EYE MOVEMENT DESENSITIZATION AND REPROCESSING
A Chronology of Its Development and Scientific Standing

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The development of eye movement desensitization and reprocessing (EMDR) has been hotly debated, with rhetoric often being posited as evidence either for or against the technique. This paper aims to provide a brief overview of the procedure, a critical review of the studies completed to date, a meta-analytic review of the available data, and a chronology of the evolution of EMDR over the past 10 years. Treatment-outcome studies were of such disparate quality—even studies meeting similar broad criteria—that combining their results in a meta-analysis was of very questionable value. Overall, an appraisal of the published research supported the following conclusions: (1) There is overwhelming evidence that eye movements are neither a necessary nor a useful addition to the procedure; (2) there is strong and consistent evidence that EMDR is better than no treatment, yet only as good as any other treatment that utilizes some aspect of exposure therapy; and (3) there is strong evidence that a full-exposure-based intervention package is superior to EMDR. There is also some evidence that "reprocessing" is likewise superfluous to EMDR and that the effects of EMDR dissipate over time. It is also concluded that the current debate cannot be entirely settled through scientific investigation due to the rapid and constant reshaping of what constitutes EMDR, the similarity to extant alternative methods, and the lack of a falsifiable theory underpinning the procedure.

Atreatment for posttraumatic stress disorder (PTSD) has been proposed by Shapiro (1989b) involving ocular tracking, by the client, of a bilaterally moving stimulus while holding in mind a mental representation of the traumatic event. Shapiro claimed to have happened upon this procedure, eye movement desensitization (EMD), through serendipity. During a now famous walk in the woods (Rosen, 1995, 1997; Shapiro, 1995; Welch, 1996), while thinking of an anxiety-provoking situation, she noticed that her eyes were involuntarily moving in a multisaccadic manner, followed by the disappearance of these anxiety-provoking thoughts. Shapiro also noted that following this procedure she, and later her clients, found it very difficult to retrieve the memory of the anxiety provoking material, and that even when the thoughts were deliberately retrieved, she found that their anxiety valence had dissipated. This treatment procedure was first investigated by Shapiro in 1989(a) and the method of treatment quickly developed to the point of workshop training (Shapiro, 1992).

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During the development phase of the process, the acronym "EMD" grew into EMDR (eye movement desensitization and reprocessing), to keep in line with the descriptive rationale for its effects. It has now been promoted as an unique, fast-acting, and effective intervention method for treating a plethora of problems including, but not limited to, substance abuse (Shapiro, Vogelmann-Sine, & Sine, 1994), personality disorders (Fensterheim, 1996), PTSD (Shapiro, 1989b), sexual dysfunction (Wernik, 1993), dissociative disorders (Paulsen, 1995), body dysmorphic disorder (Brown, McGoldrick, & Buchanan, 1997), and morbid jealousy (Blore, 1997). EMDR has even been advocated for enhancing performance in athletes (Foster & Lendl, 1995). Such claims have been partly responsible for the development of a growing rift within clinical psychology. Critics claim that the level and method of marketing EMDR has outstripped its evidence, and proponents claim that EMDR is being treated unfairly by the academic "fraternity" who expect a higher standard of evidence than would be accepted for other, more conventional treatments.

Whatever the truth, widespread and vigorous application of any therapeutic technique requires evidence that the procedure is not just more effective than a wait list condition, but that there are incremental effects over placebo treatments. It is also desirable that the mecha-

nism responsible for improvement in patient functioning is isolated. Furthermore, evidence is needed that the intervention is at least effective as standard clinical care. It is the purpose of this article to address these issues. In order to accomplish this goal, the treatment-outcome literature was critically reviewed, effect size estimates for completed studies were derived, and the results were integrated into an overall "state of affairs." In addition, a chronological review of EMDR research and of EMDR protocol development was conducted in order to provide the reader with an appreciation of why this area of research is so controversial. Past reviews of the EMDR literature have focused on methodological critiques of the EMDR research (Lohr et al., 1993), efficacy of EMDR for specific conditions (Lohr, Tolin, & Lilienfeld, 1998), specific and nonspecific factors involved in EMDR (Lohr, Lilienfeld, Tolin, & Herbert, 1999), and factors involved in the dissemination and promotion of EMDR (Herbert et al., 2000). However, this review adopts a chronological approach to the controversies concerning EMDR, providing a historical context upon which effectiveness and efficacy can be judged and a presentation of the methodological weaknesses in the research that have been addressed over time.

In order to conduct this review, a literature search was performed on PsychInfo using the keywords "eye movement desensitization and reprocessing" (and the acronyms "EMD" and "EMDR"), "treatment," "outcome," and "controlled." Literature reviews by other authors were also checked for studies, and Web sites (particularly that of the EMDR Institute) were checked for additional studies. In order to provide a chronology of the debate, some single case studies that were of importance in the early stages (1989 to 1994) were included in the critical review, but otherwise all controlled clinical outcome studies that were in a publishable format and available for independent review were selected.

EMDR Background

Shapiro (1989a, 1989b) suggested that any therapist using EMDR should be properly trained by herself and, later (1991b), that any research carried out by untrained therapists would be invalidated by this lack of skill. Although the level of EMDR training has yet to be determined to correlate with outcome (see Rosen, 1999), Shapiro has discussed at length the development and evolution of EMDR in several articles (e.g. Shapiro 1989, 1994, 1995, 1997, 1999). However, in light of the review of the literature later in this article, a brief outline of the technique is presented below as taught at training workshops in 1992, and the development of the EMDR procedure is further examined at the end of this review.

The Components of EMDR

As within any therapeutic framework, it is important to establish rapport with the client in order to engender trust and make it clear that one is not simply applying a "quick fix" without understanding the person. During this process the therapist should move from history taking to identifying the presenting problem and obtaining some idea (preferably with quantification) of how it interferes with daily functioning. Next, she should introduce the process and provide the client with a suitable rationale, appropriate to his level of understanding, of how the technique works, what he can expect during the session, and how it may affect him later. With respect to the rationale of EMDR, the following has been typical:

> Traumas cause a pathological change in the brain at the neural level resulting in these incidents becoming "locked" in the nervous system and not being processed in the normal way and, therefore, not being dealt with. Repetitive eye movements may be the body's natural way of desensitizing the person to the memory and so, inhibiting anxiety, the traumatic "overload" becomes resolved. Do you have any questions?
>
> I will be moving my hand in front of you at about this level [demonstrate] and sweeping it back and forth across your field of vision like this [demonstrate]. I want you to keep track of my finger tips with your eyes and without moving your head. (Shapiro, 1992)

Aprototypical transcript explaining the procedural questions reads as follows:

> What we will be doing is often a physiology check. I need to know from you exactly what is going on with as clear feedback as possible. Sometimes things will change and sometimes they won't. I may ask you if the picture changes—sometimes it will and sometimes it won't. I'll ask you how you feel from "0" to "10" sometimes it will change and sometimes it won't. I may ask if something else comes up—sometimes it will and sometimes it won't. There are no "supposed to's" in this process. So just give as accurate feedback as you can as to what is happening, without judging whether it should be happening or not. Just let whatever happens—happen. (Shapiro, 1988, p. 66)

Next, the therapist should instruct the client to generate an imaginary, representative picture of the distressing issue (or associated bodily sensation) that is cen-

tral to his problem. Having done this, it is easier to obtain his negative belief statement about the event. An example of this would be, "I could have done more to help." Then she should obtain the client's desired (though realistic) positive cognition for this same event (e.g., "I did the best I could"). For both of these Shapiro (1989a) suggested that a validity of cognition (VoC) rating of how much the client believes each of these statements (on a 1–7 scale) should be made. Having completed this much, the therapist should then discuss the emotions that the mental picture evokes and also identify the body sensations that accompany these emotions. Next, she should acquire a measure of the degree of anxiety/disturbance that this picture/feeling/sensation/cognition evokes by a Subjective Units of Disturbance Scale (SUDS) from 0 to 10. Following these steps, the actual EMDR process itself is applied.

While the client concentrates on the imaginary picture and accompanying body sensations, he is instructed to concentrate on the therapist's first two fingers, which are moved rapidly back and forth across the line of vision 12–14 inches away from the face. Each sweep should cover the extreme left and extreme right of the field of vision (at least 12 inches) at the rate of two back and forth movements per second. This rate may vary depending upon the individual's ability to track the movements. Initially the direction should be on a horizontal plane, but if this has little effect it can be changed to a diagonal, vertical, or circular motion in order to "accommodate individual client differences." Twenty-four back-and-forth sweeps should be given for each set, although if a high level of emotional distress is noticed, these movements should be continued until a plateau in affect is reached. Throughout nurturing prompts may be given (e.g., "good," "well done," "that's it"). At the end of one set, the client is instructed to "blank it out and take a deep breath." Following this, he is are asked whether anything else came up. If it did, he should concentrate on it for the next set of eye movements until it is desensitized. If not, the client is instructed to bring the picture/feeling/cognition up again and give it a SUDS rating. This process is continued until a rating of 0 (no anxiety) is obtained and the issue is desensitized. The desired cognition is then concentrated upon during the eye movements until a VoC rating of 7 (completely true) is obtained. This cognition and the original issue are then linked together during the eye movements by keeping both in mind (referred to as the "installation" process; Shapiro, 1995), and finally a body scan is completed, checking for any physiological residue. If there are any signs of tension, the eye movements are again induced, while the client concentrates on these sensations until they have dissipated. It should be noted that this is an outline of the procedure as used by Shapiro (1989a) and taught at trainings until at least 1992. An analysis of the progression and addition of components to the EMDR protocol over time will be presented later in this article.

However, first we should inspect the theoretical underpinnings of any treatment, as it is within this context that techniques are usually taught and disseminated among professionals.

THEORIES OF EMDR ACTION

Shapiro (1989a) discussed the similarity between the EMDR eye movements and those of REM sleep. Although this proposal initially seems attractive and the little-understood information processing perspective that it implies appears credible, there are important inadequacies in this assertion. First, Shapiro described EMDR as inducing multisaccadic eye movements. This is not always the case: Following the researcher's finger movements creates a smooth pursuit eye movement at the lower speeds sometimes used by EMDR therapists. Although similar to REM movements, they are not the same. Second, there are no known neural mechanisms that connect eye movements to neurological structures of memory. Shapiro also talked of Pavlov's (1927) Excitation-Inhibition theory, whereby if at some point in the cortex excitation and inhibition conflict, the resultant state is a pathological change and the subject may display associated symptoms. Shapiro suggested that this is what happens in cases of PTSD: The subject receives a type of information overload and the trauma is left in a "frozen state." She suggested that this primed state is then unblocked by the EMDR, in a hitherto unexplained manner, and the information can be processed in the normal way. However, although conceptually Pavlov's theory may provide an explanation, localizing memories (as Pavlov originally did) is not congruent with psychophysiological findings (Bower & Hilgard, 1981). The implication that eye movements are linked to long-term memories has, therefore, very little theoretical grounding. This gap underscores the need for research to examine the utility of eye movements within EMDR, the overall effect of the treatment method, and the efficacy of EMDR relative to other treatment options.

