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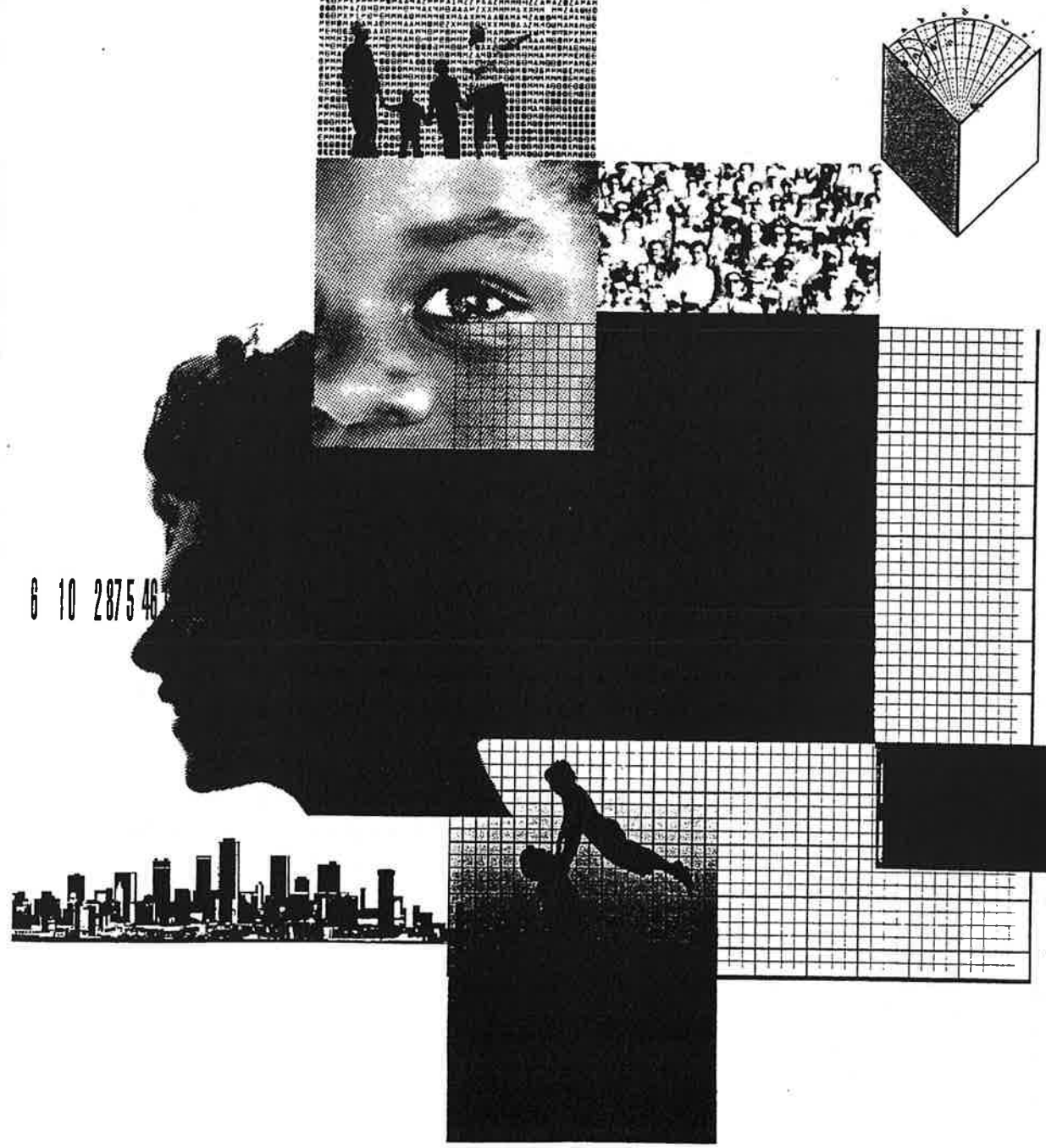

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EDITORIAL

ARTICLES

The effectiveness of EMDR with adult female survivors of childhood sexual abuse

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A randomized experimental evaluation found support for the effectiveness of eye movement desensitization and reprocessing (EMDR) in reducing trauma symptoms among adult female survivors of childhood sexual abuse. Fifty-nine women were assigned randomly to one of three groups: (1) individual EMDR treatment (six sessions); (2) routine individual treatment (six sessions); or (3) delayed treatment control group. A MANOVA was statistically significant at both posttest and follow-up. In univariate ANOVAs for each of four standardized outcome measures EMDR group members scored significantly better than controls at posttest. In a three-month follow-up, EMDR participants scored significantly better than routine individual treatment participants on two of the four measures, with large effect sizes suggestive of clinical significance.

Key words: **childhood sexual abuse; EMDR (eye movement desensitization and reprocessing); practice effectiveness; trauma**

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This article reports the results of a randomized experimental evaluation of the effectiveness of eye movement desensitization and reprocessing (EMDR) in reducing trauma symptoms among adult female survivors of childhood sexual abuse. These survivors, who suffer a variety of symptoms that are persistent and at times debilitating, comprise a large target population for social workers. Although the literature is replete with information on the prevalence and effects of childhood sexual abuse and on practice wisdom about its treatment, very little information is available that examines treatment efficacy. Numerous clinical accounts of treatment with adult survivors of childhood sexual abuse have been published, but controlled treatment research rarely has been conducted with this population (Gordon & Alexander, 1993). Of the studies found that examine treatment efficacy exclusively with this population, none involved the use of random assignment (Alexander, Neimyer, Follette, Moore, & Harter, 1989; Apolinsky & Wilcoxon, 1991; Jehu, 1988, 1989; Roberts & Lie, 1989).

EMDR is a relatively new treatment approach that has from its beginning been fraught with controversy, largely because of its unusual use of eye movements and dramatic claims of rapid efficacy with severely traumatized individuals. According to Shapiro (1995), EMDR is "an interactive, intrapsychic, cognitive, behavioral, body-oriented therapy" whose goal is "to rapidly metabolize the dysfunctional residue from the past and transform it into something useful" (pp. 52–53). Given its claims of dramatic successes within relatively few treatment sessions in this era of managed care, given the promise of the early experimental studies of its effectiveness with traumatized individuals, and given its compatibility with existing practice wisdom about trauma work, testing the effectiveness of EMDR with adult survivors of childhood sexual abuse is both important and timely.

