence that treatment gains may continue from 3 months to 12 years posttreatment, generalization from these studies is not yet possible, and the specific conditions under which these treatment gains were maintained (i.e., with whom, under what conditions, in which types of therapy) remain unclear. In response, we encourage CM treatment researchers to better track and report client attrition rates, conduct intent-to-treat analyses, and obtain follow-up data on all clients completing treatments, because professionals who work with this group have long asserted that “sleeper effects,” defined as the development of serious psychological symptoms at some point in time after termination of abuse, are likely present among victims of CM (Finkelhor & Berliner, 1995). Likewise, assessment of neurobiological correlates of CM together with collection of longer-term follow-up data may help to elucidate (a) the mechanisms through which sleeper effects operate, (b) what forms such symptoms may take over time and across developmental levels, (c) whether sleeper effects can be averted through early interventions (Finkelhor & Berliner, 1995), and (d) specific ways in which CM treatments may lead to long-term improvements in functioning.

In conclusion, the current meta-analysis provides one of the first quantitative summaries of the effectiveness of treatments for CM and indicates that across a variety of psychological inter-

ventions for CM, treated children and their parents report positive changes in adjustment in comparison with their nontreated counterparts. Caution is urged in interpreting these collective findings because self-reported outcomes were associated with larger treatment gains (i.e., “moderate” effect sizes; Cohen, 1988), whereas effects associated with objective behavioral observations of parent–child and family interactions were considerably smaller (i.e., “small” effects; Cohen, 1988), about one half the size of self-report-based treatment effects. Much remains to be understood about whether CM treatments can consistently produce positive behavioral change, the conditions under which treatments for CM are most effective, and the mechanisms through which effective treatments facilitate meaningful behavioral improvements (i.e., reductions in recidivism and reabuse). To answer these questions, researchers will need to devote greater attention to identifying and examining potential moderators of treatment effectiveness (i.e., participant characteristics, treatment characteristics, and type, source, and target of outcomes assessed), increasing quality of study designs (i.e., including attrition rates and intent to treat analyses), and incorporating multidimensional assessments into future CM treatment outcome studies. Furthermore, process research that assesses quality of the therapeutic alliance (Horvath, 2001), stages of change (Prochaska & Norcross, 2001), and other key change events (e.g., commitment to engage; Friedlander, Heatherington, Johnson, & Skowron, 1994) is needed to shed light onto the mechanisms through which CM treatments exert their influence.

Finally, findings from the current investigation have implications for social policy and practice. Our results indicate that various treatments of CM are effective and provide evidence for funding agencies to continue to support research in this area. Although our results are encouraging, caution must be used in interpreting these meta-analytic findings given the number of well-designed treatment outcome studies completed to date. Continued support for high quality CM intervention research is essential to build on these current findings in order to reach more definitive conclusions about CM treatment effectiveness.

Furthermore, results highlight the need for scientists and practitioners to work together to create meaningful and relevant outcomes assessment protocols that not only provide self-report infor-

mation but that also measure "real world" outcomes (e.g., into/out of home placements, incidents of reabuse, family involvement with the criminal justice system, etc.). Results also suggest the importance of evaluating children and families for multiple forms of victimization given that comorbidity of CM types appears to be commonplace. Comprehensive assessment that identifies child victims of maltreatment along a continuum of severity of psychological reactions (from no symptoms to manifestation of psychiatric disorders) also will help practitioners identify the level of intervention needed. For families with youngsters who evidence mild symptomatology, a psychoeducational approach may best suit their needs, whereas those who evidence moderate levels of symptomatology may benefit from time-limited, abuse-focused, cognitive-behavioral therapy (Saywitz et al., 2000). Finally, multicomponent, longer term treatment may best be reserved for those who experienced early, severe victimization and who evidence significant psychopathology; though, to date, there is little empirical evidence to provide definitive suggestions regarding the form(s) of treatment for maltreated children who present with more complicated psychological profiles (Saywitz et al., 2000). A lack of attention to matching the level of services needed also may contribute to high rates of treatment dropout and high rates of recidivism. Thus, one challenge for the field is to obtain more specific information regarding severity of abuse and related symptomatology. This information can then be used to (a) identify an appropriate level of intervention, (b) more accurately assess the effectiveness of an intervention by taking into account severity of reactions to CM, and (c) determine the role that severity of CM plays in treatment compliance and revictimization.

