

Unanswered Questions about the Empirical Support for EMDR in the Treatment of PTSD: A review of research

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A literature review was conducted to examine whether EMDR should be considered an empirically-supported treatment for PTSD. Relying largely but not exclusively on electronic data bases such as Medline and PsychInfo, journal articles published through April 2003 were identified which reported a randomized experimental evaluation of the effectiveness of EMDR in treating PTSD. EMDR appears to be an empirically supported treatment for adults with single-trauma civilian PTSD. However, the evidence supporting the effectiveness of EMDR is much less compelling when we focus on children, combat PTSD, multiple-trauma PTSD, and whether EMDR is more effective than exposure therapies. Proponents of EMDR hotly debate proponents of exposure therapy regarding methodological issues, with each side in the debate frequently employing a double standard. Clinicians are advised to use either EMDR, exposure therapy, or stress-inoculation therapy when treating civilian adults with single-trauma PTSD. They may also want to employ EMDR when treating children with PTSD or clients with multiple-trauma or chronic PTSD. But if they do, they should do so in light of the inadequate evidence base, be guided by future evaluations of EMDR with these populations, and recognize that many more sessions of EMDR, with less robust effects, may be required than what they might currently expect.

Eye Movement Desensitization and Reprocessing (EMDR) is a cognitive-behavioral therapy that was developed in 1987 to desensitize clients to distressing memories, feelings and cognitions and to replace negative cognitions with positive ones (Chemtob, C. M., Tolin, D. F., van der Kolk, B. A., & Pitman, R. K. (2000). The key component in EMDR is the use of bilateral stimulation. The therapist typically stimulates rapid back-and-forth eye movements or alternates right and left hand taps or sounds in the right and left ear while the client visualizes the distressful scene and keeps in mind the related cognitions and feelings. A full description of the eight-phase EMDR protocol can be found in a text written by Shapiro (1995), EMDR's founder.

More than 30,000 mental health practitioners have been trained in EMDR since Shapiro's (1989) first article on EMDR was published. During the early 1990's EMDR was depicted by the popular media and some early EMDR disciples as perhaps a miracle cure for a wide range of problems. The dramatic results of first study on EMDR (Shapiro, 1989) impressed some as implying that one session of EMDR led to the long-lasting resolution of the distress associated with traumatic memories connected to PTSD (Herbert et al., 2000). In her first text on EMDR, Shapiro (1995) suggested that EMDR clinicians could expect success rates as high as 80-90

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percent for some clients. In the subtitle of her subsequent text Shapiro (1997) called EMDR a “breakthrough therapy.” An article in *Newsweek* (Cowley & Biddle, 1994) cited an EMDR proponent who called it “the most significant advance since the introduction of pharmacological drugs” (p. 70). An article in *New York Magazine* (Stone, 1994) depicted EMDR as possibly a “miracle cure.” Acknowledging the hype about EMDR that was emerging in the popular media, Shapiro (1994) cautioned EMDR proponents not to make “miracle” claims when discussing the effects of EMDR with their local media.

According to Spector and Read (1999), published claims have been made touting the success of EMDR in treating virtually every area of psychopathology. These include such problems as eating disorders, phobias, test anxiety, work performance problems, conduct disorders, personality disorders, panic disorder and physical pain (Bauman & Melnyk, 1994; Carrigan & Levis, 1999; Feske & Goldstein, 1997; Gosselin & Mathews, 1995; Kleinknecht, 1993; Hudson et al., 1996; Maxfield & Melnyk, 2000; Spector & Read, 1999; Stevens & Florell, 1997). Perhaps the most extreme claim was made by Botkin (2000), whose study concluded that EMDR helps “bereaved individuals experience what they believe is actual spiritual contact with the deceased” (p.181). In light of the above claims, it is easy to be skeptical about EMDR. Nevertheless, if we limit our focus to the effectiveness of EMDR in treating posttraumatic stress disorder (PTSD), per se, there is a strong foundation for considering it to be an empirically-supported treatment.

