

Research on the Treatment of Sexually Abused Children: A Review and Recommendations

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ABSTRACT

Objective: To review findings and conclusions from 29 studies that evaluated with quantitative outcome measures the effectiveness of treatments for sexually abused children. **Results:** The studies overall document improvements in sexually abused children consistent with the belief that therapy facilitates recovery, but only five of them marshal evidence that the recovery is not simply due to the passage of time or some factor outside therapy. There has yet to be a true large-scale, randomized trial of treatment versus control. The studies suggest that certain problems, such as aggressiveness and sexualized behavior, are particularly resistant to change and that some children do not improve. A number of considerations that merit special attention in future sexual abuse therapy outcome research are identified, including (1) the diversity of sexually abused children, (2) the problem of children with no symptoms, (3) the possible existence of serious "sleeper" effects, (4) the importance of family context on recovery, (5) the utility of abuse-focused therapy and targeted interventions, (6) the optimal length of treatment, (7) the problem of treatment dropouts, and (8) the development and use of abuse-specific outcome measures. **Conclusions:** The need for more treatment outcome research is highlighted by the rising demand for accountability in the health care system that will increasingly require professionals in the field of sexual abuse treatment to justify their efforts and their methods. *J. Am. Acad. Child Adolesc. Psychiatry*, 1995, 34, 11:1408-1423. **Key Words:** sexual abuse, treatment outcome, therapy.

An estimated 114,000 cases of sexual abuse were substantiated by child welfare authorities in 1994 (Wiese and Daro, 1995), and if past studies are any indicator, 44% to 73% of these children may receive some form of counseling or psychotherapy in the aftermath (Chapman and Smith, 1987; Finkelhor, 1983). Such services are being provided by many thousands of generalist clinicians as well as hundreds of specialized sexual abuse treatment programs that have developed over the last decade (Cicchinelli, 1986; Keller et al.,

1989). A large body of clinical theory and expertise on this subject now exists.

Until recently, relatively little of this knowledge has been developed and refined using the tools of treatment evaluation research. But as has been well recognized in related fields, treatment evaluation methods offer a very important systematic way for a community of clinicians to ask basic questions about their practice and engage in a dialogue about their theory.

The field of sexual abuse treatment has many important issues that need to be explored through systematic evaluation (Williams and Hudson, 1991). In an effort to encourage such research, we will use this article to review the studies that have been done on the treatment of sexually abused children. Already these studies suggest some conclusions of importance to the field. In the first half of this article, we will review this body of research and its implications. In the second half, we will go beyond this literature to discuss some of the particular challenges confronting those who wish to conduct treatment outcome research on the subject of child sexual abuse.

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AVAILABLE STUDIES

To locate studies for this review, we consulted previous review articles (Beutler et al., 1994; O'Donohue and Elliott, 1992), did a literature search of journals in related fields, and contacted researchers who were known to be engaged in treatment research. Given the paucity of studies, we cast a broad net, deciding to include studies in progress and reports that have not yet appeared in peer-reviewed journals as well as studies that might only loosely be considered treatment outcome studies. The studies we retained had to meet two criteria. They needed to have used some quantitative measure to evaluate sexually abused children (under age 18) at two points in time subsequent to the disclosure of the abuse, and at least five or more of the evaluated children needed to have received treatment during that interval. Thus we excluded a number of single case reports (e.g., Becker et al., 1982; Kolko, 1986) and a large number of studies that did not report findings using quantitative measures (e.g., Furniss et al., 1988; Sturkie, 1983). But we included a number of follow-up studies of abused children that did not have as their primary objective the evaluation of treatment effects but may have been looking at other issues such as the effect of court testimony (e.g., Goodman et al., 1992), as long as they reported differences between treated and nontreated children.

In the end, we identified 29 studies on which to base our review. The studies covered a wide variety of populations, including very young children (Stauffer and Deblinger, 1993) and adolescents (Larzelere et al., 1993), children in special circumstances, such as those living in institutions and deaf children (Sullivan et al., 1992), and a wide variety of treatment modalities, including sex education, music therapy, family therapy, group therapy, and cognitive-behavioral treatment.

FINDINGS FROM THE STUDIES

The studies could be categorized into three groups based on their designs (Tables 1 and 2): (1) pre- and posttest designs with no comparison groups, (2) quasi-experimental designs with group comparisons but no assignment of treatments, and (3) experimental designs with group comparisons and treatment assignment. We will review each of these groups separately.

Pre-Post Designs

The largest number of studies (17) were pre-post designs (Table 1), in which researchers evaluated children at two points in time (and sometimes more often) during which they received some kind of professional intervention. More than half had fewer than 20 subjects, but at least two included nearly 100 or more children (Lanktree and Briere, 1995; Larzelere et al., 1993). They used a variety of measures including parent observation measures such as the Child Behavior Checklist (CBCL), child self-report measures such as the Piers-Harris, some teacher and some therapist ratings.

All but one of the pre-post studies found significant improvement for the treated children as a group on at least one of the outcome measures over time periods that ranged from 9 weeks to 12 months. The main caveat to this conclusion, however, and the inherent limitation to pre-post designs, is that we do not know whether the improvement was due to the treatment the children received or simply the passage of time. A number of longitudinal studies have shown that sexually abused children as a group improve over time whether they receive treatment or not (Gomes-Schwartz et al., 1990; Runyan et al., 1988).