Our follow-up analyses also suggest that positive gains following treatment occur for both mandated and volunteer clients and highlight the potential benefits for practitioners who persevere with mandated clients who are resistant to treatment. Further, would mandating services prove effective in increasing parental participation in the treatment process? Given these initial findings, more research is needed that considers the impact on treatment outcomes of the conditions under which maltreated children and their families are referred. In addition, though the systemic nature of CM would suggest that treatments that

engage families may (a) lead to more lasting gains, (b) reduce the risk of revictimization, and (c) alter the cycle of intergenerational transmission of abuse, these ideas await future clinical trials that include more comprehensive assessment of children, parents, and the family system at posttreatment and again at follow-up. Finally, agencies that provide treatments for victims of CM are encouraged to promote the education and training of staff in recent advances in interventions that have been shown to be effective and to explore their implementation in broader community settings (e.g., National Institute of Mental Health, 1999, 2000). Although the prevention and eradication of CM is an ultimate societal goal, the fact remains that an overwhelming number of children will become victims (NCCAN, 2000). Results of the current investigation suggest that treatments for CM may be effective and may assist maltreated children and their families to cope and facilitate the development of healthy, productive lives.

## References

- References marked with an asterisk indicate studies included in the meta-analysis.
- AMMERMAN, R. T. (1998). Methodological issues in child maltreatment research. In J. R. Lutzker (Ed.), *Handbook of child abuse research and treatment* (pp. 117-132). New York: Plenum Press.
- AZAR, S. T., & TWENTYMAN, C. T. (1986). Cognitive-behavioral perspectives on the assessment and treatment of child abuse. In P. C. Kendall (Ed.), *Advances in cognitive-behavioral research and therapy* (Vol. 5, pp. 237-267). New York: Academic Press.
- AZAR, S. T., & WOLFE, D. A. (1998). Child physical abuse and neglect. In E. J. Mash & R. A. Barkley (Eds.), *Treatment of childhood disorders* (pp. 451-493). New York: Guilford Press.
- \*BAGLEY, C., & LACHANCE, M. (2000). Evaluation of a family-based programme for the treatment of child sexual abuse. *Child and Family Social Work*, 5, 205-213.
- BANGERT-DROWNS, R. L. (1986). Review of developments in meta-analytic method. *Psychological Bulletin*, 101, 213-232.
- BARNETT, D., MANLY, J. T., & Cicchetti, D. (1993). Defining child maltreatment: The interface between policy and research. In D. Cicchetti & S. L. Toth (Eds.), *Child abuse, child development, and social policy* (pp. 7-73). Norwood, NJ: Ablex.
- BEITCHMAN, J. H., ZUCKER, K. J., HOOD, J. E., DACOSTA, G. A., & AKMAN, D. (1991). A review of the short-term effects of child sexual abuse. *Child Abuse & Neglect*, 15, 537-556.
- BELSKY, J. (1993). Etiology of child maltreatment: A developmental-ecological analysis. *Psychological Bulletin*, 114, 413-434.