More controlled studies have been done supporting the effectiveness of EMDR with PTSD than has been the case for any other psychotherapy for PTSD (Spector & Read, 1999). Division 12 (Clinical Psychology) of the American Psychological Association has deemed EMDR as one of three empirically validated treatment approaches (along with exposure therapy and stress-inoculation therapy) that are probably efficacious in treating PTSD (Chambless et al., 1998). EMDR was designated in the treatment guidelines of the International Society for Traumatic Stress Studies as an effective treatment for PTSD (Chemtob, Tolin, van der Kolk & Pitman, 2000). Two recent meta-analyses have supported the efficacy of EMDR in treating single-trauma PTSD (Van Etten & Taylor, 1998; Davidson & Parker, 2001). Research using brain scans has found that neurobiological problems in brain asymmetry among traumatized individuals appeared to be corrected after EMDR treatment (van der Kolk et al., 1997).

Despite the foregoing support, EMDR’s effectiveness in treating PTSD continues to be debated by two polarized camps, one consisting of scholars who have gained recognition as proponents of EMDR, and the other consisting of scholars who have gained recognition either as critics of EMDR or for their work in alternative cognitive-behavioral therapies that rely on traditional exposure techniques for treating PTSD. With vested interests at stake in both camps, the debate has at times resorted to personal attacks. Some critics portray EMDR as merely a placebo with a gimmick and dismiss the research supporting it as mere pseudoscience (Herbert et al., 2000). Counter arguments to this criticism have been made by EMDR proponents (Shapiro, 2002; Perkins & Rouanzoin, 2002; Lipke, 2000).

In light of the extensive number of studies and the meta-analyses supporting EMDR’s

effectiveness in treating PTSD, it is tempting to dismiss its critics on grounds of vested interests and turf protection. However, a recent review of the empirical literature on the effectiveness of EMDR with PTSD raised some questions about its effectiveness. This article reports the results of that review.

Literature Review Method

The literature review was conducted incrementally over an ten-year period, beginning in 1993, when the author first received training in EMDR and began to conduct research on it. As a member of the EMDR International Association, he received updates from the Association and the EMDR Institute identifying each newly published study on EMDR as well as bibliographies of all previous EMDR research that had been conducted. He also subscribed to a listserv of the association, which sent E-mails to subscribers informing them of new studies and various methodological debates. In May 2001 he began updating his library of EMDR studies using electronic data bases including MEDLINE (FirstSearch) and PsycINFO (Ovid). By using key words associated with PTSD and EMDR, he identified and read journal articles published in English through April 2003 which reported a randomized experimental evaluation of the effectiveness of EMDR in treating PTSD. He also searched the reference list of each article looking for additional experiments. Excluded from the review were unpublished reports, dissertations and conference presentations.

The review provided little basis for questioning the meta-analytic and other review studies mentioned above regarding the effectiveness of EMDR in treating adults with single-trauma civilian PTSD. However, the review also found that the evidence supporting the effectiveness of EMDR is much less compelling when we focus on children, combat PTSD, multiple-trauma PTSD, and whether EMDR is more effective than exposure therapies. Let's begin with the evidence regarding EMDR's effectiveness with children.

Is EMDR effective with children who have PTSD?

EMDR has been recommended for treating a wide array of emotional and behavioral problems among children, including children as young as two years old and perhaps younger (Tinker & Wilson, 1999). In one case study, Pellicer (1993) reported that one session of EMDR ended a 10-year-old girl's nightmares. Greenwald (1994) reported five case studies in which EMDR alleviated trauma symptoms connected to experiencing a hurricane. In a quasi-experiment, Puffer, Greenwald, and Elrod (1998) concluded that a single session of EMDR reduced trauma symptoms among children and adolescents who had experienced a single trauma or loss. However, very few randomized experiments have been conducted on the effectiveness of EMDR with children.

Although some randomized experiments (such as the Shapiro, 1989 seminal experiment on EMDR) have included some children along with adults as subjects, only four randomized experiments have assessed EMDR's effectiveness with a sample comprised exclusively of children. These four experiments, as well as the remaining outcome studies to be discussed in this paper, are summarized in Figure 1.