At least three of the pre-post studies used somewhat enhanced, self-comparison designs to try to deal with this problem (Deblinger et al., 1990; Larzelere et al., 1993; Stauffer and Deblinger, 1993). This involved evaluating children on multiple occasions before treatment and also in two studies, after treatment as well, to determine whether improvement was associated with the treatment interval or was occurring spontaneously during other periods when treatment was not being provided. For example, Deblinger et al. (1990) found no improvement between two pretreatment baseline evaluations 2 to 3 weeks apart, whereas significant improvement occurred between the second baseline and the end of a 12-session treatment program. Similarly, two pretreatment assessments during a baseline period that averaged the length of treatment produced no significant improvement, whereas reductions in symptoms occurred over the course of the treatment period (Stauffer and Deblinger, 1993). Larzelere et al. (1993), in one of this group's two studies, used a distinctive extended time series that plotted the frequency of behaviors like suicide gestures and sexually

TABLE 1
Treatment Outcome Studies: Pre-Post Designs

Study	Groups	Sex/Age	Treatment	Measure	Findings
Clendenon-Wallen, 1991	<i>N</i> = 11	M/F 14-19	Music therapy, 12 wk	Adjective Checklist (self-confidence)	"Significant increase . . . in indicators of self-confidence" but no decrease in "contraindicators" of self-confidence
Cohen and Mannarino, 1992	<i>N</i> = 20	F 11-15	Group, structured, 12 sessions	CDI, TSCC	Significant improvement in Anxiety, PTS, Sexual Concerns, Dissociation and Anger subscales of TSCC; no improvement on Depression subscale of CDI
De Luca et al., 1993	<i>N</i> = 7	F 10-11	Group, structured activities, 10 sessions	CSI, RCMAS, CLQ, CBCL	Significant improvement in self-esteem, anxiety, internalizing and externalizing, at both posttest and 9-mo follow-up; no change in loneliness
Deblinger et al., 1990	<i>N</i> = 19	F 3-16	Child and nonoffending parent each received individual cognitive-behavioral therapy	DSM-III-R-based PTSD questions; CDI, STAIC, CBCL	Significant improvements at post-Tx on all measures
Friedrich et al., 1992b	<i>N</i> = 33 (completed Tx)	M 4-16	Group, family, parent training, individual	Parent: BDI, Sibling Beh. Prob. Checklist, SSQ, CBCL, CSBI; Child: CDI, M-Z, RATC; Teacher: Achenbach Teacher Scale; Therapist: TRF	"At follow-up significant improvements were noted in a number of areas, including overall behavior problems and sexual behavior. Therapy outcome was related to a number of family and abuse factors, including maternal depression and social support, family conflict, and severity of abuse."
Hack et al., 1994	<i>N</i> = 6	M 8-11	Group, structured activities, 12 sessions	CBCL, CDI, CSI, RCMAS	Improvement, but not statistically significant
Hiebert-Murphy et al., 1992	<i>N</i> = 5	F 7-9	Group, structured activities, 9 wk/9 sessions/90 min	CBCL, CSI, RCMAS, CLQ	Tx appeared to positively affect behavior (CBCL); however, subject self-report measures evidenced increased disturbance
Hoier and Inderbitzenn-Pisaruk, 1987	<i>N</i> = 18	M/F 5-15	Two mixed-sex, age-based behavioral-cognitive groups	CDI, FSSC-R, CBCL, CBCL-Sex Prob.	Total sample evidenced significant reduction in depressive cognitions (CDI), fearfulness (FSSC-R), and internalizing problems (CBCL); no reductions in CBCL
Kitchur and Bell, 1989	<i>N</i> = 7	F 11-12	Group dynamic approach, 16 wks, 16 sessions	Piers-Harris, CBCL	externalizing and sexual acting-out Subjects evidenced increased self-esteem, but baseline scores were within the norm; behavior problems did not appear to change
Lanktree and Briere, 1995	<i>N</i> = 105	M/F 8-15	Outpatient sexual abuse treatment facility; Tx duration range: <3 mo-→12 mo	TSCC (administered every 3 mo, for 12 mo)	Symptom reduction as a function of time in Tx was most strongly evidenced by the reduction of anxiety, depression, PTS; sexual concerns tended to persist, and no significant change in dissociation was evidenced beyond the first 3 mo of Tx or for anger beyond 6 mo
Larzelere et al., 1993	<i>N</i> = 141	M/F 11-19	Group cognitive, 20 wk	Suicide gestures, aggressive acts, avoidance behavior, sexual acting-out, assessed 20 wk pre-Tx, 20 wk during Tx, and 20 wk post-Tx	Pre-post changes noted in suicide gestures and avoidance behaviors, but not for aggressive behaviors; sexual acting-out by males appeared to increase during but decrease after Tx

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TABLE 1
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Study	Groups	Sex/Age	Treatment	Measure	Findings
Mackey et al., 1987	N = 5	F 12-18	Drama therapy, 10 wk/10 sessions/4-5 hr	BDI, SCL-90, TSBI, ASQ, SSQ, MCSD	Overall Tx did not evidence significant changes; SCL-90 subscale Hostility, however, did show positive significant change
Neilki and Waters, 1988	N = 6	F 4-8	Group educational/dynamic approach	Caregiver report questionnaire of 33 "problems drawn from recognized symptoms following sexual abuse"	Sample evidenced significant reduction on the total scores derived from reported problems, but increase noted in sex symptoms and "clinginess"
Richardson, 1994	N = 69	F 6-16	Group, 6-12 wk	CBCL	Improvement in self-reported depression and delinquency, and teacher-reported anxiety, depression, and withdrawal; no change on parent report
Rust and Troupe, 1991	N = 25	F 9-18	Multifaceted: individual, group, art, recreation; Tx duration: 6 mo	Piers-Harris	Subjects evidenced a significant increase in self-esteem after 6 mo of Tx; post scores also dispersed, suggesting that Tx had a "large" effect on some and only a minor effect on others
Stauffer and Deblinger, 1993	N = 17	M/F 2-6	Cognitive-behavioral group	SCL-90-R, IES, CBCL, CSBI, Parent Practices Questionnaire	There were significant improvements from pretreatment to posttreatment on all measures
Sinclair et al., in press	N = 34	F 13-18	Group cognitive- behavioral PTSD Tx for females, 20 sessions/20 wk	CBCL, YSR; PTSD: YSR, CBCL, RADS	Improvements on YSR: Total/ Internal/External; CBCL Internal; PTSD: YSR/CBCL