Dyck (1993) proposed a counterconditioning model to explain the efficacy of EMDR with PTSD whereby: (a) the attendance at therapy initially acts as a form of nonavoidance in confronting the anxiety associated with the

trauma; (b) the eye movements initially act as a distraction to the exposure, which results in nonreinforcement of the fear response; (c) subsequent trials of eye movements act less as a distraction and the client is more fully exposed to trauma related thoughts and the associated anxiety; and (d) this process generalizes over time, leading to an extinction of the fear response and attendant symptomatology. However, the weakness in this model lies in the theory that the client pays less attention to the distraction and that the exposure that occurs is sufficiently complete to promote extinction of the fear response and to provide corrective information. If the exposure is not complete, one would expect relapse at a later date, a phenomenon noted in the PTSD literature (Devilly & Foa, 2001; Jaycox, Foa, & Morral, 1998) and in EMDR research specifically (Devilly, 2001a). It is suggested that a more parsimonious adaptation of Dyck's theory to practice would be the utilization of distraction techniques during exposure to *initially* engage a reticent client in therapy, but that other, more encompassing exposure techniques be used later to ensure full extinction of the fear response and promote new, more adaptive learning.

A more recent comment (Devilly, 2001a) suggests that EMDR is a variant of exposure (Foa & Kozac, 1986) that does not always extinguish the fear response in some clients, resulting in sensitisation in the long term as opposed to desensitization.

CRITIQUE OF EMDR RESEARCH

The following section provides a historical review of the EMDR literature up to the end of 2000. This method of evaluation, rather than delineating sections by population-specific criteria, has been adopted to facilitate an understanding of the development of the EMDR movement.

1989–1992

Shapiro (1989a, 1989b, 1990) reported on the efficacy of eye movement desensitization (EMD), as it was then known, for traumatic memories among 22 Vietnam veterans and rape/molestation victims. Controls were given the same instructions as the treatment group but were not given the eye movements. The controls were, therefore, required to provide imaginal descriptions of the experience and describe the body sensations they experienced and their cognitions about the event. According to Shapiro (1989a, 1989b) the treatment group experienced a remarkable recovery, suggesting that "a single [90-minute] session" was sufficient to completely desensitize a traumatic memory and promote more adaptive and realistic cognitions. One- and three-month follow-ups yielded no evidence of relapse. The controls displayed no significant change in symptomatology, although when subsequently treated with EMDR they showed the same level of improvement as the initial treatment group.

There are, however, several methodological limitations to Shapiro's (1989a) study that limit the conclusions that can be drawn. A major criticism of this study is that it failed to address the central issue, namely, whether the eye movements are responsible for the reduction in PTSD symptoms, or whether other aspects of the treatment are responsible for the reported improvement. For example, distraction techniques have long been used in the field of pain management (e.g., Devilly & Sanders, 1993; Fordyce, 1975; Vessey, Carlson, & McGill, 1994) and the eye movements may have just been a distraction (which the control group did not receive) that reduced anxiety levels during the sessions. However, this does not explain the longer-term improvement that was obtained, which now appears inconsistent with more recent research (see Devilly, 2001a, for a discussion of the role of distraction in anxiety and the use of EMDR). Alternatively, exposure to the retrieved trauma memories may explain this improvement in functioning.

Furthermore, Hedstrom (1991) argued that inducing eye movements promotes relaxation, as measured by alpha waves. Extrapolating from this result, it may be argued that Shapiro's control group was less relaxed during the treatment and the process of reciprocal inhibition, of imagining a traumatic scene and also being relaxed, was not induced. Therefore, the control group responded poorly compared with the experimental group. However, this explanation assumes that the eye movements are pertinent to treatment delivery and that relaxation promotes desensitization, a view that is not generally held within anxiety research.

Moreover, the subjects used in this study were not all suffering from PTSD, and the "events" that caused their distress were not of a similar variety (e.g., rape). Therefore, it could be argued that this introduced too much heterogeneity within the sample. However, a positive side to this seminal research is that Shapiro's results did allow for future research to conduct power analyses in preparation for dismantling studies. Of interest, though, is that an a priori power analysis for an as yet untested hypothesis (which Shapiro originally possessed) even when liberally assuming a one-tailed ($\alpha = .05$) analysis with a large effect size (0.8) and acceptable power (0.8), would normally require at least 21 subjects in *each* condition.

However, a major criticism of Shapiro's research is that all the outcome data were based upon self-reported "subjective units of disturbance" (SUD) levels, usually seen as a process variable, and upon a "validity of cognition" scale. This VoC has to date not been psychometrically assessed and no research on its utility and sensitivity as a treatment outcome measure has been reported. There were no standardized, reliable tests with validated psychometric properties utilized to measure outcome, and thus there is no certainty that all the subjects were suffering from PTSD in the first place. Further, the study was conducted by a single therapist (Shapiro) who administered therapy to both conditions and all outcome data was obtained "face-to-face" by the same therapist. The demand effects of such an approach can not be overstated, and the effect of experimenter bias has been well documented throughout clinical research (see Rosenthal, Persinger, Kline, & Mulry, 1963; Devilly, 2001a, for a summary of its possible implications for EMDR research). Because Shapiro has never conducted another group treatment-outcome study on EMDR, the above weaknesses have not been addressed using the same therapist. However, this was a novel first investigative study, the results of which warranted further attention.

The apparently stunning results of Shapiro (1989a) spurred much interest into EMDR (Buttsworth, 1990; Robinson, 1992; Shapiro, 1991) and research subsequently began to grow. Marquis (1991) reported on 78 cases of trauma treated with EMD in general practice and the treatment again evidenced remarkable effectiveness. However, a confounding variable within the Marquis study was that many of the clients were also receiving other forms of intervention, such as relaxation and cognitive restructuring. Furthermore, the diagnostic criteria for PTSD in this study were not clear and outcome was poorly measured with no standardized instruments. Another major limitation of this research was the lack of any comparison conditions. Moreover, it is not clear whether all clients who received EMD from the clinic were included in this study's results, which attempted a naturalistic design. This is an important point, one raised again later: There is a basic need for scientific inquiry to distinguish within its designs and statistical analyses between a priori and post hoc testing. For example, were clients who were seen in some unspecific way as "unsuitable" not included as participants? Were those who were responding poorly during treatment removed from the study and treated with another therapeutic technique? What were the attrition rates based on treatment factors?

In similar fashion, Wolpe and Abrams (1991) reported on a very successful case study of EMD with a rape victim, but again other techniques (e.g., relaxation and in vivo exposure) were incorporated into the treatment. Therefore, this study affords us with little insight into the effectiveness of the EMD technique specifically. Wolpe and Abrams also made very sparing use of objective measures, as was the case with another study in the same year. Puk (1991) described two case studies using EMDR, one an adult who was sexually molested as a child, and the other a woman with intrusive recollections of her sister's final stages of lung cancer. Both were reported as resounding successes, but self-reported anxiety levels were the only form of assessment. Although clinical anecdotes and process measures are of interest and can at times be instructive, they rarely make for a scientific grounding for the acceptance or rejection of a hypothesis.

Herbert and Mueser (1992) drew attention to this deficit in EMDR research, pointing out that in Shapiro's initial study no objective or standardized measures were used to validate the self-report data. Further, they made the point that there were extreme demand effects in the therapeutic process that may have encouraged the subjects to report lowered anxiety levels after treatment. Also, due to the use of few standardized inventories in past EMDR studies, there was little evidence as to how many of the subjects in the EMDR literature actually met the *DSM–III–R* criteria for PTSD, besides being "bothered by traumatic memories." These authors also raised questions as to the widespread use of EMDR in clinical practice prior to properly controlled and rigorously assessed research. They also devoted space to criticizing a new phenomenon: the restrictions upon the freedom of the scientific community to openly investigate the efficacy of EMDR due to Shapiro's methods of dissemination. For example, they discussed the insistence (via a legally binding and signed document) that no trainee could teach EMDR without Shapiro's consent and that the documents obtained during the somewhat expensive training courses should not be distributed to other professionals.

Likewise, Lohr et al. (1992) published a methodological critique of the EMDR research up to that date. While drawing attention to the same flaws as mentioned by Herbert and Mueser (1992), they also noted that many of the subjects in past research had also been receiving therapies other than EMDR. Further, they argued that the "believability" in the rationale for Shapiro's (1989a) control condition may not have equated with that of the experimental group. Therefore, participants in the experimental and control conditions may have experienced different levels of rationale credibility and treatment expectancy, which may have had a large bearing on the results.

However, single-case studies with liberal methodologies continued to appear in the literature, such as a report by McCann (1992). In this article a 41-year-old survivor of burn injuries was treated in one session with EMDR. However, no measurements were taken at all and instead the author relied on subjective reports, e.g., "At 1 year follow-up, the patient continued to be asymptomatic and continued to gain new skills and new directions in life" (p. 322).

Kleinknecht and Morgan (1992) reported on a case study of a man who had been shot and subsequently treated with EMDR. In this study, some standardized measures were used (the Spielberger trait anxiety measure, the Brief Symptom Inventory, and the Center for Epidemiologic Studies—Depression Scale). However, although pre, post and follow-up data were obtained, no baseline measurements of symptoms, such as reexperiencing the event, were taken. While there are better designs available for the single-case methodologies (see Kazdin, 1982), this study did provide more anecdotal evidence of EMDR efficacy.

It was not until Sanderson and Carpenter (1992) published their study with 58 phobic subjects that EMDR was investigated with an appropriate design. EMDR was compared with image confrontation (IC) in a single-session crossover design. For both conditions, SUD scores to feared stimuli were significantly reduced and maintained up to 1 month follow-up. However, there was no significant difference in treatment efficacy between the two conditions. These authors claimed, therefore, that the eye movements per se were not the medium of change in the EMDR process, and that it was the imaginary exposure to the feared stimuli that was responsible for any improvements. Unfortunately, as with past research, SUD scores were the only method of assessment, which again limits the use of this study in hypothesis testing.

1993

Pellicer (1993) reported an interestingly designed single-case study of a 10-year-old girl with recurrent nightmares. After 1 session of EMDR, Pellicer reported the girl's SUDS to the dream content (snakes) to have been reduced to 0, and that she began to sleep in her own bed (instead of her mother's, as had become the norm). Unfortunately, no measures such as the Child Behavior Checklist or Revised Children's Manifest Anxiety Scale were administered, and it is possible that other factors, such as parental reinforcement and therapist instruction, may have been responsible for the behavioral changes observed.

Boudewyns, Stwertka, Hyer, Albrecht, and Sperr (1993) reported upon a pilot study randomly assigning twenty Vietnam combat veterans with PTSD symptomatology, obtained from a Special Inpatient PTSD Unit program (SIPU), to one of three conditions: EMDR condition ($n = 9$), where subjects received two 90-minute sessions of EMDR; imaginal exposure control condition ($n = 6$), where the subjects received two 90-minute sessions during which they imagined the trauma, but without the eye movements; and a control condition ($n = 5$), where subjects received only the "SIPU milieu treatment." Subjects were assessed at pre- and posttreatment on the Impact of Event Scale, the Mississippi Scale, and the Clinician Administered PTSD Scale. Physiological measures were also obtained at pre- and posttreatment. At both pre- and posttreatment, two sets of physiological recordings were taken—when "at rest" and when presented with a "trauma"-related cue. For the "trauma"-related cue at pretreatment, subjects recounted their traumatic memory of the event and this account was tape-recorded. This tape was then replayed to the subjects while their physiological recordings were taken. At posttreatment, the "trauma"-related physiological measures were obtained while playing the same tape. These measures included heart rate, electromyographic response, skin conductance, and hand temperature. SUD levels were also recorded, and a significantly greater drop in these levels was found for the EMDR condition compared with the exposure condition ($p < .03$). No significant difference in changes over time was found for either EMDR or imaginal exposure on any of the measures relating to PTSD pathology. Although there was a significant change on all the physiological measures between describing the trauma and when at rest, no significant changes in these responses were found following treatment, irrespective of treatment condition.