Chemtob et al. (2002) used a randomized lagged-groups design and concluded that EMDR was effective in alleviating PTSD symptoms (using standardized measures) with 32 elementary school children (aged 6 to 12) who had experienced a hurricane. Most of the children met the criteria for disaster-related PTSD. The Chemtob et al. conclusions were based on pretest to posttest improvements that occurred for two groups receiving EMDR at different times.

Two experiments by Muris and his associates compared EMDR with an exposure in vivo treatment for reducing avoidance behaviors among spider-phobic children. In one, Muris et al. (1997) found positive effects for EMDR, but superior effects for the exposure treatment. They concluded that EMDR adds nothing of value to the exposure treatment. In the other experiment, the exposure treatment produced significant improvement on behavioral as well as self-report measures, but EMDR produced significant improvement only on self-reported spider fear (Muris et al., 1998).

In another randomized experiment, Rubin et al. (2001) found no significant differences on the parent version of the Achenbach Child Behavior Checklist between children treated with EMDR plus routine treatment and children who received routine treatment, only. The 39 children in the study were clients in a child guidance center. Although many had been traumatized, they had mixed diagnoses, and many had problems not connected to a single trauma. The very young ages or clinical problems of some required improvisational deviations from the standard EMDR protocol.

Among the above four randomized experiments, the only one that focused exclusively on PTSD symptoms obtained results supporting the efficacy of EMDR. (The other three are included in this review, even though they did not focus on PTSD per se, because only four experiments have assessed EMDR's effectiveness with a sample comprised exclusively of children.) It is therefore tempting to disregard the other three studies, two of which focused on phobias and one in which diagnoses were mixed. But only one study with positive outcomes does not yet provide sufficient grounds for concluding that EMDR is an empirically-supported treatment for children with PTSD. While that study indeed provides grounds for optimism about the efficacy of EMDR with children experiencing PTSD symptoms, more experimental studies with children are needed, including some dealing with different forms of PTSD.

Is EMDR effective with military veterans with combat-related PTSD?

Shortly before or coinciding with the advent of EMDR, interventions for treating combat PTSD that received the most empirical assessment were exposure therapies (Boudewyns & Hyer, 1990; Cooper & Clum, 1989; Keane, Fairbank, Caddell, & Zimering, 1989; Peniston, 1986; Keane, Fairbank, Caddell, & Zimmering, 1989; Fairbank, Gross, & Keane, 1983). Although the number of experimental evaluations on exposure therapies was not extensive, the studies consistently found that those therapies had modest to moderate effect sizes in alleviating symptoms of combat PTSD. Despite those results, concerns remained about drawbacks of those therapies, such as frequent relapses (Jensen, 1994), the intensity and duration of anxiety

generated (Jensen, 1994; Wolpe & Abrams, 1991; Fairbank & Brown, 1987), and insufficient attention to irrational cognitions or the development of generalizable coping skills (Jensen, 1994; Shapiro, 1989). The dramatic results of Shapiro's (1989) seminal study regarding the effectiveness of EMDR, although it did not focus on combat PTSD per se, kindled hope that her new treatment approach might overcome some of the drawbacks and improve upon the effectiveness of other therapies for combat PTSD. Also, because EMDR does not involve continued re-exposure to fear stimuli and is thought to provoke less anxiety than other exposure-based therapies, some investigators found it to be better tolerated by combat veterans (Pitman et al., 1996).

Seven randomized experimental evaluations of the effectiveness of EMDR in treating combat PTSD were found. Two quasi experiments were also found. One is included in this review because it was a follow-up study of one of the randomized experiments. The other quasi-experiment is included because it is often cited by EMDR proponents.