EMDR consists of an eight-phase treatment approach accompanied by specific protocols for different

treatment issues. The eight phases involve (1) obtaining a thorough client history and from that developing a sound treatment plan; (2) preparing the client for using EMDR; (3) assessing the target issue components to be addressed; (4) desensitizing the target material with eye movements or an alternate form of stimulation; (5) installing the desired positive cognition; (6) conducting a body scan to determine if any residual material from the target issue remains; (7) closure; and (8) re-evaluation (Shapiro, 1995).

Phase three is particularly important in that it is when the components of the treatment target are identified. The components of the target include the presenting problem, the memory connected to the presenting problem, a picture or image of the memory to be held in mind (both the negative and desired positive self-assessments associated with the memory), the emotions connected to the memory, as well as any accompanying physical sensations. While holding the picture and positive self-perception in mind, the client is asked to assess on a scale of 1 to 7 how true the positive self-perception feels so that an initial "validity of cognition" (VOC) score can be obtained. With the emotions added to the image and the negative self-perception, the client is asked to assess on a scale of 0 to 10 how disturbing the emotions are so that an initial "subjective units of disturbance" (SUDS) score can be recorded. By recording the SUDS and VOC scores at the beginning of treatment, a baseline can be established from which progress—that is, movement toward trauma resolution—can be monitored. Once a client reports a SUDS score of zero or one, in conjunction with a VOC score of six or seven, the targeted issue is considered to have been resolved (Shapiro, 1995).

During the desensitization phase, while the client is holding all of the identified target components in mind, the therapist induces saccadic eye movements by having the client follow the therapist's fingers in repeated bilateral movements. Typically, the therapist places two fingers approximately 12 to 14 inches from the client's face and begins a set of 24 eye movements. The speed, number, and direction of eye movements, however, are adjusted to meet the needs of each client (Shapiro, 1995). The number and length of sessions needed to achieve measurable change varies on an individual basis, however, 90-minute sessions are recommended, and positive results have been reported after one to four sessions (Forbes, Creamer, & Rycroft, 1994; Shapiro, 1989; Wilson, Becker, & Tinker, 1995).

Although EMDR has tended to yield the most favorable results when it is applied to a single traumatic memory, or a single memory cluster, it is being applied across a broad array of problems, "including agoraphobia and panic disorder, child trauma, learning difficulties, sexual abuse, multiple personality disorder, somatic disorders, chronic depression, obsessive–compulsive disorder, smoking cessation, and eating disorders" (Greenwald, 1994, p. 26). One common assumption of people advocating the broad application of EMDR is that traumatic memories lie at the root of the targeted symptoms (Greenwald, 1994).

At this time it is unknown what might account for the putative effects of EMDR. Although it emerged from cognitive–behavioral therapies, EMDR is not a theory-driven intervention. Speculative explanations for its results have included overcoming blocked neural patterns, mimicking REM sleep, and reciprocal inhibition (Greenwald, 1994). Shapiro (1995) has developed a working hypothesis for an "accelerated information processing" model that she believes represents an innate information processing system. She posits that trauma or stress blocks the information system, thus preventing trauma resolution and leading to the development of pathologies. Gaining access to the traumatic material and activating the information processing system is accomplished through the EMDR protocol, which provides an opportunity for information to be processed to a point of adaptive resolution in an accelerated manner.

In the first randomized experimental evaluation of EMDR, Shapiro (1989) found that one session of EMDR led to dramatic and enduring decreases in trauma symptomatology and improvements in behavior among survivors of combat or sexual trauma. This initial study, although methodologically flawed, launched the beginning of an intense debate on, and examination of, the efficacy of EMDR. It captured the interest of clinicians and researchers alike and as a result, there has been a proliferation of journal articles published, touting or discounting the efficacy of EMDR. To date over 25 case studies and at least 14 controlled studies have been conducted. The vast majority of them focused on the reduction or elimination of symptoms related to posttraumatic stress disorder (PTSD) (Boudewyns, Stwertka, Hyer, Albrecht, & Speer, 1993; Carlson, Chemtob, Rusnak, Hedlund, & Muraoka, 1996; Forbes et al., 1994; Jensen, 1994; Marcus, Marquis, & Sakai, 1996; Montgomery & Ayllon, 1994; Renfry & Spates, 1994; Rothbaum, 1997; Scheck, Schaeffer, & Gillette, 1998; Shapiro, 1989; Spates & Burnette, 1995; Vaughn et al., 1994;

Wilson et al., 1995). In fact, there have been more controlled studies of EMDR than any other method of psychotherapy used to treat PTSD (Solomon, Gerrity, & Muff, 1992).

The uniqueness of the eye movements led several researchers to conduct controlled component analysis studies (Bauman & Melnyk, 1994; Foley & Spates, 1995; Gosselin & Mathews, 1995; Renfry & Spates, 1994; Sanderson & Carpenter, 1992). With the exception of Gosselin and Mathews, each study concluded that eye movements were not an essential component in EMDR. Shapiro (1995) has asserted that various types of lateral stimulation applied within the EMDR protocol accomplish the same effect as eye movements. In fact, in the listed studies, the EMDR procedure, with or without eye movements, typically produced significant reductions in symptoms. However, more often than not, the significant change occurred on the subjective rather than objective standardized measures.

Although the volume of support for EMDR seems impressive, most of the studies that have been conducted have methodological problems that significantly weaken the ability to draw inferences about the efficacy of EMDR. To begin with, the majority of EMDR research to date has been presented in the form of case studies, which, given the absence of controls, makes them inherently limited methodologically. Furthermore, in contrast to the often dramatic positive results of the case studies, several controlled experiments have yielded far more equivocal results (Boudewyns et al., 1993; Foley & Spates, 1995; Jensen, 1994; Pitman et al., 1993; Sanderson & Carpenter, 1992). These studies also had some methodological shortcomings. They have been criticized for misapplying the EMDR procedure, using clinicians who lacked adequate EMDR training, using small samples, or providing an insufficient EMDR treatment regimen to multiply traumatized individuals whose chronic trauma symptoms require more lengthy, comprehensive EMDR treatment protocols (Acierno, Hersen, Van Hasselt, Tremont, & Meuser, 1994; Greenwald, 1994; Lohr, Kleinknecht, Tolin, & Barrett, 1995; Shapiro, 1996a, 1996b).