The authors suggested that the lack of change, as measured by the PTSD questionnaires, may be due to the tendency for the subjects in this population to overreport symptomatology. They noted that the vast majority of the subjects were either receiving or had applied for a disability pension, the maintenance of which was contingent upon continued symptomatology. They also suggested that, as the physiological measures were taken while listening to a tape recording of themselves describing their trauma, any changes in the way they appraised the event, subsequent to treatment, would not have reflected new attributions arrived at due to the intervention. This point is well taken, as the old recordings of the event may still have been semantically tied to their old perspectives of the trauma and its concomitant meaning (Foa & Kozac,

1986; Rachman, 1980), whereas posttreatment beliefs relating to the trauma may need to be triggered by a new, posttherapy recording.

Oswalt, Anderson, Hagstrom, and Berkowitz (1993) conducted one session of EMDR with 5 hospital inpatients with a diagnosis of PTSD and 3 student volunteers who reported an "intrusive traumatic memory" (recruited via an advertisement in a campus newspaper). No standardized measures were administered at intake or posttreatment with the exception of SUD ratings. The results of this study were imparted by eight case vignettes. Five out of the eight cases were "judged" by the authors as unsuccessful and three were judged as successful. Besides the extreme methodological flaws in this study, it is relevant to note that the three "successes" were the three student volunteers rather than the inpatients.

Cocco and Sharpe (1993) extended the case study literature on EMDR to include the treatment of childhood PTSD, using an auditory variant of the procedure. The patient was a 4-year-old boy who had been emotionally and physically abused when intruders entered his house and demanded money from, and physically assaulted, his parents. He complained of many PTSD symptoms and met diagnostic criteria for PTSD. His parents kept a record over a 1-week period of nightmares about the incident, seeking reassurances, sleeping in his parents' bed, telling "the story," wetting the bed, and carrying a toy gun with him on trips "just in case." A high frequency of all these behaviors was found. The Child Behavior Checklist (CBCL) was also administered at pretreatment and he was found to be within the clinical range for the Thought Problems subscale. During the one EMDR treatment session he was instructed to draw a picture of the invaders and another of his favorite hero and was then asked to look at the pictures and remember what happened. The therapist then snapped his fingers on either side of the child's head at the rate of 4 per second for 48 clicks. After two sets of this procedure, during which time the boy demonstrated (with his pen) that the hero had shot the intruders, he reported that "they're dead . . . [and] . . . can't get me." A 3-week follow-up found the boy to be in the normal range on the CBCL Thought Problems subscale and to be asymptomatic on all the behavioral measures. This improvement was maintained at 3-month follow-up. At 6-month follow-up, however, he had returned to preintervention scores on bed-wetting and sleeping with his parents.

Although this case study presents an imaginative intervention for a very difficult problem, the procedure should not be described as EMDR. No eye movements were induced and the use of trauma-relevant drawings may be seen as an intervention in its own right. The technique also has much in common with emotive imagery (e.g., using hero images) as used successfully in the treatment of childhood phobias (e.g., Cornwall, Spence & Schotte, 1996; Jackson & King, 1981; Lazarus & Abramovitz, 1962). However, this was the first case study that suggested a dissipation of gains through follow-up.

Kleinknecht (1993) likewise reported a single-case study of a client with blood and injection phobias treated over four sessions. The client was assessed at pre- and posttreatment and 14-week follow-up with a host of self-report scales relating to phobic anxiety and SUD ratings. She was also assessed both within and between sessions using physiological measures (blood pressure and pulse rate). The results indicated a reduction on all self-report measures and physiological reactivity to the feared stimuli, both imaginary and in vivo. However, because this client was also receiving in vivo exposure during the sessions, the treatment method also involved systematic desensitization and the findings therefore tell us little or nothing about the effectiveness of EMDR per se.

1994

The year 1994 saw an increased push for rigorous research methodologies in the evaluation of EMDR. Forbes, Creamer, and Rycroft (1994) reported a pilot study on EMDR with 8 clients suffering from PTSD. These clients were assessed at intake using the Structured Interview for PTSD to ascertain the presence and severity of PTSD symptomatology, and the Structured Clinical Interview for the *DSM–III–R* to assess for any comorbid diagnoses. Three self-report measures were also utilized to assess treatment outcome, namely the Impact of Events Scale, the Beck Depression Inventory, and the Symptom Checklist–90–Revised. Muscle tension measures (EMG) were taken "to provide a physiological correlate of clinical improvement" (p. 115), as well as SUD levels. The Stanford Hypnotic Clinical Scale was also administered at pretreatment to assess the effect of suggestibility on outcome.

Subjects were treated with four 90-minute EMDR sessions spaced 1 week apart and were assessed 1 week following treatment and at 3-month follow-up. The results suggested that the subjects improved statistically on all measures from pre- to posttreatment and that the improvements were maintained at follow-up. However, despite the apparent improvements, 4 subjects remained sufficiently symptomatic to meet the full criteria for PTSD at both posttreatment and follow-up. Further, at

posttreatment, 4 subjects met criteria for the avoidance symptom cluster of PTSD, 6 for hyperarousal, and 7 for reexperiencing. At follow-up, 4 subjects again met criteria for the avoidance symptom cluster of PTSD and 7 met criteria for both hyperarousal and reexperiencing. SUD levels and EMG recordings displayed marked decreases, although statistical analyses were not possible due to missing data.

Although this research was well documented and used appropriate measures, the subject numbers were small and there was no control group, points acknowledged by the authors. In addition, the participants were drawn from subject pools with different traumatic etiologies, and it would have been interesting to determine any differences in outcome according to stressor type. It should also be noted that suggestibility correlated with improvement from pre- to posttreatment the avoidance symptom cluster of PTSD. Time since trauma also correlated with reductions in hyperarousal from pretreatment to follow-up. However, these covariates were not partialled from the results at the appropriate time points. Nevertheless, this study at least provided direction toward an appropriate research methodology and showed that meaningful quantitative investigations into EMDR were possible.

Jensen (1994) reported a study similar to Boudewyns et al. (1993), whereby 25 Vietnam combat veterans with PTSD were randomly assigned into either an EMDR treatment condition ($n = 13$) or a control condition ($n = 12$) of no extra treatment. These subjects came from an original pool of 74 candidates. Participants were screened out if they showed: "an unstable psychological condition," "questionable motivation for completing the study," "questionable symptomatology," or "an unclear military record." The three pre- and posttreatment (approximately 17 days after assessment) measures were the Structured Interview for PTSD, SUD levels, and VoC scale. Two posttreatment measures were also used: Goal Attainment Scaling (GAS) and the Mississippi Scale for Combat-related PTSD (MPTSD). There were two therapists, both of whom had been trained by Shapiro in EMDR during a 2-day workshop. Both therapists were psychology interns at a VA medical center and the subjects were randomly assigned into the two conditions. The EMDR subjects received one history-taking session and two treatment sessions, whereas the control subjects did not receive the treatment sessions.

The results indicated no differences among the EMDR and control groups at posttreatment on the Structured Interview or the VoC, and there was no improvement within either condition. Likewise, there was no difference between the two groups at posttreatment on either the MPTSD or GAS. However, there was a significant difference on SUD scores at posttreatment between the two conditions, with the EMDR condition showing a greater reduction. Videotaped sessions rated by an expert judge led to the conclusion, by the judge, that the therapists did not spend enough time on the "active treatment phase" of EMDR with the subjects who did not respond to the treatment. However, it must be stressed that the video rater (H. J. Lipke, an EMDR Institute–authorized trainer) would appear to have mixed the process of treatment (i.e., did the therapists include all elements of EMDR and were they proficient with its use) with treatment outcome (i.e., the result from administering a treatment). This point is demonstrated where the rater stated that "the clients may have received enough treatment to open difficult areas, but without enough fidelity to the treatment to resolve these problems" (Jensen, 1994, p. 321). This distinction has been discussed in greater detail elsewhere and interested readers are directed to Rosen (1999).

Surprisingly, in light of the outcome, Lipke commented that the results support the view that the study "could, theoretically, be somewhat supportive of EMDR as a therapeutic modality. However, negative results could not be used to criticize EMDR . . ." (Jensen, 1994, p. 321).

Jensen also stated that the consent form notified control subjects that they were not in the experimental condition, which may have confounded the study. Nonetheless, this design flaw would not explain why no improvements in PTSD symptomatology were found at posttreatment for the EMDR condition. Criticisms of this study by Shapiro (1996) have predominantly been related to the fact that the therapists were interns and not experienced clinicians. Although this point is legitimate, an extension of this argument would mean that about 90% of all psychiatric outcome studies are of dubious value. This would also mean that treatment adherence ratings are of no value if the therapist is still a student—a point with which few would agree.

Acierno, Tremont, Last, and Montgomery (1994) conducted a well-designed and -implemented single-subject multiple-baseline study. A battery of standardized tests were administered at pre- and posttreatment, and physiological measures were also taken. The subject presented with multiple simple phobias. The first two sessions addressed one phobia (dead bodies) and the next four addressed fear of the dark. She was instructed to concentrate on the therapist's stationary finger while imagining the feared stimuli. This yielded no noted im-

provements in goal attainment for either stimulus. Next, the subject received EMDR for six sessions with regard to dead bodies and five for her fear of the dark. This procedure yielded minimal, if any, improvement on any outcome measure. However, the client was subsequently taken through graded in vivo exposure with reinforced practice. A 100% goal attainment was achieved using this approach for both phobias and self-report measures of anxiety, and it was found that negative cognitions decreased considerably. This study suggests that the combination of in vivo exposure and reinforced practice was more effective than EMDR for this patient and that patient resistance to change was unlikely to explain the lack of effect with EMDR.

Vaughan et al. (1994) randomly assigned 36 patients with PTSD (assessed using the SCID) to four sessions of either EMDR, image habituation training (IHT), or applied muscle relaxation, after 17 of the subjects were initially assigned to a 3-week wait list control. All active treatments were superior to the wait list on a variety of measures, but there was no significant difference among the treatment conditions at posttreatment or 3-month follow-up. However, inspection of the means revealed a trend for the EMDR condition to display larger improvements. The authors suggested that the limitations of the study be made clear in that only four treatment sessions were provided and that this may not have been long enough to produce improvement.

Furthermore, they acknowledged that a larger sample would have been desirable to separate out the treatments, as all CBT techniques for PTSD tend to be efficacious to some degree. In addition, they acknowledged that the wait list was not as optimal a control condition as a placebo. However, one must also note that IHT involved subjects listening to a looped-tape recording of their traumas, and not utilizing exposure in vivo and/or active, present tense, first-person, imaginal exposure, as had been used by Foa, Rothbaum, Riggs, and Murdoch (1991) in their study looking into the efficacy of exposure. This later method accesses the full range of affective responses and has been found to be a very potent treatment for PTSD. The Vaughan et al. study could not, therefore, be considered to compare EMDR with a recognized CBT treatment package for PTSD.