The first randomized experimental evaluation of the effectiveness of EMDR in treating combat PTSD was conducted by Boudewyns et al. (1993). Compared to subjects receiving an exposure control condition and those receiving a milieu-only control condition, EMDR subjects had better outcomes on self-reports of distress and on (non-blind) clinician ratings. However, standardized psychological measures and physiological assessments failed to support the superior effectiveness of EMDR. The Boudewyns et al. study had only 18 subjects divided among three conditions, and provided only two sessions of EMDR treatment. Questions emerged, therefore, as to whether a larger sample size or a lengthier treatment dosage would yield significant EMDR effects.

Boudewyns and Hyer (1996) reported preliminary results from a study that built upon the earlier Boudewyns et al. study by adding more subjects and providing a lengthier treatment dosage. The later study provided five to seven treatment sessions and randomly divided 61 veterans with PTSD among three conditions: 1) EMDR; 2) imaginal exposure therapy; and 3) standard group therapy for PTSD. EMDR subjects had better outcomes than standard group therapy subjects on two of four standardized outcome measures, but did no better than exposure therapy subjects on any outcome measure.

Another early randomized experimental evaluation of the effectiveness of EMDR in treating combat PTSD was conducted by Jensen (1994). Jensen randomly assigned 25 subjects to either an EMDR (N=13) or a control (N=12) condition in which control subjects were merely given a list of alternative mental health sites where they could seek treatment. EMDR subjects received one intake session and two treatment sessions. Although the EMDR recipients self-reported greater reductions than controls in in-session subjective anxiety, none of the other outcome measures used in this study (including a standardized measure of PTSD symptoms) supported the effectiveness of EMDR. Again, however, questions can be raised as to sample size issues as well as treatment dosage. In addition, there is disagreement as to whether there was adequate treatment fidelity in Jensen's study. Various reviewers have suggested that Jensen did not have adequate fidelity (Shapiro, 1996; Greenwald, 1996; Feske, 1998; Cahill et al., 1999;

Spector & Read, 1999). One criticism is that Jensen's therapists were psychology interns who had received Level I EMDR training, only, not Level II training. Rosen (1999) countered that at the time Jensen conducted his study only Level I training was required for an official certificate of completion of EMDR training. Rosen therefore argued that Jensen did have adequate fidelity and that by changing the rules as to what qualifies as fidelity to the EMDR treatment protocol EMDR proponents make EMDR a moving target and thus "are not playing fair with science" (p. 182). Lipke (2000), however, identified other serious flaws in the way the Jensen study implemented EMDR treatment, particularly the premature termination of treatment sessions. In light of the potential flaws in the treatment fidelity of Jensen's study, there is disagreement as to whether to consider its findings to represent a negative outcome for EMDR.

A quasi-experimental evaluation of the effectiveness of EMDR in treating combat-related PTSD was conducted by Silver et al. (1995). This study's findings provided more support for the effectiveness of EMDR than the earlier studies; however, any inferences drawn from its findings must be tempered in light of its methodological limitations. In particular, group assignment of subjects was not random, and the findings were based on evaluations that were not blind (standardized measures were not used) and multiple t-test comparisons that inflated the Type I error probability (Lohr, Lilienfeld, Tolin, & Herbert, 1999; Lohr, Kleinknecht, Tolin, & Barrett, 1995).

A fifth evaluation of the effectiveness of EMDR in treating combat PTSD was conducted by Pitman et al. (1996). Seventeen Vietnam combat veterans with chronic PTSD were assigned randomly to either a condition where they received up to six sessions of EMDR or a condition where they received all aspects of EMDR except the eye movements. Standardized outcome measures showed modest to moderate improvement for both groups of subjects; however, the slightly better improvement in the comparison condition suggested that factors other than eye movements accounted for EMDR's therapeutic effects. As with the previous studies with null results, this one too had low statistical power.