Some controlled studies that support the efficacy of EMDR (Bauman & Melnyk, 1994; Vaughn et al., 1994) have been criticized for using inappropriate statistical procedures, thus casting doubt on the significance of their findings (Lohr et al., 1995). The methodological limitations most often cited include the lack of confirmed diagnoses based on recently administered, standardized assessment instruments; a lack of clarity regarding whether clinically significant symptoms were present before treatment; failure to use objective, standardized instruments to evaluate outcomes; and vulnerability to measurement bias through an overreliance on therapists' impressions and unstandardized, subjective client self-reports as primary outcome measures (Acierno et al., 1994; Greenwald, 1994; Herbert & Mueser, 1992; Lohr et al., 1992; Lohr et al., 1995).

Particularly vulnerable to measurement bias are the SUDS and VOC scales, which in much of the early EMDR research were the only measures in EMDR research reflecting statistically significant effects. EMDR clinicians are instructed to continue to repeat the saccadic eye movements throughout the session, in an attempt to process the trauma to a point of resolution, which is determined by a SUDS rating of zero or one on a 10-point scale and a VOC rating of six or seven on a seven-point scale. It has been suggested that after a 90-minute session of this, clients might feel a sense of failure or fear of disappointing the therapist unless they report a significantly lower SUDS rating at the end of a session than they did at the beginning. In addition, there has been a potential for therapist bias in the results because the therapists who provided EMDR in many of the studies also evaluated the outcomes. Therefore, it is possible that the effects evidenced by the SUDS and VOC scales were a result of demand characteristics or experimenter bias rather than EMDR (Acierno et al., 1994; Greenwald, 1994; Herbert & Mueser, 1992).

More recent EMDR research has overcome many of the methodological shortcomings of earlier studies and has produced stronger support for its efficacy (Carlson et al., 1996; Marcus et al., 1996; Rothbaum, 1997; Scheck et al., 1998; Wilson et al., 1995). In each of these studies, prior to treatment, the clients were assessed clinically (the majority of whom were diagnosed with PTSD), random assignment was used to compare EMDR with either a delayed treatment control or a comparison treatment, and standardized measures were used and scored by an independent assessor. Consistently, in these studies EMDR was found to be significantly more effective than the comparison treatment or no treatment at all.

Recently, further support for EMDR was noted by Van Etten and Taylor (1998) in their meta-analysis of the efficacy of various treatments for PTSD. Analyzing data from 59 PTSD treatment outcome trials, they concluded that EMDR and behavior therapies, although essentially equal in effectiveness, were both superior in treating PTSD than other

psychotherapies or pharmacotherapies. In addition, Chambless et al. (1998), using rigorous criteria, developed a list of empirically validated therapies that they categorized as either well-established treatments or probably efficacious treatments. They categorized EMDR as probably efficacious for treating civilian PTSD.

Thus, although uncertainty remains as to why EMDR works and some still question the necessity of its bilateral stimulation component, sufficient empirical evidence has accumulated regarding its efficacy with PTSD to warrant an evaluation of its efficacy with additional traumatized target groups with whom it has not yet been evaluated. The need for controlled studies of the efficacy of EMDR for additional target problems or target populations has been noted in the EMDR literature (Lohr et al., 1995; Shapiro, 1995). Although adult survivors of childhood sexual abuse have been included in EMDR research, no EMDR study to date has been focused exclusively on this difficult treatment population. Of particular value would be studies that compare EMDR not only to wait-list control conditions, but also to nonspecific treatment control conditions and that separate the effects of EMDR from the effects of measurement reactivity or therapist expectations associated with the SUDS and VOC components of the EMDR protocol (Lohr et al., 1995). The current study attempts to address this need by evaluating EMDR's efficacy with adult female survivors of childhood sexual abuse.

RESEARCH QUESTIONS

Based on the foregoing material, we sought to answer the following research questions:

- Is EMDR effective in reducing trauma symptoms among adult female survivors of childhood sexual abuse?
- Is EMDR more effective in reducing trauma symptoms than a mix of other individual therapy techniques routinely used by therapists who work with this target population?
- Is EMDR more effective than a mix of other individual therapy techniques in maintaining therapeutic gains as measured at a three-month follow-up?

METHOD

Participant Recruitment Process

Participants in this study were recruited through the use of a newspaper ad and flyers to agencies and clinicians in central Texas. Volunteers were then screened for eligibility in a telephone interview and a 90-minute office interview. To be selected for participation in the study, respondents had to be adult female survivors of childhood sexual abuse who had no previous exposure to EMDR, who exhibited no contraindications for use of EMDR (that is, ocular problems, active suicidal ideation, serious medical condition, inadequate ego strength, or severe mental disorders such as psychosis), and who were not receiving any concurrent therapy. Although PTSD is a common diagnosis for adult survivors, neither it nor any other diagnosis was used as a prerequisite for inclusion in this study. The complexity and broad range of trauma symptoms exhibited by adult female survivors make applying a single specific diagnosis very limiting for generalizing the findings to the adult female survivor outpatient treatment population. Beutler (1993), in an article focused on designing outcome studies on treatments for adult survivors, states that "little is known about the incidence of specific symptoms and syndromes in this population" (p. 403). Had the sample been made up exclusively of survivors with PTSD, the findings could only have been generalized to a population of survivors with PTSD. In addition, restricting the sample only to women diagnosed with PTSD would have lengthened the subject recruitment process prohibitively and probably would have resulted in a much smaller sample size, thereby reducing the power in the study. Eighty-three potential participants expressed interest in participating in the study and of those, 59 met the selection criteria.

Design

The 59 participants were assigned randomly to one of three groups: (1) individual EMDR treatment (n = 20); (2) routine individual treatment (n = 20); or (3) delayed treatment control group (n = 19). Routine individual treatment in this study was defined as a variety of methods, techniques, and theories incorporated into a treatment approach designed to resolve a broad range of symptoms and target problems resulting from sexual abuse trauma and routinely used by therapists who work with adult female survivors of childhood sexual abuse.

Composition of "Routine" Individual Treatment in This Study

We use the term, "routine individual treatment," to label our second group primarily for purposes of simplicity. To determine the types of interventions used in this study under the rubric of "routine individual treatment," each therapist was instructed

to record in their process notes any interventions used. The list of interventions provided on the process notes form was developed with input from the participating therapists before the implementation of the study.