Goldstein and Feske (1994) reported on an uncontrolled series of 5 subjects with panic disorder treated with EMDR. These subjects displayed a decrease in panic symptoms on a wide range of psychometric instruments after five sessions. Effect sizes ranged from 0.86 to 1.69 for pre- to posttreatment, although long-term follow-up was not obtained. This study spawned a better controlled investigation (Feske & Goldstein, 1997) discussed later in this article.

One of the major issues in the EMDR procedure, as it currently stands, is whether it is necessary to include eye movements as an element of treatment. However, as of 1994, only one controlled trial had assessed the necessity of eye movements, utilizing a clinical sample with the inclusion of appropriate controls. Renfrey and Spates (1994) reported upon 23 PTSD subjects who received either the EMDR procedure, the EMDR procedure facilitated by a light tracking task, or a variant of the EMDR procedure without the eye movements. The results indicated that both the EMDR condition and the EMDR procedure without the eye movements were equally effective in reducing PTSD symptomatology. This led the authors to suggest that eye movements were not a necessary part of the procedure.

1995

Quantitative studies of treatment outcome using EMDR continued to produce conflicting results. As a consequence, much debate surrounding their methodology occurred. For example, Wilson, Becker, and Tinker (1995) outlined some of the design issues and attempted to rectify these problems with their own treatment-outcome investigation. They administered three 90-minute sessions of EMDR to 80 participants displaying PTSD symptomatology (according to the PTSD–Interview [PTSD–I]). These participants were assigned to either an immediate treatment group or a delayed treatment group. Outcome measures comprised SUD scores, the Impact of Events Scale, the Spielberger State-Trait Anxiety Inventory, and the Symptom Checklist. The treatment condition displayed significant decreases in presenting complaints of anxiety and increases in positive cognitions when compared with the wait list controls, yielding a PTSD composite treatment effect size of 1.82. It was also found that the wait list group, when subsequently treated with EMDR, similarly improved. Treatment efficacy was not mediated by trauma type. However, this investigation did not control for placebo effects or effects that may be nonspecific to EMDR. Furthermore, subjects in the control condition were made aware of their conditional assignment, which constitutes a major confound. The major scale for measuring PTSD symptomatology, the PTSD–I, was also modified to increase its sensitivity over a short time period (1 week), which may have invalidated the measure at posttreatment. Also, the participants were seen as part of the therapists' private practice, which may have introduced ex-

traneous effects into this study. For example, it is not clear whether *all* subjects who received EMDR in these therapists' practices were included in the analyses.

Another problem with this study was that subjects in the EMDR condition were asked to write descriptions of their trauma. Such a treatment approach has been advocated in its own right (Pennebaker, Kiecolt-Glaser, & Glaser, 1988) and, more important, has been shown to be effective within multiple domains of health and personal well-being (e.g., Esterling, L'Abate, Murray, & Pennebaker, 1999; Petrie, Booth, Pennebaker, Davison, & Thomas, 1995). Therefore, it could be argued that this introduced a further potential confound into the EMDR treatment. Because this aspect of the intervention was very brief, however, it is improbable that this aspect of the procedure was responsible for all the improvement.

Wilson, Becker, and Tinker (1997) subsequently published a continuation study that was a 15-month follow-up of the aforementioned participants. They concluded that the treatment gains had been maintained and in some cases built upon. Nevertheless, besides the procedural problems of the initial study, this study was confounded further by increasing estimates of effect size by using only pretreatment standard deviations (which tend to be smaller than pooled standard deviations) and mixing methodologies for obtaining the reliable change index and measure of clinical change. In this analysis Wilson et al. applied a reliable change index to the grouped data as opposed to examining whether individuals reliably improved as initially proposed by Jacobson and Traux (1991). However, Wilson et al.'s research appeared to again suggest long-term gains for EMDR-treated subjects.

Foley and Spates (1995) reported a dismantling design with EMDR, designed to further investigate the necessity of the eye movements, using students who displayed anxiety for public speaking as subjects. These participants received one of four conditions: EMDR, EMDR with a moving audio stimulus to replace the eye movements, EMDR with the eyes kept stationary (focused on the therapist's hand), or a no-treatment control condition. Foley and Spates concluded that eye movements were not necessary for improvement and that a decrease in symptomatology was not uniform across measures. Participants improved in all conditions except the no-treatment condition, suggesting the importance of demand and expectancy effects, the influence of imaginal exposure to anxiety-related stimuli, or both. Although this sample was not drawn from a clinical population, the results support the findings of Sanderson and Carpenter (1992), who used a clinical sample in comparing EMDR with image confrontation and likewise found equivalent improvement in both groups.

1996

Wilson, Silver, Covi, and Foster (1996) investigated the effects of a single session of EMDR on autonomic arousal (galvanic skin response, respiratory rate, fingertip skin temperature, blood pressure, and pulse rate) with 18 subjects apparently suffering from PTSD. Two other conditions were also compared: a time interval control (TIC) and a tapping alternate phalanges (TAP) condition. All subjects in the TIC and TAP conditions who did not report full improvement (SUDs of 0) were then provided with EMDR treatment. Although SUD levels and VoC were collected, no standardized measures were utilized at any time points and diagnoses were never substantiated. The results led the authors to conclude that "the autonomic measures demonstrate the effectiveness of EMDR with respect to single session treatment effects" (p. 226), with EMDR being the only treatment to display a significant improvement in heart rate, skin temperature, systolic blood pressure, and galvanic skin response at posttreatment. However, these results were obtained by multiple *t* tests on each condition and seeing which were significant, without even providing effect sizes for each condition. It should be noted that this is an idiosyncratic method of analysis in that it does not take into account the variance within each condition and, in fact, the standard deviations for the means at pre- and posttreatment were not reported. This is particularly troublesome when one considers, for example, that the mean improvement figure for heart rate in the EMDR group after TIC (4.4), which was significant at $p < .001$, is actually even less than the improvement of TAP (4.8), which was not significant. No clarification on this matter was possible, because the data were unavailable and the first author was unable to shed further light on the issue (D. Wilson, personal communication, 1997). Even without these shortcomings, the results are interesting in that Shapiro (1995) suggested that any form of bilateral stimulation, such as tapping on opposing body parts, may have the same effect as eye movements.

The measurement of physiological indices during treatment was also utilized by Dunn, Schwartz, Hatfield, and Weigele (1996). These authors randomly assigned 28 university students who scored in the severe range on the Impact of Events Scale to either an EMDR treatment session or a similar treatment that mimicked EMDR without the eye movements, yoking subjects in each condition. Subjects in this placebo group were instructed to

keep their eyes fixed on a stationary red dot. Measures included EMG, finger skin temperature, heart rate and galvanic skin response, and SUD ratings. Inter- and intragroup analyses showed that although the subjects in the EMDR condition improved in SUD ratings, there was no significant difference between groups. On the physiological indices the EMDR condition showed improvement in heart rate, whereas the control condition displayed improvements in both heart rate and galvanic skin response. However, there were no intergroup differences on these measures. The authors interpreted these results as casting doubt on eye movements as the necessary component of EMDR when PTSD symptomatology is reduced.

This position was strengthened further by Boudewyns and Hyer (1996), who allocated subjects with combat-related PTSD to between five and eight sessions of one of three conditions: EMDR, an imaginary exposure control (EC), or a no-imagery control procedure (C). Standardized self-report measures, interviewer-based questionnaires, and physiological indices were collected at pre- and posttreatment by an interviewer blind to group assignment. The results revealed the EMDR and EC treatment groups to be effective in reducing symptomatology on most measures, but without an intergroup difference. On the Clinician Administered PTSD Scale, however, the two treatment groups did not differ from the control condition, thereby raising questions regarding the role of imaginal exposure with this subject group. In any case, the results add further weight to the notion that eye movements or bilateral stimulation (Shapiro, 1995) are neither a necessary nor useful addition to imaginal exposure.

This conclusion was further strengthened by Pitman et al. (1996), who utilized a crossover design with 17 Vietnam veterans with chronic PTSD, who received either an eye-fixed or eye-movement condition. This study revealed only moderate improvements in both conditions and again suggested that eye movements may not be the active therapeutic mechanism of EMDR when positive treatment effects are found. In fact, the only statistically significant interaction effects over time were in favor of the *eye-fixed* condition. More recently, a 5-year follow-up of the EMDR treated participants was conducted by Macklin et al. (2000). These authors found that the modest beneficial effects of EMDR on these veterans were lost by follow-up, with a deterioration in symptomatology even from pretreatment. In fact, compared with matched controls who did not receive the intervention 5 years previously, the EMDR treated veterans were more distressed on 3 of the 5 outcome measures. Quite inexplicably, particularly in light of a negative effect size on all other measures, the authors of this study claimed that because EMDR showed a small to moderate positive effect size on 1 subscale of 1 measure (Impact of Event Scale—Avoidance, $d = 0.37$, even though statistically nonsignificant), "the results do not rule out the possibility that EMDR may have produced some lasting reduction in self-reported avoidance symptomatology. Our data also cannot rule out the possibility that more general and lasting benefits might have been obtained with EMDR in less entrenched and chronically ill and/or nonveteran patients" (p. 26).

1997–1998

Following from their 1994 study, Feske and Goldstein (1997) examined the efficacy of EMDR and the necessity of eye movements for patients with panic disorder (predominantly complicated by agoraphobia). Compared with a similar treatment without the eye movements, EMDR was superior on 2 of the 5 primary outcome measures at posttreatment. But by 3-month follow-up this superiority over the eye-fixed condition had dissipated. This line of research added to the body of evidence suggesting that eye movement is not a necessary ingredient to treatment efficacy and that other properties inherent to the EMDR procedure may be responsible for treatment gains. Results such as these led some of EMDR's most ardent supporters to argue that even if the eye movements per se add nothing to the process, EMDR is still a "unique" process and that the procedure is "uniquely impactful" (Hyer & Brandsma, 1997, p. 521), a point of view difficult to reconcile with the available evidence.

The findings of Feske and Goldstein (1997) were strengthened further by the work of Muris and Merckelbach (1995, 1998). A series of single case studies by the authors in 1995 suggested that exposure was the necessary ingredient in the EMDR procedure when objective and standardized instruments were used. This pilot study led to further controlled investigation within this domain when Muris, Merckelbach, Holdrinet, and Sijsenaar (1998) compared EMDR, in vivo exposure, and computerized exposure in 26 arachnophobic children. The children were randomly assigned to one of the three conditions. In the first phase of the study they received 2.5 hours of their assigned treatment and in the second phase of treatment all subjects received 1.5 hours of exposure in vivo. Assessments were carried out at all time points and included standardized measurement instruments and a behavioral avoidance test. Results indicated that exposure in vivo significantly decreased anxiety symptoms

within all domains, whereas the EMDR group improved on only self-reported arachnophobia. The improvement rate of the exposure procedure was also found to be superior to that of EMDR. EMDR did not potentiate the efficacy of a subsequent treatment and, of interest in its own right, computerized exposure did not produce any significant improvement in symptomatology.