Macklin et al. (2000) reported a five-year follow-up study of 13 of the 17 veterans who participated in the Pitman et al. study. In the follow-up study, subjects who participated in either group in the original study were combined into one group in the data analysis and compared to 14 Vietnam combat veterans with chronic PTSD who did not participate in the original study (and thus were not assigned randomly). Macklin et al. found that the modest to moderate improvements observed in the original study were lost at follow-up and that PTSD symptoms worsened over the five-year period across all 27 treated and untreated subjects. Macklin et al. justified combining the two groups from the original study on grounds that the comparison condition in that study substituted finger tapping for eye movements and that finger taps are now considered one acceptable alternative to eye movements in the EMDR protocol for bilateral stimulation. However, in their original study, the subject tapped his own fingers and did so *not* in a bilateral fashion. The finger taps in the EMDR protocol are to be done by the therapist, who alternates taps on the client's left and right hands in a bilateral fashion.

In the seventh study on the effectiveness of EMDR in treating combat PTSD, Devilly, Spence and Rapee (1998) randomly assigned 51 war veterans with chronic PTSD to one of three

conditions: 1) two sessions of EMDR; 2) the EMDR protocol without the bilateral eye movements, using instead eye dilation with a stationary, flashing light; or 3) no intervention control. Although all three groups had a significant pre-to-post improvement in symptoms, no statistically significant differences between the three conditions were found on any of the outcome measures. However, when the analysis combined the EMDR group with the EMDR minus eye movements group, the combined group's outcome was significantly better than the outcome for the no intervention control group.

Despite concerns about dosage adequacy in most of the previous seven studies (with most studies with null findings providing no more than two sessions of EMDR treatment), Rogers et al. (1999) conducted a randomized experimental evaluation of the effectiveness of a single session of EMDR treatment for Vietnam veterans undergoing inpatient treatment for combat PTSD. Subjects who received one session of EMDR treatment were compared to those who received one session of an alternative form of exposure treatment, involving an implosion procedure. On standardized outcome measures both groups showed significant pre-to-post improvements in intrusive symptoms, but there was no significant difference between the groups on these symptoms, and no improvement was found in avoidance symptoms for either group.

Most of the findings in the eight studies reviewed above raise doubts about whether EMDR is more effective than traditional exposure therapies in treating combat PTSD. However, in all but two of the studies with such findings no more than two sessions of EMDR treatment were provided, and in the two exceptions the treatment dosage was only five to seven sessions. Related to the issue of an insufficient number of sessions is the need to target multiple combat-related traumas in treatment, rather than just one.

Noting that the number of treatment sessions in studies of traditional exposure-based therapies ranges from 9 to 48, Carlson et al. (1998) postulated that more EMDR sessions are required to resolve chronic symptoms in light of the chronic and severe nature of combat PTSD and the need to target multiple traumas. They tested the efficacy of a 12-session dosage of EMDR in a randomized experiment with 35 veterans with combat PTSD. The subjects were assigned randomly to either 12 sessions of EMDR, 12 sessions of biofeedback-assisted relaxation, or routine clinical care. Significant treatment effects favoring the EMDR condition were found on self-ratings and (non-blind) interviewer ratings of PTSD symptoms and on standardized psychometric tests of PTSD symptoms and depression. A standardized psychometric measure of anxiety found an insignificant group x treatment interaction on state anxiety and a near significant ($p < .06$) group x treatment interaction (with a large effect size) on trait anxiety. A three-month follow-up found that these effects were generally maintained. On physiological measures, however, none of the findings showed significant outcome differences between groups. Carlson et al. noted that previous studies supporting the effectiveness of other exposure therapies with combat veterans by and large failed to find differences on physiological measures, and that the evidence supporting these therapies has generally been limited to measures of cognitions and behaviors. Carlson et al. also called attention to their large effect sizes, and compared them to the generally modest effect sizes found in studies of other treatment

approaches for combat PTSD. In light of that, and since 12 treatment sessions is more in keeping than 2 sessions with traditional short-term therapy approaches and with previous exposure studies for combat PTSD, they called for further studies on the amount of treatment that may be required to overcome the symptoms associated with severe and chronic PTSD.

Is EMDR effective in treating civilians who have experienced multiple traumas?