Note: Abbreviations used in Tables 1 and 2: APSWS = Anatomy Physiology Sexual Awareness Scale; ASQ = Attributional Style Questionnaire; BDI = Beck Depression Inventory; CBCL = Child Behavior Checklist; CDI = Children's Depression Inventory; CITES = Children's Inventory of Traumatic Events Scale; CLQ = Children's Loneliness Questionnaire; CSBI = Child Sexual Behavior Inventory; CSI = Coopersmith Self-Esteem Inventory; FSSC-R = Fear Survey Schedule for Children-Revised; IES = Impact of Events Scale; IPAT = Institute for Personality & Ability Testing Anxiety Scale; IPAT = Institute for Personality & Ability Testing Depression Scale; LBC = Louisville Behavior Checklist; MCSD = Marlowe-Crowne Social Desirability Scale; MSSJ = Maternal Social Support Inventory; M-Z = Martinek-Zaichkowsky Self-Concept Scale; NHIS = National Health Interview Survey; PPVT-R = Peabody Picture Vocabulary Test-Revised; PRESS = Preschool Symptom Self-Report; PSCI = Primary Self Control Scale; PSQ = Parental Support Questionnaire; PTSD = Posttraumatic Stress Disorder; RADS = Reynolds Adolescent Depression Scale; RATC = Roberts Apperception Test for Children; RCMAS = Revised Children's Manifest Anxiety Scale; SCL-90-R = Symptom Checklist 90-Revised; SPC-CP = Structured Parent Counseling-Child Psychotherapy; SSQ = Social Support Questionnaire; STAIC = State-Trait Anxiety Inventory for Children; TRF = Teacher (Therapist) Rating Form of CBCL; TSBI = Texas Social Behavior Inventory; TSSC = Trauma Symptom Checklist for Children; Tx = Treatment; YSR = Youth Self-Report form of CBCL; WBR = Weekly Behavior Record.

acting-out behaviors in a residential group over a period that included 20 weeks before, 20 weeks during, and 20 weeks after the children entered therapy. Several behaviors showed pre-post therapy differences that, upon examination of the plots of behavioral frequencies over time, appeared to be more a function of treatment than of the passage of time.

In a somewhat different approach to the issue of spontaneous remission, Lanktree and Briere (1995) compared statistically the effect on symptoms of the amount of time in therapy versus the amount of time between the end of abuse and the start of treatment. They found that time in therapy was considerably

more predictive of symptom status, suggesting that spontaneous remission was not the primary explanation of treatment improvement. However, this approach only controls for spontaneous remission if remission is considered something that occurs in a linear fashion from time of last abuse. If symptoms tend to worsen for many children at disclosure or if symptom worsening is what brings many children into therapy at a point many months after the last abuse, then using time since last abuse is not the best way to assess for the effects of spontaneous remission.

Despite such problems, the pre-post designs are important contributions, especially for their negative

TABLE 2
Treatment Outcome Studies: Experimental and Quasi-Experimental Designs

Study	Groups	Sex/Age	Treatment	Measure	Findings
Quasi-experimental designs					
Downing et al., 1988	Tx 1 N = 12	M/F 6-12	Psychodynamic oriented	Parent report, school counselor report	Reinforcement group improved more on sleep disturbances, sex play with others, enuresis, and general misbehavior; neither group improved on sexual self-stimulation
Gomes-Schwartz et al., 1990	Tx 2 N = 10 Tx 1 N = ?	M/F 0-18	Reinforcement theory oriented Crisis intervention + in-house Tx	LBC (E-1, E-2, E-3), Piers-Harris or Purdue Self-Concept, CBCL	Greater improvements were associated with in-house Tx
	Tx 2 N = ? Tx 3 N = ? Total N = 156		Crisis intervention + Tx in community Crisis intervention only		
Goodman et al., 1992	Tx N = 22 Control N = 5	M/F 3-17	Mixed, unspecified No Tx	CBCL pre-post	"Psychological counseling was unrelated to improvement"
Oates et al., 1994	Exp N = 64 Control N = 76	M/F 5-15	Mixed, unspecified No Tx	Pictorial Scale of Perceived Competence and Social Acceptance, or McDaniel-Piers Young Children's Self Concept Scale, or Piers-Harris Self Concept Scale, CDI, CBCL	"No relationship was found between Tx and outcome"
Sullivan et al., 1992	Tx N = 36 Control N = 37	M/F 12-16	Individual psychotherapy No Tx	CBCL pre-post	Tx group had fewer problems after 1 yr
Experimental designs					
Baker, 1987	Tx 1 N = 15	F 13-17	Individual, 10 wk	Piers-Harris, IPAT Anxiety, IPAT Depression	Group Tx > individual on self-esteem, equal in other outcomes
	Tx 2 N = 24		Group, 6 wk		
Berliner and Saunders, 1993	T ₁ = 48 T ₂ = 55	M/F 4-13	Structured, supportive educational group, 10 sessions Above plus stress inoculation and gradual exposure	RCMAS, FSSC, CDI, CITES, CBCL, CSBI	No difference group × time
Burke, 1988	Tx N = 12	F 8-13	Structured group, 6 sessions/6 wk	CDI, RCMAS, distress at follow-up, FSSC-R, CBCL	Tx group had less negative affect at post-Tx and at follow-up
	Control N = 13		No Tx (waitlisted)		
Cohen and Mannarino, in press	Tx 1 N = 39	M/F 3-6	Abuse-specific cognitive/behavioral, structured therapy	PSQ, CSBI, CBCL, NHIS, WBR, MSSI, BDI, PPVT-R, PRESS	Abuse-specific group had more improvement on CBCL total, internalizing and WBR
	Tx 2 N = 28		Nondirective supportive psychotherapy		
Monck et al., 1994	Tx 1 N = 25 Tx 2 N = 22	M/F 4-16	Family Tx Family Tx plus group	CDI, Harter, school behavior (teacher-rated)	No difference between groups on standardized measures

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TABLE 2
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Study	Groups	Sex/Age	Treatment	Measure	Findings
Perez, 1988	Tx 1 N = 18	M/F 4-9	Individual play	PSCI (Self-Concept), Locus of Control Scale	Both play Tx groups had higher self-concept and self mastery, but no difference between individual vs. group
	Tx 2 N = 21		Group play		
	Tx 3 N = 16		No Tx		
Verleur et al., 1986	Tx N = 16	F 13-17	Group (sex education)	CSI APSWs	Tx group had more self-esteem and were more sexually aware than the control group
	Control N = 14		No Tx		

Note: Abbreviations are explained in the footnote to Table 1.

results. Although children's improvement cannot be conclusively linked to treatment in these studies, failures to improve definitely suggest places where therapy is missing the mark. And the studies do show that not all problems and not all children respond to treatment.