A total of 20 different interventions were incorporated into the "routine individual treatment": support, information, ego strengthening, interpretation, cognitive restructuring, problem solving, dreamwork, neurolinguistic programming (NLP), psychoeducation, behavior modification, gestalt, hypnosis, artwork, assertiveness training, observation of children (a technique often used to help survivors recognize they were not responsible for the abuse by demonstrating how small and vulnerable they were at the time at which the abuse began), writing assignments, relaxation exercises, guided imagery, and visualization. As a result of the unique needs of each survivor as well as the different skills and styles of the therapists, not all recorded interventions were given to every survivor in the routine individual treatment group. The most frequently used interventions were information and support, which were given to all 20 survivors in the routine individual treatment group. The psychodynamically oriented intervention of interpretation was provided to 80 percent (n = 16) of the routine individual treatment group. Other techniques used with great frequency included cognitive restructuring (n = 14; 70 percent), ego strengthening (n = 13; 65 percent), and problem solving (n = 13; 65 percent). Seventy-five percent of the participants (n = 15) received some form of visualization, 60 percent (n = 12) experienced guided imagery, and 45 percent (n = 9) participated in relaxation exercises. Over half (n = 11) of the routine individual treatment participants kept journals as part of their treatment, and 50 percent (n = 10) completed writing assignments. Dreamwork was done with 30 percent of the routine individual treatment group (n = 6). Of the remaining interventions used, all involved four or fewer survivors: behavior modification (n = 4, 20 percent), assertiveness skills (n = 4; 20 percent), psychoeducation (n = 3, 15 percent); NLP (n = 3, 15 percent), observation of children (n = 3; 15 percent), artwork (n = 2, 105), hypnosis (n = 2, 10 percent), and gestalt (n = 1, 5 percent).

It is important to note that some of the techniques used in the routine individual treatment are also incorporated aspects of EMDR. These include support, information, ego strengthening, cognitive restructuring, keeping a journal, and visualization. In addition, because dreams can be specific treatment targets in EMDR, some of the EMDR participants also experienced dreamwork. Because these techniques are an integrated part of the EMDR protocol, the frequency with which they occurred during the EMDR treatment was not measured.

Instruments

Outcome Measures. The participants' trauma symptomatology was measured in pretests and posttests on four standardized instruments that were the primary outcome measures: (1) the state anxiety scale of the State-Trait Anxiety Inventory (STAI) (Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983) that assesses anxiety related to any specific issue of concern; (2) the Impact of Events Scale (IES) (Horowitz, Wilner, & Alvarez, 1979) that assesses posttraumatic stress symptoms for any specific trauma; (3) the Beck Depression Inventory (BDI) (Beck & Steer, 1993); and (4) the Belief Inventory (BI) (Jehu, Gazan, & Klassen, 1985) that identifies and measures common distorted beliefs among adult survivors of childhood sexual abuse.

The STAI has good construct validity as demonstrated by its ability to discriminate between normal and psychiatric patients with anxiety symptoms. It also has good concurrent validity with the IPAT Anxiety Scale and the Taylor Manifest Anxiety Scale, with correlations ranging from .73 to .85 (Spielberger et al., 1983). The internal consistency for the scale is very high, with median alpha coefficients of . 90 (Spielberger et al., 1983). Test–retest reliability is relatively low for the S-Anxiety scale, which is as it should be for assessing situational stress (Spielberger et al., 1983).

The BDI has been shown to have good to excellent validity and reliability. Strong internal consistency has been demonstrated with split-half reliabilities, which have ranged from .78 to .93. Test–retest reliability has been good, with a range of .48 for psychiatric patients after three weeks to .74 for college students after four months (Corcoran & Fischer, 1987).

The IES is very sensitive to change and thus is viewed as an appropriate instrument for monitoring treatment progress (Corcoran & Fischer, 1987). The IES has shown groups validity and very good internal consistency with alphas ranging from .79 to .92.

Although the 26-item version of the BI was used in the study, the reliability and validity information currently available on the BI was developed from the original 17-item version. The inventory has very high test–retest reliability (.93) after one week. Concurrent validity has been established with the BDI (.55).

Subjective Process Measures. The SUDS and the VOC scales were used as in-session process measures. These subjective instruments were selected because they are part of the standard administration of EMDR and have been used in numerous EMDR studies as the primary method of measuring reported change. SUDS (Wolpe, 1990) is a 0- (neutral) to-10- (highest possible disturbance) point scale used to obtain a verbal report from an individual about his or her level of emotional disturbance associated with a specific traumatic experience. It is a scale that is used routinely in systematic desensitization (Shapiro, 1989; Lohr et al., 1992). A change in SUDS level to a 0 or 1 is considered to be a good indication that sufficient desensitization has occurred to proceed with the rest of the EMDR procedure (Shapiro, 1995). Very little information is available in the literature about the validity or reliability of this instrument. Thyer, Papsdorf, Davis, and Vallecorsa (1984) reported that the SUDs scale is correlated with objective measures of physiological stress.

The VOC Scale (Shapiro, 1989) is a semantic differential scale, ranging from 1 (completely false) to 7 (completely true), and is used to assess rapidly the client's cognitive beliefs associated with the trauma. It relies on client verbal self-report before and during administration of the EMDR procedure. A rating of 6 or 7 is considered a strong indication that significant positive cognitive restructuring has occurred. The VOC has face validity, but additional forms of validity as well as reliability have not yet been determined (Shapiro, 1989).

Respondent Characteristics

Demographic Characteristics. The sample consisted of 59 adult female survivors of childhood sexual abuse who were predominately white (85 percent), with a mean age of 35 years and a mean of 15 years of education. The majority of the participants were employed full-time (62 percent) with an additional 15.5 percent working part-time. The mean income for the respondents was $29,178. Thirty-six percent were married, 24 percent were single, 20 percent were divorced, 17 percent were living with a significant other, and 3 percent were widowed. Forty-five percent of the participants had children. The vast majority of the survivors ($n = 52$, 90 percent) had received some type of therapy focused on the sexual abuse issues before participating in the study.

Abuse-Specific Variables. The survivors who participated in this study reported severe abuse histories. The mean age at which the abuse began was 6.5 years, and the mean age at which it stopped was 13 years. Nearly 50 percent were abused for five years or more. For 61 percent of the participants, the abuse occurred between three and four times a month to three and five times a week. During the course of the sexual abuse, almost half ($n = 28$; 47.5 percent) were abused by multiple perpetrators. Men accounted for the vast majority of perpetrators—biological fathers (42 percent), grandfathers (24 percent), brothers or half brothers (23 percent), male friends (18 percent), uncles (15 percent), and stepfathers (12 percent). Mothers were identified as perpetrators by 7 percent of the participants. Most of the survivors also experienced childhood physical abuse (58 percent) and some form of adult revictimization, such as domestic violence and rape (66 percent).