- BERLINER, L., & ELLIOTT, D. M. (2002). Sexual abuse of children. In J. E. B. Myers, L. Berliner, J. Briere, C. T. Hendrix, C. Jenny, & T. A. Reid (Eds.), *The APSAC handbook on child maltreatment* (2nd ed., pp. 55–78). Thousand Oaks, CA: Sage.
- BERRICK, J. D. (1997). Child neglect: Definition, incidence, and outcomes. In J. D. Berrick, R. Barth, & N. Gilbert (Eds.), *Child welfare research review: Vol. II* (2nd ed., pp. 1–12). New York: Columbia University Press.
- BROWNE, A., & FINKELHOR, D. (1986). Impact of child sexual abuse: A review of the research. *Psychological Bulletin*, *99*, 66–77.
- BRYK, A. S., & RAUDENBUSH, S. W. (1992). *Hierarchical linear models: Applications and data analysis methods*. Thousand Oaks, CA: Sage.
- \*BURCH, G., & MOHR, V. (1980). Evaluating a child abuse intervention program. *Social Casework: The Journal of Contemporary Social Work*, *61*, 90–100.
- CAMPBELL, D. T., & STANLEY, J. C. (1963). *Experimental and quasi-experimental designs for research*. Boston: Houghton Mifflin.
- CASEY, R. J., & BERMAN, J. S. (1985). The outcome of psychotherapy with children. *Psychological Bulletin*, *98*, 388–400.
- \*CELANO, M., HAZZARD, A., WEBB, C., & MCCALL, C. (1996). Treatment of traumagenic beliefs among sexually abused girls and their mothers: An evaluation study. *Journal of Abnormal Child Psychology*, *24*, 1–17.
- Centers for Disease Control and Prevention. (2004, January). *WISQARS leading causes of deaths reports*. Retrieved April 5, 2004 from <http://webapp.cdc.gov/sasweb/ncipc/leadcaus10.html>
- CICCHETTI, D., & BARNETT, D. (1991). Toward the development of a scientific nosology of child maltreatment. In D. Cicchetti & W. Grove (Eds.), *Thinking clearly about psychology: Essays in honor of Paul E. Meehl* (pp. 346–377). Minneapolis, MN: University of Minnesota Press.
- CICCHETTI, D., & OLSEN, K. (1990). The developmental psychopathology of child maltreatment. In M. Lewis & S. M. Miller (Eds.), *Handbook of developmental psychopathology* (pp. 261–279). New York: Plenum Press.
- COHEN, J. (1960). A coefficient of agreement for nominal scales. *Educational and Psychological Measurement*, *20*, 37–46.
- COHEN, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). Hillsdale, NJ: Erlbaum.
- \*COHEN, J. A., & MANNARINO, A. P. (1996). A treatment outcome study for sexually abused preschool children: Initial findings. *Journal of the American Academy of Child and Adolescent Psychiatry*, *35*, 42–50.
- \*COHEN, J. A., & MANNARINO, A. P. (1997). A treatment study for sexually abused preschool children: Outcome during a one-year follow-up. *Journal of the American Academy of Child and Adolescent Psychiatry*, *36*, 1228–1235.
- \*COHEN, J. A., & MANNARINO, A. P. (1998). Interventions for sexually abused children: Initial treatment outcome findings. *Child Maltreatment: Journal of the American Professional Society on the Abuse of Children*, *3*, 17–26.
- COHN, A. H., & DARO, D. (1987). Is treatment too late: What ten years of evaluative research tells us. *Child Abuse & Neglect*, *11*, 433–442.
- CONGER, R. D. (1982). Behavioral intervention for child abuse. *The Behavior Therapist*, *5*, 49–53.
- COX, A. D. (1998). Preventing child abuse: A review of community-based projects II: Issues arising from reviews and future directions. *Child Abuse Review*, *7*, 30–43.
- \*CULP, R. E., HEIDE, J., & RICHARDSON, M. T. (1987). Maltreated children's developmental scores: Treatment versus nontreatment. *Child Abuse & Neglect*, *11*, 29–34.
- \*CULP, R. E., LITTLE, V., LETTS, D., & LAWRENCE, H. (1991). Maltreated children's self-concept: Effects of a comprehensive treatment program. *American Journal of Orthopsychiatry*, *61*, 114–121.
- DARO, D., & DONNELLY, A. C. (2002). Charting the waves of prevention: Two steps forward, one step back. *Child Abuse and Neglect*, *26*, 731–742.
- DAVIS, M. K., & GIDYCH, C. A. (2000). Child sexual abuse prevention programs: A meta-analysis. *Journal of Clinical Child Psychology*, *29*, 257–265.
- \*DEBLINGER, E., & LIPPMAN, J. (1996). Sexually abused children suffering posttraumatic stress symptoms: Initial treatment outcome findings. *Child Maltreatment*, *1*, 310–322.
- \*DEBLINGER, E., STEER, R. A., & LIPPMAN, J. (1999). Two-year follow-up study of cognitive behavioral therapy for sexually abused children suffering posttraumatic stress symptoms. *Child Abuse & Neglect*, *23*, 1371–1378.
- DELUCA, R. V., BOYES, D. A., GRAYSTON, A. D., & ROMANO, E. (1995). Sexual abuse: Effects of group therapy on pre-adolescent girls. *Child Abuse Review*, *4*, 263–277.
- DURLAK, J. A. (1995). Understanding meta-analysis. In L. G. Grimm & P. R. Yarnold (Eds.), *Reading and understanding multivariate statistics* (pp. 319–352). Washington, DC: American Psychological Association.
- DURLAK, J. A., FUHRMAN, T., & LAMPMAN, C. (1991). Effectiveness of cognitive-behavioral therapy for mal-adapting children: A meta-analysis. *Psychological Bulletin*, *110*, 204–214.
- DURLAK, J. A., & WELLS, A. M. (1997). Evaluation of indicated preventive intervention (secondary prevention) mental health programs for children and adolescents. *American Journal of Community Psychology*, *26*, 775–802.
- \*ELMER, E. (1986). Outcome of residential treatment for abused and high-risk infants. *Child Abuse & Neglect*, *10*, 351–360.
- EMERY, R. E. (1989). Family violence. *American Psychologist*, *44*, 321–328.
- EMERY, R. E., & LAUMANN-BILLINGS, L. (1998). An overview of the nature, causes, and consequences of abusive family relationships. *American Psychologist*, *53*, 121–135.
- ERICKSON, M. F., & EGELAND, B. (2002). Child neglect. In J. E. B. Myers, L. Berliner, J. Briere, C. T. Hendrix, C. Jenny, & T. A. Reid (Eds.), *The APSAC handbook on child maltreatment* (2nd ed., pp. 3–20). Thousand Oaks, CA: Sage.
- \*FANTUZZO, J. W., JURECIC, L., STOVALL, A., HIGHTOWER, A. D., GOINS, C., & SCHACHTEL, D. (1988). Effects of adult and peer social initiations on the social