As noted above, one reason postulated by EMDR proponents for why EMDR did not appear to be more effective than traditional exposure therapies in treating combat PTSD is the chronic and severe nature of that form of PTSD, due in part perhaps to veterans with combat PTSD having experienced multiple traumas in battle. EMDR proponents argue that multiply traumatized individuals with chronic forms of PTSD require many more EMDR treatment sessions than do individuals who have experienced only a single trauma (Shapiro, 1999). The findings of the Carlson et al. (1998) study discussed above support that contention. However, one study is not a sufficient basis for concluding that the provision of additional sessions makes EMDR more effective than traditional exposure therapies in treating individuals who have experienced multiple traumas. Additional studies are needed, especially since four other randomized experiments – albeit with fewer EMDR treatment sessions – had findings that questioned the superior effectiveness of EMDR in treating combat PTSD.

But what about civilians who have experienced multiple traumas and who therefore may suffer from chronic forms of PTSD? No randomized experiments were found that tested the efficacy of EMDR with such subjects exclusively. Colosetti & Thyer (2000) conducted a single-case evaluation design study with five women prisoners who had PTSD and histories of being battered. In most cases their PTSD appeared to be chronic and the result of multiple traumas. The EMDR treatment was provided by an experienced clinician with advanced EMDR training, who was supervised by a clinical psychologist who had been providing EMDR therapy for six years. The EMDR treatment phase was compared to a placebo phase in an ABC design. Outcome was measured using two standardized instruments with excellent psychometric properties. The multi-phase design was replicated across five subjects, offering increased control for history. Colosetti and Thyer concluded that EMDR is no more effective than a credible placebo treatment in ameliorating the effects of chronic abuse. Only one of the five women in the Colosetti and Thyer study received more than three sessions of EMDR treatment. EMDR proponents might argue that three sessions are insufficient for treating victims of multiple traumas with chronic PTSD.

Much more research on this issue is needed. For now, all we can infer is that: 1) EMDR appears to be as effective as traditional exposure therapies in treating combat related PTSD; 2) we lack sufficient evidence for concluding that EMDR is more effective than traditional exposure therapies in treating combat PTSD; and 3) questions remain about the efficacy of EMDR in treating other chronic forms of PTSD among individuals who have experienced multiple traumas. And if the argument that many more EMDR sessions are required to produce significant results among such individuals is eventually supported with future research, it is worth noting the contrast between that potential reality and the initial hopes that EMDR would be a historic breakthrough that produced dramatic alleviation of trauma symptoms with only a

few sessions or less. Even the studies dealing only with single traumas are finding on average only medium effect sizes for EMDR (Davidson & Parker, 2001) that contrast with the extremely large effect-sizes in Shapiro's (1989) seminal study, effect sizes that – along with other early writings on EMDR – stimulated so much optimism about a therapy that seemed to many to offer an almost magical cure for PTSD as well as a variety of other disorders that might somehow be related to traumas.

Is EMDR simply a variant of exposure therapy with an unnecessary component?

Some of EMDR's harshest critics argue that the benefits of the EMDR procedure are due exclusively to its incorporation of certain aspects of exposure therapy and that the eye movement component of EMDR is just an unnecessary gimmick that aids in the marketing of EMDR as something new (Herbert et al., 2000; Lohr et al., 1998). Some of the randomized experiments on this issue have been conducted in relation to combat PTSD, and thus were mentioned above.

In addition to the combat PTSD studies, seven randomized experiments on this issue conducted with civilians who have PTSD were found. The results of those studies are uneven, with two finding EMDR to be more effective than a comparison exposure therapy, two with findings that indicated little or no difference in outcome between the two treatment approaches; two with mixed results, and one concluding that a particular exposure treatment was more effective than EMDR. First let's look at the two studies whose results indicated that EMDR was more effective.