Clinical Symptomatology. Pretest scores revealed that the participants had sufficient symptomatology to warrant seeking clinical treatment. Although a normative sample of working adult females had a mean score of 36 on the STAI, the sexual abuse survivors in the study had a substantially higher level of anxiety reflected in a mean of 59 (Spielberger et al., 1983). On the BDI, the sample had a mean score of 18, which indicates moderate depression (Beck & Steer, 1993). The mean score on the IES was 38. On the IES, scores of this level are indicative of high levels of posttraumatic stress (personal communication, Linda Raab, research assistant to Dr. Horowitz, June 17, 1997). In addition, the women had a mean of 25 distorted beliefs about the sexual abuse where a score of zero is desired (Jehu et al., 1985).

No significant differences were found between groups on the basis of treatment or therapist assignment on any of the demographic characteristics, abuse specific variables, or pretest scores. Furthermore, no significant differences were found on pretests between survivors sexually abused by a single versus multiple perpetrators.

Testing Procedures

During the screening interview, each survivor was asked to select the most troubling issue that she would most like to see resolved. This information was traced by the survivor to a specific memory, which became the treatment target. The treatment target represented only one issue, and as such, was not expected to resolve all issues connected to the sexual abuse. Participants were not informed of their random treatment assignment until after completing the pretests. The principle investigator collected data on the standardized outcome measures at pretest, posttest, and

a three-month follow-up. The subjective data from the SUDS and VOC scales, which were collected only for the EMDR and routine individual treatment groups, were obtained by the therapists at pretest and posttest, and by the principle investigator at the three-month follow-up. The SUDS and VOC have the potential to confound the effects attributed to EMDR. Exposing the comparison (routine individual treatment) group to these instruments without EMDR was intended to control for this confound. Control group members were not included in the SUDS or VOC assessment because having them complete these instruments would have intensified their exposure to their traumatic material while withholding treatment, which was deemed to be ethically unsound.

Therapists

Four white, female therapists participated in the study. All had master's degrees (two in social work and two in psychology). The therapists had a range of 2.5 to 24 years of clinical experience working with sexual abuse survivors. Before treating any participants, each of the therapists, as well as the principle investigator, completed level-two (advanced) EMDR training. Following the training, the principle investigator (a social worker) interviewed each therapist to ascertain pretreatment biases. Only one of the therapists had knowledge about EMDR before being asked to participate in the study. That therapist had been using EMDR for approximately one year and was found to be positively biased toward the method. Of the remaining therapists, after completing EMDR training, one was extremely skeptical to the point of being negatively biased against the method, and the other two therapists were cautiously open-minded and therefore viewed as neutral. Each therapist was randomly assigned five EMDR and five routine individual treatment participants to treat.

Treatment Procedure

The survivors in the EMDR and routine individual treatment groups each received six 90-minute individual therapy sessions. The first session provided the survivor and therapist an opportunity to establish rapport, confirm the treatment target, obtain pretest scores on the subjective process measures, and review the treatment procedure. In addition, the first session was used to facilitate the creation of a safe place. Visualization, guided imagery, and hypnosis were used to create a safe place with the survivors in the routine individual treatment group, and EMDR was used to do it in the EMDR treatment group. The remaining five 90-minute sessions consisted of either EMDR or routine individual treatment (any routine individual treatment combination of techniques, methods, and skills within each therapists' repertoire, that could reduce trauma symptoms). During the routine individual treatment, the therapists were instructed to check and record the survivor's SUDS and VOC scores approximately four times per session. The EMDR protocol does not specify a number of times that SUDS and VOC scores should be checked during a session; rather, the scores are checked as seems clinically appropriate to adequately monitor the client's emotions and cognitions. Therefore, during EMDR, the therapists were not instructed to check the SUDS and VOC scores a set number of times per session. However, in actuality, the average number of times that the SUDS and VOC scores were checked per session was identical (3.2) for both treatment groups.

Participants in the control group were posttested after waiting approximately six weeks from the time of their pretest, the same length of time in which the EMDR and routine individual treatment group members were expected to complete six sessions of individual therapy. After the posttesting with the principle investigator, each member of the control group was referred to a therapist outside the study to receive six 90-minute sessions of either EMDR or routine individual treatment. Given that this was unfunded research, and the therapists in the study were providing their services for free, the principle investigator decided to use therapists outside the study to treat the control group so that the therapists within the study would only be responsible for providing six 90-minute sessions of therapy to 10 women rather than to 15.

Although each session was expected to be held weekly so that each survivor could complete treatment within six weeks, scheduling conflicts resulted in some variations. There was a significant difference between groups on the mean length of time between pretesting and posttesting (10.4 weeks for the EMDR participants, 11 weeks for the routine individual treatment participants, and 7.4 weeks for the control group). However, length of time was used as a covariate to assess for its effects on the posttest results and was found not to be a significant factor.

Fidelity Checks

Intervention fidelity checks were done on each therapist by randomly selecting a client and session to videotape. The tapes were reviewed by an EMDR trainer, a social worker who is now the director of

the EMDR International Association, to assess the appropriateness of EMDR treatment delivery. The EMDR trainer used a standardized evaluation form developed by the EMDR professional association. All four therapists were judged to have administered EMDR adequately. Interestingly, the two therapists with the most clinical experience received the lowest ratings, the skeptical therapist had the highest rating for accurately administering the method and the positively biased therapist had the lowest.

Attrition

No attrition occurred during the pre- and posttest phase of the study, and three-month follow-up data were collected for 52 participants (that is, 20 from the EMDR group, 19 from the routine therapy group, and 13 from the control group). However, from posttest to the three-month follow-up, over half of the survivors (n = 30) obtained counseling services for a mean of five sessions. The largest number of survivors who sought services between posttesting and the three-month follow-up were from the routine individual treatment group (n = 13), followed by the controls (n = 9) and then the EMDR group (n = 8). As could have been expected, all nine control group participants who received therapy after posttesting focused on their selected target memory issue. Such was the case for four of the 13 routine individual treatment participants and one of the eight EMDR participants who obtained therapy from posttest to follow-up. Furthermore, three routine individual treatment, three control, and two EMDR group participants obtained EMDR between posttest and follow-up, creating a potential for comparing EMDR to itself. Those women, as well as the rest of the control group members, were excluded from the follow-up analysis.

The control group members were excluded for several reasons. Because some of the control group received EMDR, some received routine individual treatment, and some received nothing, as a group, they no longer represented a control. In addition, the routine individual treatment and EMDR received by the controls was not monitored or evaluated for treatment fidelity nor were the therapies provided by the same therapists used in the study. Thus, the controls were excluded from the follow-up analysis so that EMDR and routine individual treatment could be more fairly compared to determine how well each maintained therapeutic gains.