- behavior of withdrawn, maltreated preschool children. *Journal of Consulting and Clinical Psychology*, 56, 34–39.
- \*FANTUZZO, J., SUTTON-SMITH, B., ATKINS, M., MEYERS, R., STEVENSON, H., COOLAHAN, K., ET AL. (1996). Community-based resilient peer treatment of withdrawn, maltreated preschool children. *Journal of Consulting and Clinical Psychology*, 64, 1377–1386.
- FANTUZZO, J. W., & Twentyman, C. T. (1986). Child abuse and psychotherapy research: Merging social concerns and empirical investigation. *Professional Psychology: Research & Practice*, 17, 375–380.
- FINKELHOR, D. (1988). The trauma of child sexual abuse: Two models. In G. E. Wyatt & G. J. Powell (Eds.), *Lasting effects of child sexual abuse* (pp. 61–82). Beverly Hills, CA: Sage.
- FINKELHOR, D. (1990). Early and long-term effects of child sexual abuse: An update. *Professional Psychology: Research and Practice*, 21, 325–330.
- FINKELHOR, D., & BERLINER, L. (1995). Research on the treatment of sexually abused children: A review and recommendations. *Journal of the American Academy of Child and Adolescent Psychiatry*, 34, 1408–1423.
- FRIEDBERG, R. D., & McCLURE, J. M. (2002). *Clinical practice of cognitive therapy with children and adolescents: The nuts and bolts*. New York: Guilford Press.
- FRIEDLANDER, M.L., HEATHERINGTON, L., JOHNSON, B., & SKOWRON, E.A. (1994). Sustaining engagement: A change event in family therapy. *Journal of Counseling Psychology*, 41, 438–448.
- \*GAUDIN, J. M., WODARSKI, J. S., ARKINSON, M. K., & AVERY, L. S. (1990). Remedying child neglect: Effectiveness of social network interventions. *Journal of Applied Social Sciences*, 15, 97–123.
- HAZELRIGG, M. D., COOPER, H. M., & BORDUIN, C. M. (1987). Evaluating the effectiveness of family therapies: An integrative review and analysis. *Psychological Bulletin*, 101, 428–442.
- HEDGES, L. V., & OLKIN, I. (1985). *Statistical methods for meta-analysis*. San Diego, CA: Academic Press.
- HORVATH, A. O. (2001). The alliance. *Psychotherapy: Theory, Research, Practice, Training*, 38, 365–372.
- HOWARD, K. I., KOPTA, S. M., KRAUSE, M. S., & ORLINSKY, D. E. (1986). The dose-effect relationship in psychotherapy. *American Psychologist*, 41, 159–164.
- HYMAN, S., & SHORE, D. (2000). An NIMH perspective on the use of placebos. *Biological Psychiatry*, 47, 689–691.
- KAPLAN, S. J., PELCOVITZ, D., & LABRUNA, V. (1999). Child and adolescent abuse and neglect research: A review of the past 10 years. Pt. I: Physical and emotional abuse and neglect. *Journal of the American Academy of Child and Adolescent Psychiatry*, 38, 1214–1222.
- KAZDIN, A., BASS, D., AYERS, W. A., & ROGERS, A. (1990). Empirical and clinical focus of child and adolescent psychotherapy research. *Journal of Consulting and Clinical Psychology*, 58, 729–740.
- KENDALL, P. C. (2000). Guiding theory for therapy with children and adolescents. In P. C. Kendall (Ed.), *Child and adolescent therapy: Cognitive-behavioral procedures* (2nd ed., pp. 3–27). New York: Guilford Press.