Several of the studies show improvement on some indicators and failure to improve on others. In particular, sexual problems and "externalizing" problems (the term used for the CBCL items that indicate aggressiveness, acting out, etc.) seem to be more resistant to change. For example, Hoier and Inderbitzenn-Pisaruk (1987) found no improvement in CBCL sex problems or externalizing behaviors, while change did occur on CBCL internalizing items, as well as on a measure of children's depression and fearfulness. Sinclair et al. (in press) found this same contrast in the externalizing versus internalizing items on the CBCL. Nelki and Waters (1988) found no abatement in sexual symptoms. Lanktree and Briere (1995) also found that sexual concerns were among the most persistent symptoms, and anger (an externalizing symptom) only improved in the first 6 months and then plateaued without further improvement. On the other hand, other studies have noted improvement in sexual behaviors and externalizing symptoms under some particular conditions. Friedrich et al. (1992b) found improvement in sexual acting-out in a study of boys, a finding supported by another study that was able to break out effects by gender (Larzelere et al., 1993). This suggests that sexual concerns could be more difficult to treat in girls or that therapists make more of an effort to target them in boys, because they are seen as more dangerous. Stauffer and Deblinger (1993) also had success treating sexual behaviors and externalizing symptoms, but this study, exclusively of preschoolers, contained a specific

treatment module aimed at helping parents to manage such acting-out behavior.

The pre-post studies also show that some children improve quite a bit more than others. Reflecting this, Rust and Troupe (1991) noted that posttest scores tended to be much more dispersed than pretest scores, suggesting that some children had made dramatic improvements and others not at all. It is unfortunate, however, that most of the pre-post studies reported only group scores and thus did not comment on the percentage of children who failed to improve, and who these children were.

Quasi-Experimental Designs

In the quasi-experimental design studies, two groups are compared, usually some children who received treatment and some who did not, but these groups may not necessarily be equivalent. Sullivan et al. (1992) is a good example of such a design: the children in the therapy group had significantly fewer behavior problems 1 year later than the no-therapy children, but the groups were not equivalent. The no-therapy comparison group consisted of children whose parents had specifically refused therapy for their children, when it had been offered. Even if such children had been equivalently symptomatic to start with, they might be systematically different in other ways that made it more difficult for them to recover (for example, having less psychologically minded parents). Thus the difference between the groups might not be due to the treatment at all.

The quasi-experimental design category also includes several studies that simply followed up groups of sexually abused children and compared those who happened to get treatment, of whatever kind, for whatever reason,

and those who did not. Such designs are useful insofar as they evaluate the actual, heterogeneous state-of-affairs that exists in regard to treatment in most communities. The weakness is, again, that the children with treatment may be very different from those without, and because the treatments may be very diverse, not much can be inferred about what works.

It is interesting that three of these quasi-experimental designs failed to find any advantage for children receiving therapy *per se* compared to children with no therapy at all (Gomes-Schwartz et al., 1990; Goodman et al., 1992; Oates et al., 1994). This could mean that children without therapy in fact were getting something else of equal value, such as good parental support, or it could mean that they were not in therapy because parents noted that they were already recovering well without professional help. But it could also mean that non-specific therapies were rather ineffective. This is the implication from the Gomes-Schwartz et al. (1990) study, which found greater improvements among those children who received the abuse-focused therapy provided by the researchers' own in-house program than among those treated in the community. However, Gomes-Schwartz et al. (1990) provide few details to help evaluate their findings and conclusion.

Experimental Designs

There have been a few studies that randomly assigned sexually abused children to treatments or systematically matched them to try to make groups as equivalent as possible. These studies have produced mixed results, depending in part on who was being compared. The three experiments comparing treatment and no-treatment groups (Burke, 1988; Perez, 1988; Verleur et al., 1986) have all found significant effects of treatment. Burke (1988) for example, used random assignment to a treatment or a waiting list group and found group therapy was related to a decline in children's depression, anxiety, sexual abuse fears, and CBCL internalizing symptoms. It is unfortunate that two of these studies are unpublished dissertations (Burke, 1988; Perez, 1988), and the relatively small-scale designs of all three, and in one case (Verleur et al., 1986) the unconventional approach, detract from their scientific weight.

By contrast, the five experiments comparing alternative treatments (Baker, 1987; Berliner and Saunders, 1993; Cohen and Mannarino, 1992; Monck et al.,

1994; Perez, 1988) have not shown such consistently favorable results. Berliner and Saunders (1993) did not find significant advantage for a program that used a module specifically designed to reduce anxiety over a program lacking such a module. Monck et al. (1994) did not find that children assigned to age-specific group treatment in addition to family treatment did better than those receiving only family treatment. Perez (1988), despite finding differences between play therapy and controls, did not find differences between group or individual play therapy modes. Baker (1987) did find group treatment more effective than individual therapy in terms of self-esteem, but not on two other important outcomes, anxiety and depression. In the only comparison of treatment alternatives to show strong differences, Cohen and Mannarino (in press) demonstrated advantages to a structured, abuse-specific group as opposed to simply supportive psychotherapy.

It is unfortunate that treatment comparison designs such as these have some serious inherent challenges to overcome. One problem is ensuring that the treatments really are different from one another in ways that matter, since theory and practice sometimes differ greatly. Another problem is making sure the design is powerful enough (particularly that there are enough subjects) to detect a difference. Detecting a difference between two treatment groups is inherently more difficult than detecting a difference between a treatment and no-treatment group, because of the placebo effect, which ensures that almost any intervention will produce some improvement and thus the difference being measured between treatments is much smaller (Kazdin, 1991). Samples of approximately 70 subjects per group are considered necessary in treatment comparison studies to detect the differences in average effect sizes reported in the literature (Kazdin, 1991). All the extant experimental studies in the sexual abuse treatment literature fall far short of that standard.

SUMMARY OF FINDINGS

Taken as a whole, the studies of sexually abused children in treatment show improvements that are consistent with the belief that therapeutic intervention facilitates children's recovery. However, only five of the studies—two fully described, true experimental designs (Burke, 1988; Perez, 1988) and the pre-post tests with enhanced self-comparison—marshal adequate evidence that the widely observed recovery is

not simply due to the passage of time or some other factor outside of therapy, and unfortunately the two experimental studies are unpublished dissertations.