Although there was no significant difference between the EMDR and routine individual treatment groups in terms of dropout rates between posttest and follow-up, (0 versus 1), because five participants (two EMDR and three routine individual treatment) obtained EMDR during that period, they were excluded from the three-month follow-up analysis. Therefore, the follow-up analysis included 18 EMDR participants and 16 routine individual treatment participants.

Considering that only the EMDR and routine individual treatment women were included in the three-month follow-up analysis, from posttest to the three-month follow-up, 63 percent (n = 10) of those who received routine individual treatment obtained some type of counseling, whereas only a third of the EMDR participants (n = 6) did. With respect to the mean number of counseling sessions attended, the EMDR and routine individual treatment group means were nearly identical (4.2 and 4.3, respectively). In addition, only three of the survivors—one EMDR and two routine individual treatment participants—focused on the same memory issue as originally targeted in the study. None of these factors were found to be significantly different between the EMDR and routine individual treatment conditions.

RESULTS

Standardized Outcome Measures

The first step in the analysis involved using a multivariate analysis of variance (MANOVA) to test the overall significance of the differences in posttest scores among the three groups across all four standardized outcome measures (STAI, IES, BDI, and BI). MANCOVA had been considered but is not recommended if the covariates have little or no effect. The analysis was run with and without the pretest scores used as covariates; however, no appreciable improvement was noted with the inclusion of the covariates, so they were eliminated. In this situation, MANOVA is a more powerful analytic procedure than MANCOVA. Pillai-Bartlett trace was used as the test statistic because it is the most conservative measure for protecting against Type I errors. Similarly, we used MANOVA (rather than MANCOVA) to test the overall significance of the differences in follow-up scores between the EMDR and routine therapy groups.

Statistical significance on the MANOVA was found at both posttest (n = 59; Pillais = .399) [$F(8)$ = 3.37, $p < .002$] and follow-up (n = 34; Pillais = .319) [$F(4)$ = 3.39, $p < .05$], indicating the groups differed from each other at both points. Using Wilks's lambda, we determined that 39 percent of the variance in the dependent variables at posttest

and 32 percent of the variance in the dependent variables at follow-up were accounted for by treatment condition, both of which are indicative of large treatment effects.

Having found statistically significant differences between groups through MANOVA, we conducted a separate univariate analysis of variance for each measure at posttest and at follow-up to determine which dependent variables contributed to the multivariate significance (Table 1). On the BDI, the results at posttest fell short of statistical significance ($p < .07$) at the .05 level. However, because the univariate analysis was close to the set significance level ($p = .05$), in light of the potential for a Type II error, and because there was potentially clinically significant differences in scores among groups, the results of the contrast procedure were examined. Although the EMDR mean of 10.3 was significantly better than the control mean of 16.7, it was not significantly better than the routine individual treatment group mean of 12.7. The difference between the routine individual treatment and control groups also was not significant. At follow-up, however, the difference between the EMDR mean (4.3) and the routine individual treatment mean (11.9) became significant ($p < .001$), with a large effect size (ES) of 1.29.

On the STAI, the results were significant at both posttest ($p < .001$) and follow-up ($p < .01$). At posttest both the EMDR mean (34.7) and the routine individual treatment mean (40.4) were significantly better than the control mean (54.0), but the difference between the EMDR and routine individual treatment group was not significant. Again at follow-up, however, the latter difference became significant ($p < .01$). And again the ES between the EMDR mean (30.1) and the routine individual treatment mean (41.8) was large (ES = 1.02).

On the IES, the results were significant at posttest ($p < .001$) but not at follow-up ($p = .11$). At posttest both the EMDR mean (14.1) and the routine individual treatment mean (14.0) were significantly better than the control mean (32.1), but there was no difference between the EMDR and routine individual treatment groups. At follow-up the EMDR versus routine individual treatment difference had a moderate effect size (ES = .56).

At posttest on the BI the EMDR (12.1) and the routine individual treatment mean (16.3) were significantly better ($p < .01$) than the control mean (25.1). The difference between the EMDR and routine individual treatment group at posttest was not significant. The difference between the EMDR (8.6)

TABLE 1—Mean Scores, Standard Deviations, and Sample Size on Standardized Measures, by Group and Time

		Pretest		Posttest		Follow-up	
Measure	Group	Mean Score (*SD*)	*n*	Mean Score (*SD*)	*n*	Mean Score (*SD*)	*n*
BDI	EMDR	16.0 (6.3)	20	10.3[a] (7.2)	20	4.3[c] (4.5)	18
	Routine	17.7 (8.9)	20	12.7 (8.2)	20	11.9 (7.1)	16
	Control	19.3 (7.5)	19	16.7 (10.0)	19		
STAI	EMDR	58.4 (9.6)	20	34.7[c] (10.7)	20	30.1[d] (8.2)	19
	Routine	59.8 (11.4)	20	40.4[c] (12.2)	20	41.8 (14.4)	16
	Control	59.4 (12.9)	19	54.0 (17.3)	19		
IES	EMDR	38.7 (16.4)	20	14.1[c] (15.9)	20	10.3 (12.4)	19
	Routine	34.8 (14.6)	20	14.0[c] (12.0)	20	18.0 (15.1)	16
	Control	39.6 (12.5)	20	32.1 (17.0)	19		
BI	EMDR	24.1 (12.2)	20	12.1[b] (10.0)	20	8.6 (9.0)	19
	Routine	25.4 (14.0)	20	16.3[b] (10.4)	20	15.3 (12.4)	
	Control	27.1 (15.2)	19	25.1 (15.3)	19		

NOTES: BDI = Beck Depression Inventory (Beck & Steer, 1993); STAI = State-Trait Anxiety Index (Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983); IES = Impact of Events Scale (Horowitz, Wilner, & Alvarez, 1979); BI = Belief Inventory (Jehu, Gazan, & Klassen, 1985). Scale ranges for each instrument are as follows: BDI, 0–63; STAI, 20–80; IES, 0–75; BI, 0–104.
[a]Significantly better than control at $p < .05$.
[b]Significantly better than control $p < .01$.
[c]Significantly better than control at $p < .001$.
[d]Significantly better than routine individual treatment at $p < .01$.
[e]Significantly better than routine individual treatment at $p < .001$.

and routine individual treatment (15.3) means grew at follow-up, with a moderate ES of .63, however this difference fell short of significance (p = .08). EMDR produced a composite ES of 1.46 on the outcome measures at posttest and a composite ES of 1.08 at follow-up.