Rothbaum (1997) conducted a controlled study of rape victims, comparing EMDR with a wait list control, utilizing standardized outcome measures rated by a blind assessor. She found that, after 3 treatment sessions, EMDR reduced PTSD symptomatology more than the wait list control. However, as this study did not include any therapeutic treatment conditions other than the EMDR, these results could be attributed to variables that are nonspecific to EMDR. Because almost all treatments will out-perform a wait list control (e.g., Carlson, Chemtob, Rusnak, Hedlund, & Muraoka, 1998) this study tells us very little about the relative efficacy of EMDR or its active elements. Furthermore, it should be noted that the follow-up data suggest that those subjects who were faring the worst were the ones most likely to drop out. At follow-up the effect size of EMDR treatment rose to an exceptionally high $d = 4.00$ on some measures, yet the variance at follow-up had become very small due to attrition. A small sample combined with a 50% dropout rate (n=5) yield data of questionable generalization. The attrition rate with this population was further demonstrated when the wait list group was subsequently treated with EMDR and there was again a 50% dropout rate. However, this study served as a pilot study for a larger research project that is still ongoing.

A study conducted by the current author investigating the efficacy of EMDR and the necessity of eye movements in treating war-related PTSD was also published in 1998. Devilly, Spence, and Rapee (1998) randomly assigned 51 war veterans with PTSD to one of three conditions: two sessions of EMDR, an equivalent procedure without the eye movements using a flashing light (Rapid Eye Dilation Desensitization and Reprocessing [REDDR]), or a Standard Psychiatric Support control condition. A battery of standardized assessment instruments was administered at pre- and posttreatment, including an assessment controlling for treatment credibility. A3-month follow-up was conducted by mail to reduce possible demand effects. Psychophysiological assessment was performed by taking blood pressure and heart rate readings when the participants were relaxed and when imagining their trauma pre-, during, and posttreatment.

The results indicated an overall significant main effect of time from pre- to posttreatment, with a reduction in symptomatology for all groups. However, no statistically significant differences were found among the groups. Participants in the two treatment conditions, however, were more likely to display reliable improvement in trauma symptomatology than subjects in the control group. By 6-month follow-up, reductions in symptomatology had dissipated and there were neither statistical nor reliable differences between the two treatment groups. Overall, the results indicated that, with this war veteran population, improvement rates were less than had been previously reported. Also, where improvements were found, eye movements were not implicated as the mechanism of change. Rather, the results suggested that other nonspecific or therapeutic processes account for any beneficial effects of EMDR. A major criticism of this study, however, is that because only one veteran agreed to be videotaped, fidelity ratings could not be procured. Also it should be noted that this population has a more complex presentation with multiple comorbid conditions and may have required longer-term intervention. However, this does not explain Shapiro's (1989a) original single-session results that included "veterans."

Carlson, Chemtob, Rusnak, Hedlund, and Muraoka (1998) returned to comparing EMDR against poorly supported, even inert, comparison treatments by randomly allocating 35 combat veterans with PTSD to either 12 sessions of EMDR, biofeedback-assisted relaxation, or routine clinical care. Unsurprisingly, while all subjects improved, the subjects in the EMDR condition out-performed the subjects in the other two conditions on a number of self-report, psychometric, and standardized interview measures. Treatment gains within each condition were maintained to 3-month follow-up. Carlson et al. also claimed that these results were maintained to 9-month follow-up, as measured by the CAPS. However, at this assessment period only 8 EMDR and 4 biofeedback participants completed the questionnaire. The authors then applied t tests to the data, as opposed to more conservative nonparametric testing usual with such an exceptionally small sample. Such an approach greatly increases the likelihood of a Type I error. Furthermore, these t tests were applied using *one*-tailed significance testing, which also increases the likelihood of finding a significant difference. Nine of these one-sided t tests were applied to the data without any correction, seven of which were described as significant. However, even just applying two-sided tests would have meant that only three would have been significant. It would be interesting to see the number that remained significant if Mann-Whitney U tests had been applied with Bonferroni

corrections. Moreover, the data of one EMDR participant was removed from the analyses due to "serious concerns about the fidelity of responses" (p. 18). One can, therefore, conclude very little from this follow-up. Either way, the large effect sizes on measures of PTSD at posttreatment ($d > 1.1$) and 3-month follow-up ($d > 1.5$) have not been obtained in other studies using the subjects from the same population (e.g., Devilly et al., 1998; Pitman et al., 1996), although whether this difference is due to the length of treatment provided remains unclear. Although this study did use fidelity checks for the EMDR condition, it is surprising that such checks were not also applied to the biofeedback condition.

It should also be stressed that EMDR was the only therapy procedure in this study that included imaginal exposure, an already validated component in the treatment of combat-related PTSD (e.g., Keane et al., 1989). These results, therefore, are again uninstructive regarding the relative efficacy of EMDR compared to an empirically and clinically supported alternative approach. Moreover, the control group used did not control for the theorized active components of the technique. However, this study did provide a complete treatment timeline (12 sessions) and the results are probably more reflective of EMDR's long-term use potency with veterans than Shapiro's (1989a) original, and unreplicated, claim of a one-session cure for any traumatic memory.

A possible confound within Carlson et al.'s (1998) study, however, is that assessment was obtained during "face-to-face" interview. Although the interviewer was blind to subject treatment allocation, demand characteristics are still inherent to this method of data collection. Both Carlson and Chemtob are known and vociferous advocates of EMDR. When results such as theirs appear to be at odds with multiple other research groups working with the same population type, one has to consider the possibility that researcher allegiance and experimenter demand effects may have played a role in the derived outcome (see Devilly, 2001a, for a discussion of this). A postal follow-up may minimize this effect.

Scheck, Schaeffer, and Gillette (1998) likewise compared EMDR with an unvalidated therapy for women under the age of 25 with PTSD. Two sessions of EMDR were compared with two sessions of active listening following random allocation to groups. Unsurprisingly, the results revealed EMDR to be more effective than the active listening group. Although a 3-month follow-up assessment was attempted, the authors noted that this was achieved by reading questions over the telephone. Moreover, 33% of the participants had received additional psychotherapy during the prior 3 months. This study, therefore, provides some support that EMDR is more effective than active listening, yet again raises doubts concerning the long-term durability of EMDR.

In a meta-analysis of PTSD treatments, Van Etten and Taylor (1998) concluded that at posttreatment EMDR was as effective as behavior therapy on self-reported symptoms, but that behavior therapy was more effective on observer-rated symptoms of PTSD. They also concluded that both behavior therapy and EMDR were as effective as selective serotonin reuptake inhibitors (SSRIs) in treating PTSD, and at follow-up there were no differences at all between EMDR and behavior therapy efficacy. The authors also reported that as EMDR used significantly fewer sessions over fewer weeks than behavior therapy, the results of their study "suggest that EMDR is effective for PTSD, and that it is more efficient than other treatments" (p. 140). However, as these conclusions contrast quite sharply with other reviews (particularly a meta-analysis conducted by Davidson and Parker, 2001, discussed later in this review) it is important to clarify Van Etten and Taylor's analytic approach. Rather than analyze between group effect sizes obtained *within each* research study, they aggregated the effect sizes for each treatment condition (e.g., EMDR, behavior therapy, SSRIs) *across all* research studies. Although this allows for a comparison between the relative effect sizes of, say, EMDR and SSRIs, when no individual treatment study had ever directly compared these two treatment modalities, one must bear in mind that the results were obtained under vastly different and uncontrolled research methodologies. In the light of this evidence alone, to then judge one treatment as more efficacious or efficient than another without direct comparison would obviously be a very unusual conclusion.

1999

This year saw a special issue of *The Journal of Anxiety Disorders* specifically devoted to the EMDR debate. It contained four controlled treatment-outcome investigations and various commentaries regarding the procedure. The guest editors of this issue, commenting on the value of invited papers, remarked that they "received both more and less than . . . [they] . . . bargained for" (Acierno & Cahill, 1999, p. 1).

Cusack and Spates (1999) turned to controlling for other aspects of EMDR, continuing with this research group's methodologically rigorous dismantling of the procedure. They treated 27 subclinical trauma clients with up to three sessions of either EMDR or a variant that omitted the overt cognitive aspects (positive cognitions

and the VoC scale were not utilized) during the procedure (termed "EMD" in the study). A battery of standardized outcome measures was administered by independent assessors, blind to the treatment condition of the participants, and fidelity by the therapists to the treatment protocols was also assessed. Results showed an improvement in functioning within both conditions and these improvements were maintained to 2-month follow-up. However, there was found to be no difference between the two conditions on any of the dependent variables and the authors concluded that the cognitive aspects of EMDR appear to be as superfluous to the technique as the eye movements. However, this study still did not compare EMDR to a validated treatment for PTSD, and also screened many individuals out from treatment during the initial assessment due to comorbid diagnoses. Although such subject removal ensures a "pure" sample of PTSD sufferers, one questions the external validity of such an approach. Many long-term PTSD patients also abuse substances and manifest other anxiety and depressive disorders.

Carrigan and Levis (1999) attempted to isolate the effects of the eye-movement component of EMDR in the treatment of public speaking anxiety. Seventy-one female college students who responded in a fearful manner on the Fear Survey Schedule II and on a standardized self-report measure of public speaking anxiety were randomly assigned to one of four groups: imagery plus eye movements or imagery alone, coupled with either a relaxing or negative image. Dependent variables included self-reported and physiological anxiety during exposure and behavioral indices of anxiety while giving a speech. Although process measures indicated that exposure to fear-relevant imagery increased anxiety during the procedure, no significant differences among groups were found on any of the outcome measures, except that subjects who received eye movements were less likely to give a speech posttreatment than subjects who did not receive eye movements. Addition of the eye movements to the experimental procedure did not result in enhancement of fear reduction. These findings lend weight to those of Foley and Spates (1995) and suggest that the positive effects of EMDR may be due largely to exposure. A criticism of these results, however, relates to the procedural element of EMDR as used in this study. A standardized number of eye movement sets (9) and duration of eye movements (15 seconds) were utilized, and this does not readily equate with the necessary and usual tailoring of procedures to individuals during the delivery of therapy. The authors acknowledge this point themselves and respond that "a standardized, rather than ideographic, duration of treatment was chosen because of the necessity of equating treatment across conditions. Although a yoking procedure would accomplish this goal, Church (1964) has presented some cogent arguments against this procedure" (p. 107).

Rogers et al. (1999) treated 12 Vietnam veterans with either one session of EMDR or one session of exposure—based upon the method of Lyons and Keane (1989)—each administered by different therapists. All participants were inpatients undergoing treatment for combat-related PTSD who met criteria for PTSD according to the Clinician Administered PTSD Scale. Comorbid diagnoses of psychosis, dissociative disorder or personality disorder, or a previous history of exposure therapy or EMDR treatment acted as exclusion criteria. Dependent variables were obtained by an assessor blind to condition allocation, and included physiological reactivity (blood pressure and heart rate both when imagining the trauma and when at rest, similar to Devilly et al., 1998), SUD levels, and the Impact of Event Scale (IES). The results displayed a trend for EMDR to produce more improvement at posttreatment on all self-report measures, but there was no difference between groups on the physiological measures. In fact, on subjective ratings, the means showed a deterioration of the exposure group over time. No follow-up data were reported.