From the standpoint of scientific evaluation, which today primarily relies on randomized clinical trials, it would have to be said that, because enough appropriate studies have not been done, the effectiveness of sexual abuse treatment has not yet been proven. However, the existing research does count as something important, since even pre-post designs provide an opportunity to confirm the null hypothesis of no change, and almost all of them reject this hypothesis. By the strict scientific standard, the evidence does not say that sexual abuse treatment is ineffective or unnecessary. Rather it is promising, but the definitive studies have not yet been done.

Moreover, the evidence about the effectiveness of sexual abuse treatment has to be put into the context of the literature on treatment outcome in general, which is quite a bit more extensive and advanced. There is a substantial body of literature on the psychotherapy of children that confirms its utility with a wide variety of disorders in a wide variety of children (Casey and Berman, 1985; Kazdin, 1991, 1993; Weisz et al., 1995). There is much reason to expect that this will also be true with sexually abused children. While the research on sexually abused children, specifically, is still in its infancy, many and more sophisticated designs are sure to follow.

In the meantime, the studies on treatment of sexually abused children contain a number of other findings and suggestions that merit the attention of both clinicians and researchers.

First is the important suggestion that some symptoms are more resistant to treatment. Several studies have found that problems such as aggressiveness and sexualized behaviors do not change as much or as consistently in response to treatment. This could be because such problems, once they are triggered, are inherently more difficult to change. But it could also be that therapists are not addressing these problems effectively because these behaviors make them uncomfortable or because they lack the specific skills and do not apply treatment approaches that alter such behaviors. More studies should address what works in treating these particular problems.

A second important reminder from the research to date is the clear fact that in spite of overall treatment

effects, some children do not improve or improve as much. This is a crucial issue and one that is not sufficiently attended to by the research. Even the simplest pre-post designs could be adding to knowledge about so-called treatment failures by regularly reporting on the number of such failures and their characteristics, although this is rarely done. More analysis is needed of who the children are who do not improve and why. Larzelere et al. (1994a) suggest that children with denial and avoidance patterns have more difficulty recovering, whereas children who have symptoms that give them a strong motive to change (e.g., anxiety or fear) are more likely to do so.

Finally, there are suggestions in the current research that some therapies may not be particularly effective or their distinctive effectiveness has been oversold. Several of the quasi-experimental studies fail to show that children as a group do better when they take advantage of generally available community treatment options. A number of other studies have not been able to show advantages to one type of treatment over another. This does echo findings from the general literature on children's psychotherapy, which overall has not found reliable differences between modalities such as group or play or individual formats (Casey and Berman, 1985).

As can be seen from this review, the field of treatment evaluation research on sexual abuse is still in the process of development. A wide variety of issues need to be taken into account by clinicians and researchers in the elaboration of this field. In the next several sections, we will highlight some of these issues and signal some of the importance they hold for clinicians and researchers trying to make decisions about how to organize treatment and how to evaluate it.

SEXUAL ABUSE AS A SPECIAL TREATMENT AND EVALUATION CHALLENGE

One important consideration distinguishes sexual abuse treatment from much other child psychotherapy: sexual abuse is an experience, not a disorder or a syndrome. It can lead to disorders and syndromes, but it is not one in itself. In most other psychotherapy, children enter into treatment because of some disturbing way they are feeling or behaving, for example, depressed, hyperactive, acting out, or suicidal. In sexual abuse, they are often referred because of the discovery that they had a particular kind of experience. This

means that sexual abuse treatment confronts a very diverse set of children: of all ages, with a variety of histories and presentations, with many different kinds of symptoms, and some without symptoms at all. These realities have implications for the organization of treatment and its evaluation.

Diversity of Manifestations

Sexual abuse appears to be associated with an extremely wide range of symptom patterns (Kendall-Tackett et al., 1993). Although those patterns may vary by age (for example, with running away or substance abuse being more typical of older children and nightmares and anxiety more typical of younger children), there is no special sexually abused children's syndrome or even any symptom that is characteristic of a majority of such children.

This diversity means that it is unlikely that any one particular therapy is going to be suitable or effective for all children. Moreover, it poses some dilemmas for those who want to evaluate treatments. Studying children of diverse ages and problems can be a very complex task. There are big advantages to working with relatively homogeneous groups of children. A particular treatment can be more systematically administered with groups of children of the same age, same gender, and experiencing similar kinds of symptoms. Moreover, restricting the age range of subjects simplifies the choice and administration of outcome measures. Thus many of the studies we reviewed focused on girls alone, adolescents alone, preschool children alone, or residential subjects alone.

On the other hand, the more homogeneous the group studied, the less generalizable the results. A treatment demonstrated to be successful with one type of victim may not work with a different group. If homogeneous groups are regularly used, then in order to help ordinary practitioners with their diverse caseloads, evaluation researchers will need to replicate studies on a wide variety of different groups of children of different ages, genders, and symptom patterns. Only in this way will the literature arrive at the complex and nuanced conclusions about what works for which group.

Asymptomatic Children

Another aspect of the variety among sexually abused children is that some of them do not appear to have any clinically significant symptoms at all. Initial assessments

suggest that up to 40% have few symptoms on standard instruments used for clinical evaluation, and most do not exhibit as much emotional and behavioral disturbance as the typical clinical populations of non-sexually abused children (Kendall-Tackett et al., 1993). This is in part because they come into treatment not because of symptoms, but as a result of the discovery of their abuse. Nonsymptomatic sexually abused children are frequently sent to treatment, first, because adults fear unseen damage that they want therapy to detect and ameliorate, and second, because adults see treatment as prophylaxis against the onset of problems at some future time.

There is much debate about why some sexually abused children appear to be asymptomatic. Some such children may simply have had relatively minor kinds of abuse experiences. Others may be resilient children who are dealing successfully with the challenges posed by the abuse. Still another possibility is that clinicians and researchers do not yet know how to detect all of the disturbances that sexual abuse creates. One suggestion is that some children have an avoidant coping style that allows them to successfully suppress any conflicts or discomforts related to the abuse, even though at another level they are quite distressed (Elliott and Briere, 1994; Johnson and Kenkel, 1991; Leitenberg et al., 1992). These children may look asymptomatic, and this style may even be quite functional for them in the short run.

Asymptomatic children pose serious dilemmas for therapists. One concerns how to distinguish well-functioning, resilient children who do not need therapy from other nonsymptomatic children in whom the trauma is serious, subtle, or being denied. Another is how to decide how much treatment is sufficient; asymptomatic children do not have symptoms that will abate with treatment to mark their improvement and signal when therapy should terminate.