Subjective Outcome Measures

The MANOVA to test the overall significance of the differences in posttest scores between the EMDR group and the routine individual treatment group on the SUDS and VOC found statistical significance at both posttest [n = 40; Pillais = .341; $F(2)$= 9.32, p < .001] and follow-up [n = 34; Pillais = .345; $F(2)$= 8.16, p < .001]. Wilks's lambda revealed that 34 percent of the variance in the subjective dependent variables at posttest and 35 percent of the variance in the subjective dependent variables at follow-up were accounted for by treatment condition, indicative of large treatment effects by EMDR.

In the univariate analysis, the SUDS posttest mean of the EMDR group (1.2) was significantly better (p < .001) than the routine individual treatment posttest mean (4.1) (Table 2). At follow-up, the EMDR mean stayed at 1.2, and the routine individual treatment group mean dropped a bit to 3.7. This difference was statistically significant (p < .001), and the ES of 1.35 was large. The VOC posttest mean of the EMDR group (6.8) was significantly better (p < .001) than the routine individual treatment posttest mean (5.1). At follow-up, the EMDR mean was 6.6, compared with a mean of 5.4 for the routine individual treatment group (p < .01, ES = 1.06). EMDR produced a composite ES of 1.36 at posttest and 1.38 at follow-up on the subjective measures.

CLINICAL SIGNIFICANCE

The statistical significance of the reported results generally support the effectiveness of EMDR in reducing trauma symptoms among adult female survivors of childhood sexual abuse. But what about the clinical significance of these findings?

On the STAI, pretest scores revealed the sample had sufficient symptomatology to warrant seeking clinical treatment, with all three groups ranked in the 95th percentile of the normative group, indicating the presence of a high degree of trauma-specific anxiety. By posttest, however, the EMDR group mean (34.7) on the STAI was somewhat lower than the norm group mean of 36, indicating the EMDR group was no longer exhibiting clinical symptoms of trauma-specific anxiety, while the control group posttest mean (54) remained very high (92nd percentile). On the IES at posttest the control group mean of 32 was more than twice as large as the EMDR mean (14.1). Whereas the EMDR group mean still reflected a medium level of trauma-specific posttraumatic stress, the control group remained at a high level. On the BDI the EMDR group was exhibiting mild levels of depression at posttest, whereas the control group was experiencing a moderate level of depression. On the BI the EMDR group mean of 12 at posttest was less than half of the control group's mean of 25. Although the BI does not possess a clinical cutoff, these results indicate that the EMDR group mean of negative beliefs

TABLE 2—Mean Scores, Standard Deviations, and Sample Size on SUDS and VOC, by Group and Time

		Pretest		Posttest[a]		Follow-up[b]	
Measure	Group	Mean Score (*SD*)	*n*	Mean Score (*SD*)	*n*	Mean Score (*SD*)	*n*
SUDS	EMDR	7.5 (2.0)	20	1.2[c] (1.3)	20	1.2[c] (1.2)	18
	Routine	7.8 (1.6)	20	4.1 (3.0)	20	3.7 (2.4)	16
VOC	EMDR	2.7 (1.3)	20	6.8[c] (0.5)	20	6.6[d] (0.6)	18
	Routine	3.1 (1.5)	20	5.1 (1.7)	20	5.4 (1.5)	16

NOTES: SUDS = subjective units of disturbance; VOC = validity of cognition. Scale ranges for each instrument are as follows: SUDS, 0–10; VOC, 1–7.
[a]At posttest, 65.0% of the EMDR group and 25.0% of the routine group obtained a score of zero or one, indicating resolution of original target (p < .05).
[b]At follow-up, 61.0% or the EMDR group and 12.5% of the routine group obtained a score of zero or one, indicating resolution of original target (p < .01).
[c]Significantly better than routine individual treatment at p < .01.
[d]Significantly better than routine individual treatment at p < .05.

about the sexual abuse had dropped by 50 percent from pre- to posttest, whereas the control group mean dropped by only 7 percent. Thus, there appears to be clinically significant difference on our standardized outcome measures between the EMDR and the control groups.

Less clinical significance can be claimed for the results connected to our research questions comparing EMDR to routine individual treatment. None of our posttest comparisons between these two groups was statistically significant on any of the standardized measures. However, a statistically significant posttest difference was found between the two treatment groups on the subjective process measures: The EMDR group mean scores were better on both the SUDS and VOC measures than the mean scores for those in the routine individual treatment. Although the SUDS and VOC have many limitations, if we accept the notion presented in the EMDR training, that a SUDS score of zero or one, in conjunction with a VOC score of six or seven, indicates resolution of the targeted traumatic memory, then at posttest 65 percent of the EMDR participants reached resolution compared with 25 percent of the survivors in the routine individual treatment ($p < .05$).

At follow-up, a MANOVA found a statistically significant difference and a large effect size in comparing the EMDR and routine treatment groups. Two of the four univariate comparisons were statistically significant, with large effect sizes (1.0 on the STAI and 1.3 on the BDI). Although the EMDR group exhibited no clinically significant levels of trauma-specific anxiety or depression at follow-up, the survivors who had been in routine individual treatment were exhibiting above average trauma-specific anxiety and mild depression. The MANOVA for the subjective measures also was statistically significant at follow-up, with a large effect size. Furthermore, 61 percent of the EMDR participants continued to meet the criteria for resolution of the targeted traumatic memory, compared with only 12.5 percent of the survivors in the routine individual treatment.

DISCUSSION

On every outcome measure the EMDR participants scored significantly better than controls at posttest, indicating that EMDR reduced trauma-specific anxiety, trauma-specific posttraumatic stress, depression, and negative beliefs. Statistically significant decreases in trauma-specific emotional disturbances, as well as increases in desired positive self-referencing beliefs, also were noted when comparing pretest with posttest scores within the EMDR group. Moreover, all of these results were clinically significant.

In addition, the multivariate analysis of variance conducted on the three-month follow-up data for the EMDR and routine individual treatment groups found that EMDR was more effective than routine individual treatment at maintaining therapeutic gains. EMDR was particularly more effective than routine individual treatment at maintaining therapeutic gains in reducing trauma-specific anxiety and depression. On the basis of scores on the subjective measures, EMDR was also more effective than routine individual therapy at maintaining therapeutic gains in reducing trauma-specific emotional disturbances and increasing desired positive beliefs about a specific trauma memory/issue. However, the follow-up findings should be viewed with caution, because some participants in both treatment conditions continued receiving some treatment after posttesting.