Interpretation of these results is difficult indeed due to the limitations of the study: small sample size, concurrent inpatient treatment, an absence of treatment integrity ratings, and the use of only one standardized outcome measure (IES). Moreover, exposure was applied idiosyncratically and within only one domain (imaginal) without an in vivo aspect. Thus, the study does not provide an accurate assessment of the efficacy of exposure therapy, especially since the exposure subjects were significantly worse at intake on self-monitored intrusions and means on all pretreatment measures. In fact, SUD levels and self-rated intrusions for subjects in the exposure condition worsened over time (although not significantly), suggesting that the therapists sensitized rather than desensitized participants. For exposure therapy to work, and to be consistent with the theoretical underpinnings of the approach, between-session habituation is required—not just (or even) within-session habituation (Jaycox et al., 1998). Indeed the authors state that within this one session the participants were first given a rationale for the intervention (an aspect usually taking 60 minutes in itself) and then taught a "quick relaxation method" (an aspect usually taking at least 30 minutes). Furthermore, the 35-minute average exposure time is short of the 50 minutes recommended by other re-

searchers. Consequently, habituation, a process consistently related to successful behavior therapy outcome (Jaycox et al., 1998), was unlikely to have been achieved. Applying a *single* 60- to 90-minute session of this type of therapy as an intervention technique, within either research or general practice, to treat any subject sample raises ethical issues that lie outside the boundary of this paper. However, until this study, treatment adherence emphasis had been placed upon the EMDR procedure. The Rogers et al. (1999) study emphasizes the need for training and adherence to all CBT methods, not just newly developed techniques.

In contrast, Devilly and Spence (1999) compared nine sessions of EMDR against a cognitive behavioral trauma treatment protocol (TTP). TTP was based on and extended from the work of Foa et al. (1991), and was comprised of imaginal and in vivo exposure, stress inoculation techniques, and cognitive therapy. Thirty-one participants (23 treatment completers) with PTSD from a range of traumatic experiences were randomly allocated to the two treatments. These participants were treated by therapists trained in both techniques. Outcome was measured at posttreatment and 2-week and 3-month follow-up using a full range of validated, clinician-administered, and self-report questionnaires that enabled comparison to the past research of both protocols (i.e., the PTSD—Interview, Impact of Events Scale, Civilian Mississippi scale for PTSD, PTSD Symptom Scale—Self-Report, Spielberger State-Trait Anxiety Inventory, Beck Depression Inventory, and Symptom Checklist–90–R), as well as subjective ratings of distress and goal attainment. It should, however, be noted that the follow-up assessments were administered via the postal service to decrease the demand effects inherent in face-to-face assessment during therapeutic research. Sessions were also rated for treatment fidelity by an independent assessor. These ratings were applied to both EMDR and the TTP techniques.

It was found that, compared with EMDR, TTP was both statistically and clinically more effective in reducing pathology related to PTSD and that this superiority was maintained and, in fact, became more evident by the 3-month follow-up. This superiority was evident on *all* assessment measures and also translated into fewer TTP participants meeting criteria for PTSD following treatment. Ratings of treatment distress showed that both approaches were equivalent, although the attrition rate was higher within the EMDR condition. A possible criticism of this study is that the CBT package contained elements of treatment not included in past research (i.e., Foa et al., 1991) rendering its comparability to other studies as limited. However, this research was the first of its kind to compare a full CBT package with EMDR in the treatment of PTSD, yet the results appear to correspond with those of Muris et al. (1998) who compared EMDR with exposure in the treatment of childhood arachnophobia (see above).

Two of the patients treated unsuccessfully with EMDR were later treated with TTP (Devilly, 2001b). The TTP was far more effective with these individuals, suggesting the difference between conditions was not due to differences in patient allocation, although it is possible that the improvement was simply a result of having received additional treatment. However, in light of the results of the larger study, this explanation is unlikely. Devilly (2001b) also outlined the full EMDR fidelity checklist that was utilized in the two studies and which, added to the high fidelity ratings obtained, clarifies the expected claims of procedural irregularity.

This *Journal of Anxiety Disorders* special issue also included two narrative reviews of the EMDR treatment literature. The first, by Cahill, Carrigan, and Frueh (1999), concluded that relative to no treatment, EMDR was superior on measures of distress; relative to nonvalidated treatments, EMDR was at least as effective and frequently superior for trauma cases; except the aforementioned Devilly and Spence (1999) study, there were no investigations comparing EMDR with a validated treatment for trauma; and eye movements are an ineffective addition to the procedure. Likewise, the second review, by Lohr, Lilienfeld, Tolin, and Herbert (1999), evaluated whether EMDR displayed treatment effects over and above existing treatments, whether eye movements were necessary, and whether the beneficial effects of intervention with EMDR are obtained by intervention elements unique to that treatment modality. In all three cases the conclusion was negative. These authors further concluded that the burden of proof that the effects of a novel treatment are not entirely accounted for by nonspecific factors should rest with the founders and proponents of those interventions. These points were similarly echoed in a short review by McNally (1999).

2000–

Goldstein, de-Beurs, Chambless, and Wilson (2000) conducted a randomized, controlled trial in which subjects suffering from panic disorder with agoraphobia (PDA) were placed in a wait list control group ($n = 14$) or administered six 90-minute sessions of either EMDR ($n = 18$) or an attention-placebo intervention (ART; $n = 13$). ART included progressive muscle relaxation training and association therapy (attempting to under-

stand the reason for their problems through free association to memories of past panic attacks), both of which are relatively inert procedures with this population. All subjects were diagnosed using the Structured Clinical Interview for *DSM–IV* Diagnoses and were administered a battery of self-report questionnaires and diary and interview measures. Assessments were taken at pre- and postintervention and again at a 1-month follow-up. All therapists had been trained in EMDR, treatment integrity ratings were obtained, patient expectancy was accounted for (ART elicited higher patient expectancy ratings than EMDR), and attrition rates were examined. Analyzing composite scores, at posttreatment EMDR patients fared better than wait list controls on panic severity and diary measures, but not on the number of panic attacks and the fear related to PDA. There were no group differences found between ART and EMDR at either posttreatment or follow-up. Furthermore, clinical significance measures showed that at posttreatment EMDR-treated patients were significantly worse than a nonclinical sample on measures related to PDA. The authors concluded that as there are extant effective treatments for PDA, EMDR should not be used as a first-line treatment for this population. Unfortunately, this study did not compare EMDR against an independently validated treatment approach (e.g., in vivo exposure) and the treatment integrity raters were not independent of the research team. However, this study did add to the mounting evidence against the specific utility of EMDR for anxiety disorders.

Herbert et al. (2000) examined the efficacy of EMDR and commented on the growth of the EMDR "movement" since its inception. Specifically, they evaluated aspects of the development of EMDR that employed pseudoscientific tactics. Using EMDR as their vehicle, the authors described the many practices common to pseudoscience (e.g., evidence based on anecdotal reports; overuse of auxiliary hypotheses in the face of disconfirming data in a way that precludes a falsifiable theory; and dismissing null results as being due to inadequate application of the EMDR protocol, yet accepting positive results as evidence of EMDR's efficacy) and outlined the implications of these practices for the professional community. In effect, these authors strongly suggested that the EMDR movement has been markedly affected by pseudoscientific processes (particularly issues related to the dissemination of knowledge). Also, and consistent with previous reviews, these authors concluded that eye movements are an inert treatment element, that the benefits of EMDR tend to be limited to verbal report indices, and that the treatment elements of EMDR that produce change are probably nonspecific factors such as imaginal exposure (see Devilly, 2001a, for a more in-depth discussion of this point).

Such conclusions have been recently reaffirmed with the use of a meta-analysis of EMDR studies conducted by Davidson and Parker (2001). Although these authors found that EMDR produced an effect on outcome measures, they did not find an incremental effect over conditions that controlled for eye movements or that utilized exposure as a treatment element. Furthermore, the authors also examined studies that utilized only therapists trained by the EMDR Institute and found that doing so had no discernable effect on their conclusions. It is important to note that this meta-analysis differed from the method used by Van Etten and Taylor (1998) in that Davidson and Parker looked at effect sizes between conditions (e.g., exposure versus EMDR) *within* each individual study and did not aggregate effect sizes *across* studies. This method of analysis better controls for methodological and procedural artifacts that may otherwise unduly affect the conclusions.

Spates's research group has continued to investigate the relative efficacy of EMDR by comparing it with Pennebaker's (1988) writing therapy for the treatment of PTSD (Largo-Marsh and Spates, 1998). Twenty-four subclinical subjects were randomly assigned to either condition and assessed on a range of standardized outcome measures that also included measures of hypnotic susceptibility and treatment expectancy. The results showed both treatments to be effective in reducing symptomatology and subjective disturbance. Hypnotizability and treatment expectancy did not predict outcome. This study showed that when a competently administered alternative therapy is compared with EMDR for participants with PTSD, although not one of proven efficacy comparable to Foa's or Devilly's research group, there appears to be no superiority in favor of EMDR. However, these authors also highlighted the procedural limitations of the Wilson et al. (1995) study, in that writing out a trauma (as used as part of the EMDR protocol in the Wilson et al. study) produced a therapeutic effect. Thus, the inclusion of a writing therapy component to the EMDR procedure undermines the claim that the Wilson et al. (1995, 1997) studies examined the efficacy of EMDR per se.

At a recent conference Thordarson et al. (2001) reported the "first-look" results from their trial comparing EMDR, an exposure-based treatment, and relaxation. In effect, these authors appeared to obtain similar results to Devilly and Spence (1999). They found that while there was a nonsignificant advantage at posttreatment for the exposure-treated subjects, this advantage grew over time

with a lowering of effect size in the EMDR condition. However, due to continued collection of the follow-up data, the posttreatment results had more statistical power and hence the definitive result from their study is not yet available. By follow-up those treated with exposure were significantly better than those treated with EMDR, and sustained remittance was also higher in the exposure-treated group by follow-up. An unexpected feature of this research was that there was also a significant decrease in symptomatology in the relaxation-treated group. Further, there was no evidence that EMDR worked any faster than exposure and, in fact, there appeared to be a trend for exposure to work more rapidly (Dr. Steven J. Taylor, personal communication, 2001). The forthcoming paper detailing the specifics of this trial is eagerly awaited.

Other investigations of the efficacy of EMDR have appeared since 1989 that have attested to its beneficial effects for a whole host of conditions. Reports of successful applications of EMDR have appeared in relation to the amelioration of pain (Hekmat, Groth, & Rogers, 1994); geriatric PTSD (Thomas & Gafner, 1993); dissociative disorders (Lazrove and Fine, 1996; Paulsen, 1995; Young, 1994); PTSD in dementing patients (Hyer, 1995); panic disorder and phobias (de Jong & ten Broeke, 1996); grief (Shapiro & Solomon, 1995); sexual dysfunction (Levin, 1993; Wernik, 1993); pathological gambling (Henry, 1996); chemical dependency (Shapiro, Vogelmann-Sine, & Sine, 1994); complex personality problems (Fensterheim, 1996); athletics (including horse dressage, Foster & Lendl, 1995); and business performance (Foster & Lendl, 1996). However, none of these studies compared EMDR with an otherwise well-established treatment. Instead, most were case studies without appropriate measurement or controls (see Kazdin, 1982). Given that these later studies tell us little about the efficacy of EMDR or the active processes within the procedure, a complete review of these studies is not necessary for evaluating EMDR's efficacy.

EFFECT SIZE IN EMDR RESEARCH

Table 1 presents an analysis of group-based outcome with respect to EMDR. To gauge the impact of this intervention type effect sizes were computed using the following formula:

$$ES = \frac{Mean_{t1} - Mean_{t2}}{\sqrt{(SD_{t1}^2 + SD_{t2}^2)/2}}$$

where SD = standard deviation; t = assessment time point.