- KENDALL, P. C., & MARUYAMA, G. (1985). Meta-analysis: On the road to synthesis of knowledge? *Clinical Psychology Review*, 5, 79–89.
- KENDALL-TACKETT, K. A., WILLIAMS, L. M., & FINKELHOR, D. (1993). Impact of sexual abuse on children: A review and synthesis of recent empirical studies. *Psychological Bulletin*, 113, 164–180.
- KOLKO, D. J. (1992). Characteristics of child victims of physical violence: Research findings and clinical implications. *Journal of Interpersonal Violence*, 7, 244–276.
- \*KOLKO, D. J. (1996a). Clinical monitoring of treatment course in child physical abuse: Psychometric characteristics and treatment comparisons. *Child Abuse & Neglect*, 20, 23–43.
- \*KOLKO, D. J. (1996b). Individual cognitive behavioral treatment and family therapy for physically abused children and their offending parents: A comparison of clinical outcomes. *Child Maltreatment*, 1, 322–342.
- KOLKO, D. J. (1998). Integration of research and treatment. In J. R. Lutzker (Ed.), *Handbook of child abuse research and treatment* (pp. 159–181). New York: Plenum Press.
- KOLKO, D. J. (2002). Child physical abuse. In J. E. B. Myers, L. Berliner, J. Briere, C. T. Hendrix, C. Jenny, & T. A. Reid (Eds.), *The APSAC handbook on child maltreatment* (2nd ed., pp. 21–54). Thousand Oaks, CA: Sage.
- KRUCZEK, T., & VITANZA, S. (1999). Treatment effects with an adolescent abuse survivor's group. *Child Abuse & Neglect*, 23, 477–485.
- LAMBERT, M. J., & BERGIN, A. E. (1994). The effectiveness of psychotherapy. In A. E. Bergin & S. L. Garfield (Eds.), *Handbook of psychotherapy and behavior change* (4th ed., pp. 143–189). New York: Wiley.
- LAMBERT, M. J., WEBER, F. D., & SYKES, J. D. (1993, April). *Psychotherapy versus placebo*. Poster presented at the annual meetings of the Western Psychological Association, Phoenix, AZ.
- LANDIS, J., & KOCH, G. G. (1977). The measurement of observer agreement for categorical data. *Biometrics*, 33, 159–174.
- LANKTREE, C. B., & BRIERE, J. (1995). Outcome of therapy for sexually abused children: A repeated measures study. *Child Abuse & Neglect*, 19, 1145–1155.
- LIPSEY, M. W., & WILSON, D. B. (1993). The efficacy of psychological, educational, and behavioral treatment: Confirmation from meta-analysis. *American Psychologist*, 48, 1181–1209.
- LUTZKER, J. R., & RICE, J. M. (1987). Project 12-Ways: Measuring outcome of a large in-home service for the treatment and prevention of child abuse and neglect. *Child Abuse & Neglect*, 8, 519–524.
- MACLEOD, J., & NELSON, G. (2000). Programs for the promotion of family wellness and the prevention of child maltreatment: A meta-analytic review. *Child Abuse & Neglect*, 24, 1127–1149.
- MELTON, G. B., & FLOOD, M. F. (1994). Research policy and child maltreatment: Developing the scientific foundation for effective protection of children. *Child Abuse & Neglect*, 18, 1–28.
- \*MOORE, E., ARMSDEN, G., & GOGERTY, P. L. (1998). A twelve-year follow-up study of maltreated and at-risk children who received early therapeutic child care. *Child Maltreatment*, 3, 3–17.
- National Clearinghouse on Child Abuse and Neglect.