EMDR produced significant effects despite the fact that the therapists in the study had limited experience using EMDR and had more experience in using routine individual treatment. In addition, given the concerns for therapist bias affecting treatment outcomes, it is important to note that although therapist bias was present in the study, analysis of the data collected at posttest and the three-month follow-up found no significant differences in outcome based on therapist assignment. Therefore, EMDR appeared to be equally effective among the therapists despite their positive and negative biases, and differences in years of overall clinical experience.

Given that the EMDR mean scores were at least as desirable as those of the routine individual treatment group on all measures at posttest and more desirable than routine individual treatment mean scores at follow-up, clinicians considering gaining proficiency in EMDR and providing it to adult female survivors of childhood sexual abuse are encouraged to go ahead in that direction—at least for the time being, while awaiting additional research on the efficacy of EMDR with this population.

Despite our findings of support for the effectiveness of EMDR, no significant differences between EMDR and routine individual treatment were present on any of the standardized measures at posttest. Consequently, we cannot conclude that EMDR is superior at posttest to routine individual treatment. It is possible that the lack of posttest difference between EMDR and routine individual treatment was influenced by the incorporation of aspects

of EMDR's treatment protocol. For research purposes, some of the EMDR protocol, which does not typically exist in routine individual treatment, was imposed on that therapy process: selecting a specific issue, relating that to a specific image or memory, identifying the associated negative emotions and beliefs as well as the desired positive cognitions and accompanying body sensations. However, it would have been difficult to construct the routine individual treatment protocol any differently and to answer the research questions and hypotheses.

Also, the lack of statistical significance between EMDR and routine individual treatment participants at posttest should be interpreted with caution because with our resource limitations we were able to obtain only 59 clients in the study. This means there is a big risk of a Type II error. Perhaps with a larger sample, some of the EMDR and routine individual treatment posttest differences would have been statistically significant.

Our limited resources also forced us to restrict the treatment regimen to six sessions focused on one specific treatment issue. It is conceivable that we would have found greater differences had posttesting occurred after many more than six sessions. In all likelihood, the number of sessions provided in the study were too few to adequately address all of the troubling issues the survivors in the study were confronting. Thus, although the time-limited treatment provided in the study helped alleviate trauma symptoms, longer-term treatment probably would be needed to address more than one specific issue. This point seems supported by the fact that so many survivors obtained additional therapy between posttest and the three-month follow-up.

Moreover, our limited number of sessions as well as the nature of the research questions did not allow for EMDR recipients to receive anything in addition to the EMDR regimen. In reality, most practitioners would incorporate EMDR into their existing practice modalities rather than practicing EMDR exclusively. In fact, Shapiro (1995), who originated EMDR, advocates its use not instead of other therapies, but along with them. This recommendation is particularly applicable to target populations with chronic symptoms associated with traumas that occurred much earlier in their lives. Future investigators of EMDR's effectiveness with adult female survivors of childhood sexual abuse should seek the resources that would permit extending the number of sessions to 12 and testing the efficacy of EMDR as part of routine individual treatment compared to routine individual treatment without the EMDR component.

Given the lack of significance between EMDR and routine individual treatment clients at posttest, our findings may spur one to wonder why this comparison became significant in the three-month follow-up. It appears unreasonable to attribute this change primarily to an inability of routine individual treatment clients to maintain the gains they made during treatment. We say this because, for the most part, the scores of routine individual treatment clients did not deteriorate very much from posttest to follow-up. In fact, on the BDI they continued to improve somewhat (and the EMDR clients continued to improve dramatically). Similar findings have also been noted by Van Etten and Taylor (1998). They found in their meta-analysis that EMDR maintained its effects at follow-up, and "effect sizes tended to increase at follow-up, whereas behavior therapy effects remained stable but did not increase notably" (p. 17). Perhaps there is something to Shapiro's (1995) claims that the EMDR intervention, more than other interventions, enables clients to continue processing a resolution of the traumatic memory after the EMDR treatment has ended. Greenwald (1994) has supported this notion, indicating that postsession processing can go on for days or weeks. It is possible that the continued processing results from a generalization of treatment effects that may occur through EMDR whereby the positive effects of working through one traumatic memory are generalized to associated memories, thereby further reducing a client's trauma symptoms (Shapiro, 1995). The plausibility of this notion is supported by the consistent gains of EMDR recipients from posttest to follow-up. Although we cannot claim to have demonstrated empirically that such an explanation is the case (especially in light of the fact that some participants continued treatment during the three-month follow-up phase), our data would seem to imply this as another line of inquiry for future EMDR research with this population.

Additional research in this area is needed also in light of the limitations of this study. We cannot rule out the possibility that the superior results of either group, compared to controls, was influenced by reactivity factors associated with the use of the SUDS and VOC—which were used with both treatment groups, but not with the wait-list group. Likewise, our posttest results do not indicate the role of nonspecific treatment factors influencing EMDR outcome. Neither do they bear on the dispute over the necessity of specific treatment components, such as saccadic eye movements, in achieving specific outcomes. Furthermore, as in most randomized field

experiments, we were unable to randomly select volunteers for our study from the population of adult female survivors of childhood sexual abuse. Consequently, it is conceivable that the survivors in this study may not represent the population of adult female survivors of childhood sexual abuse. Perhaps the fact that they all resided in central Texas at the time of this study may make them unlike survivors elsewhere in their ability to benefit from EMDR treatment. Also, the experimental nature of the research necessitated the use of voluntary participants. Perhaps there is something unique about survivors who volunteer for such an experiment.

The use of pretests could have contributed to reactive testing effects, which would further limit the external validity of our findings. The therapists in this study were not blind to the experiment and in fact, two were found to be biased. The presence of therapist bias, as well as the use of the SUDS and VOC scales, could have created a vulnerability to demand characteristics. In this study, however, termination of each session and of the total treatment was determined by a preselected period of time and not based on any particular SUDS score, and both the EMDR and routine individual treatment groups were exposed to an equal number of in-session measurements of the SUDS and VOC scales. In addition, there were no significant outcome differences based on therapist assignment despite the presence of biases for and against EMDR. Therefore, it seems unlikely that the significant differences between EMDR and routine individual treatment found on the SUDS and VOC scales at posttest and follow-up were the result of demand characteristics.

CONCLUSION

We believe that despite its limitations, this study advances the empirical knowledge base about the use of EMDR in intervening effectively with adult female survivors of childhood sexual abuse. We recommend continued use of EMDR, in combination with other treatment methods and techniques, with this target population while we await more conclusive evidence about its efficacy. ■

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