Consistent with the preceding qualitative critique, it is clear that early studies tended to use no or few standardized outcome measures, incorporated no fidelity checks, and were contaminated by numerous methodological flaws. With a plethora of methodologically poor treatment-outcome studies littering the journals and frequently being used as evidence either for or against a specific stance, one should perhaps rely upon the research gold standard suggested by Foa and Meadows (1997). These authors suggested that a good treatment-outcome design should include a clearly defined target set (e.g., diagnostic symptom clusters), reliable and valid measures of symptomatology (e.g., Structured Interview for PTSD, PTSD Symptom Scale—Self-Report), blind-assessor administration of the measures, training of these assessors, unbiased allocation to conditions, specific and detailed treatment methods (e.g., provision of a manual), and checks for fidelity to these methods. The use of blind assessors is predominantly advocated to reduce the demand effects and experimenter bias inherent in interviews. However, another means of accomplishing the same goal is to administer questionnaires through the postal system. In an analysis of EMDR efficacy, Lohr et al. (1998) suggested the additional criterion that any treatment evaluation of EMDR should control for the non-specific effects of the treatment.

From Table 1, one can see that out of the possible 27 studies, only 16 met at least 5 of the above criteria based on Foa and Meadows (1997). However, not all of these studies compared EMDR with a tenable alternative therapy, and most compared EMDR with a wait list control or an inert treatment protocol. Of all these 27 studies, none found eye movement superiority on *standardized* measures.

The only study to evaluate the "R" of EMDR (Cusack & Spates, 1999) also found that this feature added little to effectiveness and, in fact, interfered with outcome on some measures. When the "EM" and the "R" have been shown to be ineffective, one is left only with the "D" of desensitization. However, it is possible that the EMDR procedure itself is flawed due to distraction encroaching upon habituation and interfering with extinction of the fear response (Devilly, 2001a), or that the addition of the superfluous "EM" and "R" components interferes with outcome. Therefore, the effect sizes of the EMDR procedure, in isolation of any manipulation, are presented in Table 1, and comparisons with already validated alternative interventions are also provided. In essence, the only studies that have compared EMDR

TABLE 1. A CHRONOLOGICAL APPRECIATION OF STUDIES INTO EMDR

[1] Study	[2] Participants/Sessions	[3] Fidelity (1-3)	[4] Standardized Outcome Measures	[5] Follow-up Assessment	[6] EMDR Effect Size (Estimated *d*) at Post/F-Up	[7] EMDR Superior To Control at Post/F-up?	[8] Eye Movement (Vs Placebo) Beneficial at Post/F-up?	[9] EMDR Status to Empirically Supported Alternative at Post/F-up
Shapiro (1989)	Trauma (G)/1	None	None	3 month	—/—	(Yes/—)	(Yes/—)	—/—
Marquis (1991)	Trauma (G)/Variable	None	None	None	—/—	—/—	—/—	—/—
Sanderson and Carpenter (1992)	Phobia/1	None	None	1 month	—/—	(No/No)	(No/No)	—/—
Boudewyns et al. (1993)	PTSD (V)/2	None	CAPS	None	NA/NA	No/—	No/—	—/—
			IES		NA/NA			
			MPTSD		NA/NA			
Oswalt (1993)	Trauma (G + S)/1	None	None	1 week	—/—	—/—	—/—	—/—
Forbes et al. (1994)	PTSD (V)/4	None	SI-PTSD	3 month	1.19/1.22	—/—	—/—	—/—
			IES		1.51/1.68			
			SCL-90-R (GI)		0.47/0.37			
			BDI		0.58/0.25			
*Jensen (1994)	PTSD (V)/3 (2 treatment, 1 history taking)	Yes (2)	SI-PTSD	None	-0.50/NA	No/—	—/—	—/—
			MPTSD		NA/NA			
			GAS		NA/NA			
*Vaughan et al. (1994)	PTSD (G)/4	None	SI-PTSD	3 month	1.38/1.44	Yes/—	No/—	—/—
			IES		NA/NA			
			HRSD		1.45/1.20			
			STAI-S		1.82/1.17			
			BDI		1.50/0.86			
*Renfrey and Spates (1994)	PTSD (G)/2	None	CAPS	3 month	NA/NA	—/—	No/No	—/—
			IES		NA/NA			
			SCL-90-R (GI)		NA/NA			
Wilson et al. (1995; 1997)	Trauma (G)/3	Yes (9)	IES	15 month	NA/NA	Yes/—	—/—	—/—
			STAI-S		NA/NA			
			STAI-T		0.82/1.01			
			SCL-90-R (GI)		1.12/1.19			
*Foley and Spates (1995)	Public Speaking Anxiety (S)/1-2	Yes (3)	PRCA-24	None	0.86/—[6a]	Yes/—	No/—	—/—
			PRPSA	NA/NA				
			BASA	0.92/—[6a]				
Wilson et al. (1996)	Anxiety (G)/1	None	None	12 month	—/—	—/—	(Yes/—)	—/—
Dunn et al. (1996)	Trauma (S)/1	None	None	None	—/—	—/—	(No/—)	—/—
*Boudewyns and Hyer (1996)	PTSD (V)/5-8	None	CAPS	None	NA/NA	Yes/—	No/—	—/—
			IES		NA/NA			
			POMS-A		NA/NA			
*Pitman et al. (1996)/ Macklin et al. (2000)	PTSD (V)/6 (maximum)	Yes (3)	CAPS	5 year (Macklin et al., 2000)	0.26/-0.82	—/No	No/—	—/—
			IES		0.37/0.19			
			MPTSD		-0.30/-0.64			
			SCL-90-R (GI)		0.15/-0.36			
*Feske and Goldstein (1997)	Panic (G)/6	Yes (3)	ACQ	3 month	0.37/0.67	Yes/—	Mixed/No	—/—
			BSQ		0.61/0.78			
			MI (Alone)		0.55/0.45			
			MI (Accomp)		0.81/0.41			
			BAI		1.12/1.02			
			PAI (C)		0.52/0.39			
			BDI		1.16/0.77			
			BSI		0.97/0.74			
			SAS-SR		1.00/0.74			
*Rothbaum (1997)	PTSD (F + SA)/4 (3 treatment, 1 history taking)	Yes (3)	PSS-SR	3 month (*N*=5; 50% attrition rate)	2.22/2.70	Yes/—	—/—	—/—
			IES		2.64/3.67			
			RAST		1.44/2.10			
			STAI-S		1.45/1.17			
			STAI-T		1.46/1.28			

			BDI		1.80/1.74			
			DES		0.81/1.22			
*Muris et al. (1998)	Phobia (C)/1	None	SPQ	None	0.67/—	—/—	—/—	Negative/—
			SAM		1.55/—			
			BAT		0.41/—			
*Devilly et al. (1998)	PTSD (V)/3 (2 treatment, 1 history taking)	None	MPTSD	3 month	0.37/0.11	No/—	No/No	—/—
			BDI		0.54/0.15			
			STAI-T		0.63/0.33			
Carlson et al. (1998)	PTSD (V)/12	Yes (9)	MPTSD	3 month	1.38/1.59	Yes/—	—/—	—/—
			IES		1.03/1.39			
			STAI-S		1.34/0.88			
			STAI-T		1.57/1.42			
			BDI		1.96/1.35			
*Scheck et al. (1998)	PTSD (F)/2	No	IES	None	1.60/—	—/—	—/—	—/—
			BDI		1.46/—			
			STAI-S		1.39/—			
			PENN		1.31/—			
			TSCS		0.94/—			
*Cusack and Spates (1999)	PTSD (G)/3 (maximum)	Yes (3)	SI-PTSD	2 month	NA/0.93	—/—	—/—	—/—
			IES		NA/NA			
			SCL-90-R (GI)		0.60/NA			
			BASA		NA/1.67			
*Carrigan and Levis (1999)	Public Speaking Anxiety/1	No	TBC	None	NA	—/—	No/—	—/—
			PRCS		NA			
Rogers et al. (1999)	PTSD (V)/1	None	IES	None	0.85/—	—/—	—/—	—/—
*Devilly and Spence (1999)	PTSD (G)/9	Yes (3)	IES	3 month	0.79/0.28	—/—	—/—	Negative/Negative
			PSS-SR		0.75/0.31			
			MPTSD		0.75/0.27			
			SCL-90-R (GI)		0.47/0.00			
			BDI		0.68/0.35			
			STAI-T		0.79/0.28			
*Goldstein et al. (2000)	PDA/6	Yes (9)	ACQ	1 month	-0.06/0.13	2 out of 4/—	—/—	—/—
			BSQ		0.26/0.26			
			BSIQ		0.32/0.56			
			PAI (C)		0.66/0.56			
			PAI (A)		0.37/0.21			
			MI		0.37/0.38			
			BAI		0.88/0.63			
			BDI		0.52/0.62			
			Diary		0.41/0.54			
*Largo-Marsh and Spates (in press)	Trauma (G)/3 (maximum)	None	IES	1 month	NA/NA	—/—	—/—	—/—
			SCL-90-R (GI)		0.51/0.32			
			STAI-S		0.92/0.55			

Note. Where no standardized measures were used, classification has been used on the subjective measures (e.g. SUDs) or physiological measures (e.g. heart rate) that were reported in the original article and are placed here in brackets. "Not tested" represented by vertical lines (e.g. —/—).

[1] Studies with an asterisk (*) next to them fulfill at least 5 out of the 7 criteria for the research "Gold Standard" based on Foa and Meadows (1997).

[2] Anxiety = various disorders within the anxiety spectrum; PTSD = diagnosed with PTSD; Trauma = undiagnosed distress due to a traumatic experience; PDA = panic disorder with agoraphobia; G = general, non-specific population; C = children; F = female only; S = students; SA = sexual assault victims; V = combat veterans. Sessions = number of sessions of EMDR administered.

[3] Fidelity Rating Scale (1 = inappropriate, 2 = rater had some reservations, 3 = appropriate, 9 = method of assessment did not utilize assessors independent of the research).

[4] ACQ = Agoraphobic Cognitions Questionnaire; BAI = Beck Anxiety Inventory; BASA = Observer Scores on the Behavioral Assessment of Speech Anxiety; BAT = Behavioral Avoidance Test; BDI = Beck Depression Inventor; BSI = Brief Symptom Inventory; BSIQ = Body Sensations Interpretation Questionnaire; BSQ = Body Sensations Questionnaire; CAPS = Clinician Administered PTSD Scale; DES = Dissociative Experiences Scale; Diary = Panic Diary; GAS = Goal Attainment Scaling; HRSD = Hamilton Rating Scale for Depression; IES = Impact of Events Scale; MCPTSD = Mississippi Scale for PTSD; MI = Mobility Inventory for Agoraphobia; PAI (C or A) = Panic Appraisal Inventory (Coping or Anticipation); PENN = Penn Inventory for PTSD; POMS-A = Profile Of Mood Scale (Anxiety subscale); PRCA-24 = Personal Report of Communication Anxiety—24; PRCS = Personal Report of Confidence as a Speaker; PRPSA = Personal Report of Public Speaking Anxiety; PSS-SR = Post Traumatic Stress Scale—Self Response; RAST = Rape Aftermath Symptom Test; SAM = Self-Assessment Manikin; SAS-SR = Social Adjustment Scale, Self-Report; SCL-90-R (GI) = Symptom Checklist-90-Revised (Global Severity Index); SCID = Structured Clinical Interview for DSMIII-R or DSM-IV; SI-PTSD = Structured Interview for PTSD; SPQ = Spider Phobia Questionnaire; STAI-(S/T) = Spielberger State-Trait Anxiety Inventory (State/Trait Form); TBC = Timed Behavioral Checklist for Performance Anxiety; TSCS = Tennessee Self-Concept Scale.