- (2000). *Child maltreatment 2000: Reports from the states to the National Child Abuse and Neglect Data System (NCANDS)*. Washington, DC: U.S. Government Printing Office.
- National Institute of Mental Health. (1999). *Bridging science and service: A report by the National Advisory Mental Health Council's Clinical Treatment and Services Research Workgroup*. Rockville, MD: Author.
- National Institute of Mental Health. (2000). *Translating behavioral science into action: Report of the National Advisory Mental Health Council Behavioral Science Workgroup*. Rockville, MD: Author.
- National Research Council. (1993). *Understanding child abuse and neglect*. Washington, DC: National Academy Press.
- OATES, R. K., & BROSS, D. C. (1995). What have we learned about treating child physical abuse? A literature review of the last decade. *Child Abuse & Neglect, 19*, 463–473.
- O'DONOHUE, W., T., & ELLIOTT, A. N. (1992). Treatment of the sexually abused child: A review. *Journal of Clinical Child Psychology, 21*, 218–228.
- ORWIN, R. G. (1983). A fail-safe *N* for effect size in meta-analysis. *Journal of Educational Statistics, 8*, 157–159.
- PROCHASKA, J. O., & NORCROSS, J. C. (2001). Stages of change. *Psychotherapy: Theory, Research, Practice, Training, 38*, 443–448.
- REEKER, J., ENSING, D., & ELLIOT, R. (1997). A meta-analytic investigation of group treatment outcomes for sexually abused children. *Child Abuse & Neglect, 21*, 669–680.
- REID, J. B., KAVANAGH, K., & BALDWIN, D. V. (1987). Abusive parent's perceptions of child problem behaviors: An example of parental bias. *Journal of Abnormal Child Psychology, 15*, 457–466.
- ROESLER, T. A., SAVIN, D., & GROSZ, C. (1993). Family therapy of extrafamilial sexual abuse. *Journal of the American Academy of Child and Adolescent Psychiatry, 32*, 967–970.
- ROSENTHAL, R. (1995). Writing meta-analytic reviews. *Psychological Bulletin, 118*, 183–192.
- ROSENTHAL, R., & RUBIN, D. B. (1982). A simple, general purpose display of magnitude of experimental effect. *Journal of Educational Psychology, 74*, 166–169.
- ROSENTHAL, R. & RUBIN, D. B. (1986). Meta-analytic procedures for combining studies with multiple effect sizes. *Psychological Bulletin, 99*, 400–406.
- \*SANKEY, C. G., ELMER, E., HALECHKO, A. D., & SCHULBERG, P. (1985). The development of abused and high-risk infants in different treatment modalities: Residential versus in-home care. *Child Abuse & Neglect, 9*, 237–243.
- SAYWITZ, K. J., MANNARINO, A. P., BERLINER, L., & COHEN, J. A. (2000). Treatment for sexually abused children and adolescents. *American Psychologist, 55*, 1040–1049.
- SHADISH, W. R. (1992). Do family and marital psychotherapies change what people do? A meta-analysis of behavioral outcomes. In T. D. Cook, H. M. Harper, D. S. Cordray, H. Hartmann, L. V. Hedges, R. J. Light, T. A. Louis, & F. Mosteller (Eds.), *Meta-analysis for explanation: A casebook* (pp. 129–208). New York: Russell Sage Foundation.
- SHADISH, W. R., MONTGOMERY, L. M., WILSON, P., WILSON, M. R., BRIGHT, I., & OKWUMABUA, T. (1993). Effects of family and marital psychotherapies: A meta-analysis. *Journal of Consulting and Clinical Psychology, 61*, 992–1002.
- SHADISH, W. R., ROBINSON, L., & LU, C. (1999). *ES: A computer program for effect size calculation*. St Paul, MN: Assessment Systems Corporation.
- SHIRK, S. R., & ELTZ, M. (1998). Multiple victimization and the process and outcome of child psychotherapy. *Journal of Aggression, Maltreatment and Trauma, 2*, 233–251.
- SHOHAM-SALOMON, V., & HANNAH, M. T. (1991). Client-treatment interactions in the study of differential change processes. *Journal of Consulting and Clinical Psychology, 59*, 217–225.
- SNOW, R. E. (1991). Aptitude-treatment interaction as a framework for research on individual differences in psychotherapy. *Journal of Consulting and Clinical Psychology, 59*, 205–216.
- \*SULLIVAN, P. M., SCANLAN, J. M., BROOKHOUSER, P. E., SCHULTE, L. E., & KNUTSON, J. F. (1992). The effects of psychotherapy on behavior problems of sexually abused deaf children. *Child Abuse & Neglect, 16*, 297–307.
- \*SZYKULA, S. A., & FLEISCHMAN, M. J. (1985). Reducing out-of-home placements of abused children: Two controlled field studies. *Child Abuse & Neglect, 9*, 277–283.
- TEICHER, M. H. (2002, March). Scars that won't heal: The neurobiology of child abuse. *Scientific American, 286*(3), 68–75.
- TINSLEY, H. E. A., & WEISS, D. J. (2000). Interrater reliability and agreement. In H. E. A. Tinsley and S. D. Brown (Eds.), *Handbook of applied multivariate statistics and mathematical modeling* (pp. 96–124). New York: Academic Press.
- U.S. Advisory Board on Child Abuse & Neglect. (1995). *A nation's shame: Fatal child abuse and neglect in the United States*. Washington, DC: U.S. Department of Health & Human Services.
- U.S. Department of Health & Human Services, Administration on Children, Youth, & Families. (2001). *Child maltreatment 1999: Reports from the states to the National Center on Child Abuse and Neglect*. Washington, DC: U.S. Government Printing Office. Retrieved June 5, 2004 from <http://www.axf.dhhs.gov/programs/cb/publications/cm99/high.htm>
- \*VERLEUR, D., HUGHES, R. E., & DE RIOS, M. D. (1986). Enhancement of self-esteem among female adolescent incest victims: A controlled comparison. *Adolescence, 21*, 843–854.
- WEISZ, J. R., WEISS, B., ALICKE, M. D., & KLOTZ, M. L. (1987). Effectiveness of psychotherapy with children and adolescents: A meta-analysis for clinicians. *Journal of Consulting and Clinical Psychology, 55*, 542–549.
- WEISZ, J. R., WEISS, B., HAN, S. S., GRANGER, D. A., & MORTON, T. (1995). Effects of psychotherapy with children and adolescents revisited: A meta-analysis of treatment outcome studies. *Psychological Bulletin, 117*, 450–468.
- \*WHITEMAN, M., FANSHIEL, D., & GRUNDY, J. F. (1987, November–December). Cognitive-behavioral interventions aimed at anger of parents at risk of child abuse. *Social Work, 32*, 469–474.