Asymptomatic children also pose dilemmas for researchers. For example, should they be included in outcome studies? Some would argue that because they are at risk to deteriorate, they need to be included in even short-term studies to see whether the treatment (or specific forms of treatment) is differentially effective in preventing any decline. But the inclusion of such asymptomatic or marginally symptomatic children can make it much more difficult to show positive results. Since asymptomatic children cannot improve on the

outcome measures, group changes are constrained by a form of "ceiling effect."

We would argue that for short-term outcome studies designed to test the effectiveness of specific interventions aimed at particular symptoms, abused children without such symptoms might well be excluded. At the same time, however, it would be a mistake to exclude asymptomatic children from outcome research in general. Many studies that include asymptomatic children need to be undertaken in order to understand the course of their development and whether early intervention does, in fact, have prophylactic benefits in preventing later deterioration. If such children are as a rule going to receive treatment under the current philosophy of sexual abuse intervention, then the efficacy of this treatment needs to be evaluated.

" Sleeper Effects "

Another intriguing issue that separates sexual abuse from some other children's mental health problems is the widespread belief that it frequently entails " sleeper effects," or serious symptoms that may not surface until many years later (Briere, 1992). For clinicians, a big unanswered question is not just how, but also whether, sleeper effects can be prevented by early intervention. It may be that treatment of early, subtle, or transitory effects can short-circuit later more serious problems. But in theory at least, it is also possible that some of the delayed effects of sexual abuse are triggered by later developmental challenges that may not be accessible to therapy at an earlier time. Thus some promiscuous behavior may arise from a sense of being sexually stigmatized that cannot be addressed until the sexual concerns of adolescence surface.

The problem of sleeper effects also poses dilemmas for outcome research. The theory, although widely subscribed to and supported by much clinical lore, actually has little in the way of research to confirm it. The available longitudinal studies do suggest that 10% to 20% of sexually abused children deteriorate over the first year to year-and-a-half (Kendall-Tackett et al., 1993; Mannarino et al., 1991). One study (Gomes-Schwartz et al., 1990) did find that such deterioration is more likely among those children with the fewest initial symptoms. But these are very short-term trends that do not represent the kind of severe, late-onset problems that most worry those concerned with possible sleeper effects.

Moreover, short-term deterioration may even be an appropriate development and indicative of a recovery process among some children who must first confront their trauma in order to overcome it. For example, Larzelere et al. (1993) found that among boys, sexual acting-out behavior actually increased at the start of treatment before declining significantly by the end. So in addition to sleeper effects, researchers need to be concerned about what might be called reverse-sleeper effects, deterioration that is a sign of later improvement. Both sleeper and reverse-sleeper effects are strong reasons for research designs that include both multiple assessments and extended follow-up periods.

Unfortunately, the widespread belief in the existence of sleeper effects works to make the field uneasy about short-term studies. Even if treatments are shown to work in the short term, there may always be those who wonder whether the abatement of early symptoms may in some paradoxical way be associated with greater long-term sleeper effects, for example, because treatment was only suppressing symptoms. However, if it could be established that only certain kinds of victims or certain early symptom patterns were vulnerable to sleeper effects, this might simplify evaluation researchers' plight and invest more confidence in short-term results. In addition to good long-term follow-up studies to thoroughly explore sleeper effects, it may also be possible to use adult retrospective studies with good sequential histories to focus more on the issue of sleeper effects, their timing, severity, and correlates.

THE CONTEXT OF ABUSE

Another problem for clinicians and treatment evaluators is that, much as it is studied and researched as a self-contained phenomenon, sexual abuse, like many other problems of childhood, does not occur in a vacuum. A variety of other individual, family, and community problems tend to accompany sexual abuse. Research has very clearly demonstrated that certain elements of this context, particularly parental support (Everson et al., 1991), maternal upset (Deblinger et al., 1990; Newberger et al., 1993), and help-seeking (Waterman, 1993) in response to the family crisis, but also general elements of family functioning such as cohesion and healthy conflict management (Conte and Schuerman, 1987), are consistent predictors of the level of distress in children and their speed of recovery. In addition, these other factors may actually be direct

causes rather than moderators of abuse effects. For example, aggression in some sexually abused children may be associated with witnessing the family violence that is present at high rates in intrafamilial sexual abuse situations (Deblinger, 1994). Obviously the effectiveness of treatment for children is likely to be strongly influenced by the family context, and addressing it should be a very important priority for intervention.

For people designing interventions, this poses important questions about where the focus of treatment should be. It has been widely debated whether it makes sense to treat the child alone, particularly because changes that are made in the family context may continue to provide therapy payoffs even after the child's therapy has terminated. Of course, there are instances in which parents are not amenable to involvement. Are there also instances in which parental intervention could be the sole intervention? This option may be especially useful when children are very young or are reluctant to participate in treatment.

Preliminary evidence from a study (Deblinger, 1994) provides suggestive support for the idea that the effect of therapy may differ depending on who is the recipient. Significantly greater reductions in parent-reported child behavior problems were found in a treatment versus a community comparison group when the parent alone or both the parent and the child received an abuse-specific cognitive-behavioral intervention, but not so if *only* the child was treated. But the results were somewhat reversed with regard to symptoms reported directly by the children. When the children received the treatment either alone or with the parent, there were significant reductions in these child-reported symptoms compared to community controls, but not when the parent alone received treatment. These results suggest that certain behaviors considered problematic by parents are not reported to improve unless the parent receives treatment directly.

But if other family members are to be targeted in treatment, there are a number of important, so-far unevaluated options about what the focus of this family intervention should be. A treatment for parents might, for example, consist of something parallel to what the child is receiving—an opportunity to express feeling about the abuse, the correcting of misinformation about the offender and the abuse, and the provision of information and support. On the other hand, the treatment for parents might be geared much more

specifically at parental symptomatology or at the parent's dysfunctional parenting. For parents with their own abuse history, there is the question of whether they need abuse specific treatment for their own experiences, as suggested by Stauffer and Deblinger (1993). Some or possibly all of these approaches may be beneficial, but at the present the proper mix or focus of parental or family treatment has not been the subject of much evaluation.