[5] Follow-up assessments used for decisions and effect size in this meta-analysis.

[6] Only effect sizes for measures which have been validated are reported here and only for the EMDR condition. NA = not available.

[6a] Effect size provided by authors and obtained by dividing the difference in means by the pre-treatment standard deviation, or the authors did not explain their method of analysis. In light of larger post-treatment standard deviations in nearly all studies it can be assumed that these effect sizes are artificially inflated.

[7] Control = wait list or no extra treatment conditions. This does not include the vast array of unvalidated alternative treatments which have been compared with EMDR.

[8] Studies where the utility of eye movements were assessed found these results.

[9] Negative = independently validated alternative treatment superior to EMDR.

with an already validated and full treatment package have found EMDR to be inferior to that treatment.

As can also be seen, there is a marked disparity in the results obtained for EMDR's efficacy by various research groups. Obvious examples of such a disparity are the studies in which combat veterans were treated using EMDR. The results of Jensen (1994), Boudewyns and Hyer (1996), and Devilly et al. (1998) are commensurate with those of Pitman et al. (1996) and Macklin et al. (2000), yet all these results differ substantially from those obtained by Carlson et al (1998). Such disparities appear to be dependent upon the effects of five main factors, namely: participant status (e.g., veteran vs. generic PTSD); measures used (i.e., effect sizes are larger on measures specific to the subject groups presenting problem); method of data collection (blind or postal assessments appear to obtain smaller effects); method of analyses (e.g., applying parametric testing to nonparametric data can inflate the Type I error rate); and what has been termed researcher allegiance (i.e., university-based researchers who are independent of the EMDR Institute appear to obtain lower effect sizes than those connected to the institute, and/or do not find any superiority of EMDR over other controlled treatments). However, a detailed investigation into these factors is outside the scope of this article.

SUMMARY OF REVIEW

As should be apparent, three major issues have been addressed by previous studies: (1) Are eye movements essential to EMDR? (2) What is the overall efficacy of EMDR? and (3) What is the efficacy of EMDR relative to other treatments?

Eleven out of the 13 dismantling studies assessing the utility of eye movements found no significant benefit to their inclusion in the procedure. The two studies that did find a superiority for eye movements (Shapiro, 1989; Wilson et al., 1996) did not utilize standardized measures, control for therapy credibility/expectancy, investigate treatment fidelity, or maintain the various no-eye- movement groups until follow-up. It appears from Table 1 that there is now reasonably conclusive evidence that the eye movements are not in themselves curative, a conclusion consistent with that of past reviews (e.g., Lohr et al., 1998).

Turning to the treatment of combat veterans, EMDR effect sizes on PTSD specific measures show at worst a large negative effect of -0.82 after a 5-year follow-up (Macklin et al, 2000), and at best a large positive effect of 1.59 after 3 months (Carlson et al., 1998). The Carlson et al. findings are at odds with those of most of the other outcome studies and the previously discussed limitations of this study should be kept in mind. Overall, it appears that a short-term follow-up with this population yields small to moderate effect sizes and that a long-term follow-up reveals a deterioration of symptoms relative to intake.

Research on sexual assault victims suggests a very large effect size of up to $d = 4.00$ by 3-month follow-up. Nevertheless, this research is compromised by a 50% attrition rate that may be largely specific to clients who do not improve. Research on phobia and public-speaking anxiety yields moderate to large effect sizes on problem-specific measures, whereas generic PTSD cases tends to display a variable effect size by follow-up, from minimal (Devilly & Spence, 1999) to very large (Renfrey & Spates, 1994). The method of data collection may have contributed to this difference.

With regard to overall treatment efficacy, EMDR fares very well against wait list or other no-treatment controls, and likewise displays a healthy effect size relative to some other unvalidated (or placebo) treatments (e.g., biofeedback). If, however, the comparative treatment includes *some* aspect of exposure therapy, EMDR confers no obvious advantage. In fact, the one study to compare EMDR with a validated treatment for childhood arachnophobia showed EMDR to be significantly inferior. In the only study that compared EMDR with a validated approach for adult PTSD, EMDR was again found to be both statistically and clinically inferior.

In other words, the above analysis suggests that some treatment is better than no treatment; a treatment including exposure principles as a component is superior to an inert or poorly delivered treatment; and a theoretically consistent and delivered treatment based on exposure principles outperforms EMDR.

THE "EVOLUTION" OF EMDR: MICROMUTATION OR PREEMPTIVE SALTATION?

Shapiro (1996) noted that more controlled studies have been reported on EMDR than for all other interventions for PTSD put together, and offered a review of this research. She suggested that "external stimuli," such as alternating hand taps and auditory tones, may be as useful as eye movements. Unfortunately, this review omitted many of the studies that reported a poor outcome for EMDR as well as those in which no relative efficacy was evident for EMDR over controls using nonlateralized stimuli (e.g., Sanderson & Carpenter, 1992; Foley & Spates, 1995). In this report, she also suggested standards

for the evaluation of methodologies. In contrast, Foa and Meadows (1997) proposed their own standards for the evaluation of treatments. After a review of the evidence, Foa and Meadows reached different conclusions from those of Shapiro with respect to the efficacy of EMDR, stating that "the test of the efficacy of this much-discussed treatment awaits adequately controlled studies" (p. 470). Similarly, Keane (1998) was less convinced than was Shapiro regarding the efficacy of EMDR.

However, of importance to the current discussion is the development of EMDR over time. Good theories and treatment models evolve over time. However, such theories explicitly state the conditions under which they could be disconfirmed. Changes over time to the assumptions and procedures should also be made explicit and differentiated from earlier versions to preclude confusion. Failure to meet these criteria results in practices based upon unfalsifiable theories and general scientific disarray. It is, therefore, important to put EMDR to these tests if it is to be viewed as a serious scientific proposal.

In 1989 Shapiro claimed that eye movements were the necessary (although not sufficient) ingredient of the procedure, and it was not until 1994 that this claim was adequately investigated and 1998 that a general consensus appeared to be reached.

Research assessing the veracity of claims typically necessitates the acquisition of grant money, approval by ethics committees, acquisition of participants, the trials themselves, and the attainment of follow-up data before the analysis is undertaken and the research submitted for publication. However, even then the review process itself, and a backlog of already accepted articles, can periodically add an extra 2 years onto the time the findings take to reach the light of published day. With this in mind, many researchers spread word of their studies before the articles appear in print.

However, only 2 years after her initial report, Shapiro (1991a) claimed that other forms of alternating directional attention might be as useful as the eye movements, asserting that "another potentially fruitful area of investigation may be the use of other stimuli such as hand- or finger-tapping during sessions" (p. 2). The first reference to research suggesting the effectiveness of stimuli other than eye movements was published by Cocco and Sharpe (1993). These authors used auditory cues (finger snapping on either side of the head) instead of eye movements in a case study to treat a small boy with PTSD following a robbery, as already described. When Renfrey and Spates (1994) published their study on the utility of eye movements and found that there was little, the groundwork of "other stimuli" had already been laid and the results then appeared unremarkable. By 1996 this alternating directional attention had progressed to any "external stimuli," thereby discounting all research investigating the role of eye movements.

On another aspect of the process, Shapiro claimed in 1989 that the description of EMDR in her published paper was enough "to achieve complete desensitization of 75–80% of any individually treated trauma-related memory in a single 50-minute session" (p. 221), and in a footnote she asked readers who were interested in a full description of the procedure to contact the author. Readers who did so received a brochure of available training seminars. By 1991(b), Shapiro claimed that "while successful treatment without proper training may be achieved perhaps 50% of the time, in other cases, untrained clinicians place the client at risk" (p. 188) and insisted that all researchers be trained by the EMDR Institute. By 1992 EMDR had developed into a two-stage learning process, whereby trainees were presented with Level I and Level II certificates that required two separate training sessions. By 1994 Shapiro disqualified research that did not support EMDR's efficacy on the grounds of a lack of Level II training (e.g., Jensen, 1994), followed by disqualification due to the lack of fidelity checks (Shapiro, 1999). These issues were discussed by Rosen (1999), who pointed out that there was no evidence to support these claims and that the training level argument and inflated importance placed on treatment fidelity is a "specious, distracting issue that permits the continued promotion of EMDR in the face or negative empirical findings" (p. 173). As described earlier, the meta-analysis of Davidson and Parker (2001) supported Rosen's contentions that whether therapists had been trained by the EMDR Institute had no effect on the results.

The original 1-session cure (Shapiro, 1989) likewise mutated slowly over time to the eventual claim that 5 sessions for the general population and 12 sessions for veterans were necessary (Shapiro, 1999). This claim was made despite the fact that both of these subject populations had been included in the original 1989 study.

Of further concern, EMDR has recently necessitated the use of "positive future templating" (Shapiro, 1995). During this procedure the patient is encouraged to think of future situations likely to arise and, via therapist modeling, develop a healthy coping response set. It is difficult to ascertain the difference between this procedure and guided self-imagery and mastery, procedures long established as beneficial within the cognitive behavioral paradigm (e.g., Kirsch, Frankel, & Valone, 1977; Surman, 1979).

The "DNA" of EMDR appears a fickle construct with yet further mutation occurring over a short period of

time to encompass different protocols for dissociation, grief, phobias, and a whole host of differing presentations (see De Jongh, Ten Broeke, & Renssen, 1999; EMDR Web site, 1999; Shapiro, 1995, 1997). By 1999 classical EMDR had been shown to be less effective than a cognitive behavioral treatment package for PTSD (Devilly & Spence, 1999). However, EMDR now appears to encompass preparation for and initiating in vivo exposure. The following components were advanced as necessary to the EMDR protocol and were used for fidelity ratings as part of the EMDR procedure: "8 = therapist helped client to incorporate a detailed template for fear-free future action; 9 = therapist and client arranged contract for in vivo action; 10 = client runs mental videotape for full sequence in vivo action and reprocess disturbance" (Shapiro, 1999, p. 55). EMDR appears to be gradually evolving towards Keane et al.'s (1989) and Foa et al.'s (1991) exposure protocols. Due to the ever-changing nature of what counts as EMDR, it is not surprising that a coherent theoretical basis for this technique has not been seriously proposed. Throughout its evolution, EMDR has maintained its acronym, offered no new theories adding to and consistent with already understood phenomena, has become less distinct from other therapeutic interventions, and has not stated terms under which the procedure's tenets could be falsified.

When one looks back across time to the genesis of EMDR, one is faced with the many incarnations of the treatment. In the world of paleontology, such mutations of the fossil record would cause widespread alarm. With this problem in mind, until testable (and hence falsifiable) propositions are offered with regard to what constitutes the effective components of EMDR, further research in this area is of questionable value.

CONCLUSIONS

To date it has become increasingly clear that (1) EMDR is more effective than no treatment; (2) eye movements per se do not contribute to therapeutic effectiveness; (3) the reprocessing ("R") component of EMDR may be relatively inert; (4) full treatment packages utilizing competently administered exposure techniques are more effective than EMDR in the treatment of anxiety disorders; and (5) being trained by the EMDR Institute has no significant effect on treatment effectiveness. Due to the insidious mutation of the EMDR procedure over time, it is doubtful that debates regarding its utility can ever be settled within the scientific paradigm until falsifiable propositions are advanced by its proponents.

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