- WILSON, D. B., & LIPSEY, M. W. (2001). The role of method in treatment effectiveness research: Evidence from meta-analysis. *Psychological Methods, 6*, 413–429.
- \*WOLFE, D. A., EDWARDS, B., MANION, I., & KOVEROLA, C. (1988). Early intervention for parents at risk for child abuse and neglect: A preliminary report. *Journal of Consulting and Clinical Psychology, 56*, 40–47.
- \*WOLFE, D. A., SANDLER, J., & KAUFMAN, K. (1981). A competency-based parent training program for child abusers. *Journal of Consulting and Clinical Psychology, 49*, 633–640.
- WOLFE, D. A., ST. LAWRENCE, J., GRAVES, K., BREHONY, K., BRADLYN, D., & KELLY, J. A. (1982). Intensive behavioral parent training for a child abusive mother. *Behavior Therapy, 13*, 438–451.
- WOLFE, D. A., & WEKERLE, C. (1993). Treatment strategies for child physical abuse and neglect: A critical progress report. *Clinical Psychology Review, 13*, 473–500.
- WOLFE, V. V. (1989). Child sexual abuse. In E. J. Mash & R. A. Barkley (Eds.), *Treatment of childhood disorders* (pp. 545–597). New York: Guilford Press.
- World Health Organization. (1999, March). *Report of the consultation on child abuse prevention*. Geneva, Switzerland: Author.