For researchers, the family context of sexual abuse also provides an important set of possibly complicating factors that have not been sufficiently conceptualized or studied. For one thing, if family support affects recovery so strongly, then it is crucial to make experimental groups of children as equivalent as possible on these dimensions. Ideally, this can be done through randomization, but even in this case it is important to test groups for possible differences. Differences in level of family supportiveness should always be measured and entertained as a possible alternative explanation to apparent treatment effects.

To control for the confounding effects of family support, more needs to be known about what families do and how family context influences recovery. For example, little is known about what parents do when their children are (or are not) in treatment. Do they seek out various informal supports for themselves? Do they talk with children about what has happened to them or what is going on? This can affect differences found between treatment and no-treatment controls.

Context can also be a very important conditional factor in therapy. It is very likely, for example, that certain kinds of therapies work for children from some kinds of families but not others. Thus consideration should be given to designs that look for treatment by family type interactions.

TREATMENT ISSUES

Abuse-Specific Therapy

As elsewhere in the mental health field, so for sexual abuse, too, there have been debates about which treatment approach works best. Some consensus, however, has been achieved that "abuse-specific" therapy is a preferred approach to at least an important portion of the intervention (Berliner and Wheeler, 1987; James, 1989). The common elements of such treatment usually include (1) encouraging expression of abuse-related

feelings (e.g., anger, ambivalence, fear), (2) clarifying erroneous beliefs that might lead to negative attributions about self or others (e.g., self-blame), (3) teaching abuse prevention skills, and (4) diminishing the sense of stigma and isolation through reassurance or exposure to other victims (e.g., group therapy).

There are also certain assumptions underlying the "abuse-specific" approach: that there are specific effects of the abuse experience; that therapy should be both ameliorative and preventive; and that it is helpful for children to see the connection between their abuse and current distress because it provides a framework for understanding that the distress is a legitimate reaction to an aversive experience. In this way, even those responses or symptoms that are painful or interfere with functioning can be conceptualized as positive coping responses that, while initially useful, may have become maladaptive.

Because this approach has been embraced by the field and may be applicable to other traumas, it deserves special attention in future treatment outcome studies. In particular, comparisons between this abuse-specific treatment and other more generic therapies are necessary. The type of design that sheds light on this hypothesis is a study by Cohen and Mannarino (in press) in which subjects were assigned to either a structured, abuse-focused treatment condition or a nondirective, supportive therapy condition. Results suggest that the children in the abuse-focused condition made significantly greater improvement.

The major difficulty in conducting research to evaluate abuse-specific treatment is ensuring that the more generic approach used as a comparison is really different. Since most families will be seeking treatment for the effects of sexual abuse, it is likely that the abuse and its potential ramifications would be a major topic even in the generic treatment.

Targeted Interventions

Even within abuse-specific treatment programs, it appears, as suggested earlier, that some symptoms are more resistant to change than others (Lanktree and Briere, 1995). It may be that the conventional approach to abuse-specific treatment, because it is primarily supportive and educational, is less effective with certain specific emotional and behavioral problems that require

more targeted and aggressive interventions. Sexual behavior problems and externalizing behaviors, for example, might be the sort of problems that respond better to more targeted treatments such as those used for behavior problems (Hoghughli et al., 1988). The usual way of treating a behavior problem is to describe the problem behavior, determine its frequency and severity, devise an intervention, carry it out, and then reassess its occurrences to determine whether it has responded to treatment. This kind of structured approach is not the standard practice in most abuse-specific treatment programs.

To deal with the problem of resistant symptoms, more studies are needed to test the use of specific interventions for specific symptoms. One approach would be to incorporate the specific intervention into a broader treatment as Berliner and Saunders (1993) did for fear and anxiety symptoms. Another approach would be to develop a whole treatment program for the specific symptom (e.g., sexualized behavior) and compare it with a less specific treatment (Bonner et al., 1993). Unfortunately, while this whole-treatment approach might be possible with a particularly complex problem behavior such as inappropriate sexual behavior, it would be very difficult to construct a treatment or enlist participants in a treatment program that exclusively addressed only one emotional or cognitive reaction such as anger or guilt.

One important task for those who would test targeted interventions is to specify clearly what the interventions are and how they are linked theoretically to the predicted outcomes. In past research this has often been lacking. For example, many studies have used general measures of self-esteem or depression as dependent variables, presumably because treatment is expected to have an impact on these outcomes. However, it has not always been clear what aspects of the intervention were expected to improve self-esteem or alleviate the depression. Was it the relationship with the therapist, the supportive environment, the content of the treatment, or some other factor? Another example of a conceptualization problem is making a priori assumptions about the nature of negative effects that need to be addressed in treatment. The practice in almost all programs is to devote a portion of the treatment to teaching victims that offenders are entirely responsible for sexual abuse, the underlying assumption being that internalized attributions of responsibility lead to adverse

psychological outcomes. Yet few studies have assessed how often victims actually hold these internalized attributions which need correcting. Since there is some evidence that most young children do not blame themselves (Hunter et al., 1992), it may be that they do not need this component or might even question their own interpretations when the issue is made a central focus of therapy.

Several investigators have attempted to organize targeted interventions that can be applied in a flexible yet systematic way (Friedrich, 1990). Deblinger and colleagues (1990) have designed treatment modules for the various therapeutic targets that may be problematic for child victims, such as fear and anxiety or inaccurate beliefs about sexual abuse. Therapists assess the extent to which each particular target is a problem and assign therapeutic modules accordingly. For those areas in which children evince little or no disturbance, a brief review and clarification is sufficient, whereas substantial focused effort is exerted on problem areas. This approach permits inclusion of children with varying degrees of distress and adjusts the dose of the targeted interventions.

Length of Treatment

There is some debate in the field about the recommended duration of treatment. Most studies have examined relatively brief interventions of between 6 and 20 sessions, but without varying duration as an experimental factor. The psychotherapy outcome literature examining the relationship between duration and treatment effectiveness has generally not shown a clear relationship between dosage and effect, but this may be a function of client, therapist, and contextual factors (Steenbarger, 1994). In regard to sexual abuse, however, Lanktree and Briere (1995) do find that symptoms continue to improve as therapy extends for as long as 12 months.

Evaluating the optimal duration of treatment is a particularly important issue in the current climate of health care reform. Insurance companies are increasingly limiting the number of sessions they approve and requiring documentation for further treatment. It seems likely that for the foreseeable future the emphasis will be on shorter-term treatments that can demonstrate effectiveness in reducing measurable mental health symptoms. If clinicians nonetheless believe that certain children need more extended treatment, it will be important to be able to justify this with research.

Treatment Dropouts

Related to treatment length is the problem of treatment dropouts. Treatment is generally conceptualized and research planned as though children will receive the intervention that is intended. But this is not how it works in the real world. Anecdotal evidence from clinicians in large treatment programs suggests that many children do not complete a full course of treatment or only sporadically attend treatment sessions.

As with the therapy duration issue, virtually none of the treatment evaluation studies to date have examined the problem of treatment dropouts. However, Stauffer and Deblinger (1993) and Deblinger (1994) found that dropping out was more likely with boy victims, children who had fewer symptoms, in minority families, and when only the parent received treatment. It was less likely if the offender was a father. Understanding how these and other potential variables are associated with treatment compliance is a major issue confronting treatment providers and evaluators.

The issues of time limitation and treatment dropouts should perhaps encourage clinicians to consider alternative ways of structuring intervention. One approach would be to offer a brief prophylactic intervention for all children that contains the messages crucial for all victims and families to hear. The treatment would be continued only for those subjects who were assessed to have continuing significant levels of distress and the commitment to proceed. The advantage of this model is that it conforms to the realities and expectations of actual clinical practice.

For researchers, the implications of a high dropout rate include the need to expand the evaluation process to include the study of treatment dropouts. There is much more to be learned about which children drop out and why. Such studies might indicate ways in which current intervention approaches are alienating or not meeting the needs of certain groups of children and families. They might also encourage clinicians to test specific strategies for attracting and holding ordinarily recalcitrant children and families.

Researchers may also want to include dropouts in treatment evaluation designs. Although they cannot be considered as no-treatment controls, they are an important group to consider when judging whether a treatment intervention made a difference.

Abuse-Specific Measures

Most studies of sexual abuse treatment outcome have adopted generic measures like the CBCL, which has been widely used and has well-established psychometric properties. However, if researchers are to test the basic assumption that there are abuse-specific effects that treatment is designed to remedy, then studies must use abuse-specific measures in addition to generic ones. In recent years a number of instruments designed to assess the specific emotional, cognitive, and behavioral impacts of sexual abuse have been developed. They include the Trauma Symptom Checklist for Children (Briere, in press), the Children's Impact of Traumatic Effects Scale (Wolfe et al., 1989), the Children's Attributions and Perceptions Scale (Mannarino et al., 1994), the Negative Appraisals of Sexual Abuse Scale (Spaccarelli, in press), and the Child Sexual Behavior Inventory (Friedrich et al., 1992a). Three abuse-specific scales for assessing parent responses are the Parental Response to Abuse Disclosure Scale (Eversson et al., 1989), the Parent Support Questionnaire, and the Parent Emotional Reaction Questionnaire (Mannarino and Cohen, in press). Evaluations and clinical practice itself might be improved by more widespread use of these measures.

MAKING RESEARCH FINDINGS MORE CLINICALLY RELEVANT

In the research about sexual abuse treatment, as throughout the field of mental health, findings have almost always been presented simply through the comparison of group means and significance tests. A number of analysts have pointed out the shortcomings of this convention (Garfield, 1981; Kazdin, 1977). First, the group comparisons give almost no information on the variability of individual responses to treatment or the fact that some subjects improve, some deteriorate, and some do not change. They do not allow clinicians to try to understand why improvement did or did not occur. A second problem is that the statistically significant changes that are usually highlighted can sometimes be quite small and do not necessarily equate to changes that therapists would judge to be clinically significant.

Jacobson and Truax (1991) have recently suggested some relatively simple conventions and statistical methods to operationalize the notion of clinically significant change. One is to calculate the percentage of subjects

who fall into such categories as those who are truly recovered, those who improved but did not recover, those who were unchanged, or those who deteriorated. They also propose several ways of defining "recovery," such as scoring closer on an outcome measure to the mean of the functional population than to the mean of the dysfunctional population. Sexual abuse treatment research would greatly benefit from the use of these conventions, which can be applied in simple pre-post test designs (Larzelere et al., 1994b). They give useful feedback to clinicians as well as helping clinicians and researchers analyze the specific characteristics of individuals who respond less well to treatment.

CONCLUSION

In this article we have tried to make the case for how evaluation research can provide a very important foundation for improving practice and developing theory in the area of treatment for sexually abused children. Although we have covered a broad range of issues, we see certain particular matters as assuming some priority.

First, given that they are considered the gold standard for judging treatment efficacy in scientific and public policy, some large-scale randomized designs are needed to try to establish clearly the efficacy of treatment for sexually abused children.

Second, given the uneasiness about possible sleeper effects and later developmental consequences of sexual abuse, we also need some long-term follow-up studies of children, including children with no or relatively minor symptoms and children with and without treatment.

Third, the field also needs to adopt quickly a focus that tries to understand and account for treatment failures. These include children who do not improve as well as certain symptom patterns that do not change in spite of treatment.

But beyond these three priorities, what we would most urge is a new appreciation of the possibilities for partnership between treatment and research. The idea that child victims of sexual abuse benefit from therapeutic intervention has taken hold in both the lay and professional communities. As with psychotherapy in general, the question is not simply does treatment work, but what type of treatment with which children for which problems. Research that can inform practice will increase accountability, help provide the justification for treating sexually abused children, and greatly improve outcomes for these children and their families.

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Call for Submissions

The new Clinical Perspectives Section of the *Journal* reflects an attempt to acknowledge formally the value of clinical observations, perspectives, wisdom, guidelines, and pearls. Submissions should be short (750 to 1,500 words), concise, clear, pithy, and focused. Clinical Perspectives reflects the *Journal's* continuing attempt to provide the latest information for clinical practice in addition to building a knowledge base for the field grounded on formal research. We encourage questions, comments, and submissions sent to Michael Jellinek, M.D., Assistant Editor, Clinical Perspectives, Massachusetts General Hospital, 15 Parkman Street—ACC 725, Boston, MA 02114.