Wilson et al. (1996) randomly assigned 18 clients to one of three conditions: 1) normal EMDR; 2) EMDR with alternate thumb taps instead of eye movements; and 3) EMDR with eyes static. Results were better for the normal EMDR group on physiological measures and on the two subjective SUDS and VOC measures regularly used as part of the EMDR treatment protocol. The latter two measures are considered to be highly vulnerable to experimenter demand bias, and the physiological measurement procedures used in this study have been criticized (Lohr et al., 1998). In addition, the clients had mixed diagnoses, no standardized measures of psychological functioning were used, and the question has been raised of possible bias resulting from the clinicians also being the researchers (Spector & Read, 1999; Cahill et al., 1999). (Of course, the studies that have questioned the effectiveness of EMDR have been criticized as well, as noted throughout this paper.)

Lee et al. (2002) compared 12 participants who received EMDR to 12 who received Stress Inoculation Training with Prolonged Exposure (SITPE). Outcome was measured on a variety of measures, including some based on self-report and some on non-blind clinician ratings. The various data analyses that were conducted compared outcome in regard to global PTSD, depression, and subscale scores measuring intrusion and avoidance symptoms. At posttest the only significant difference between EMDR and SITPE was on the measure of intrusion symptoms. Comparison of three-month follow-up scores, however, found significant differences favoring EMDR on all but the two avoidance measures.

Next we'll look at the two studies that, in this author's view, had mixed results, although the authors of those studies – as well as some EMDR proponents (e.g., Lee et al., 2002) -- depicted their results as supportive of EMDR's effectiveness. Ironson et al. (2002) randomly assigned 22 clients who consisted primarily of victims of rape or other crimes to EMDR treatment or a prolonged exposure treatment. Their results suggested that both treatments “are equally effective at reducing symptoms of PTSD and depression” (p. 123). However, the reduction in symptoms appeared to be faster in the EMDR group, and that group also had a significantly lower treatment dropout rate.

Vaughn et al. (1994) randomly assigned 36 clients to one of three treatment conditions: 1) EMDR; 2) exposure treatment involving image habituation training; and 3) applied muscle relaxation. Although clients in all three treatment conditions improved more on standardized measures than did those on a waiting list, the three treatments were found to have equal effects. On one measure (of intrusive thoughts), the EMDR group did significantly better than the applied muscle relaxation group at posttest but not at follow-up.

In the study that found EMDR to be less effective than an exposure therapy, Devilly and Spence (1999) assigned 23 clients to EMDR treatment or a cognitive-behavior therapy called Trauma Treatment Protocol (TTP), which consisted primarily of exposure procedures. Each group received up to eight sessions of treatment provided by two therapists who were trained in both procedures. One of the therapists treated 8 EMDR clients and 12 TTP clients. The other therapist treated 3 EMDR clients, only. Although one may wonder about the implications of this regarding possible therapist bias, treatment fidelity was checked carefully and found to be good. Results on a variety of standardized measures consistently indicated that TTP was significantly more effective than EMDR at posttest as well as in a 3-month follow-up. Devilly and Spence also found a higher drop-out rate for the EMDR group and therefore questioned the view (often touted by EMDR proponents) that EMDR is more client-friendly than exposure therapies. Maxfield and Hyer (2002), however, countered that the higher EMDR drop out rate represents a methodological flaw in the Devilly and Spence study.

In one of the two studies with null findings, Renfrey and Spates (1994) randomly assigned 23 subjects to three conditions: 1) normal EMDR treatment; 2) EMDR with eye movements induced by lights; and 3) the EMDR procedure with static visual attention instead of eye movements. Although all three groups showed desired treatment effects, no significant differences in outcome were found between treatment conditions. This study, though well controlled for the most part, has been criticized for its dubious EMDR treatment fidelity, its low statistical power and its lack of blind evaluations (Spector and Read, 1999).

By far, eye movements have been the most frequently tested EMDR component in the dismantling studies. In the other study with null findings, Cusack and Spates (1999) evaluated the effects of a different EMDR component – the cognitive component. The cognitive component that they tested involved “exposure to aversive trauma-related negative cognitions” and “rehearsal of adaptive positive cognitions” (p. 89). To see if the cognitive component in the

EMDR protocol is necessary for its effectiveness, they randomly assigned 27 clients to either standard EMDR treatment or a modified EMD treatment that followed the EMDR protocol in all respects except the cognitive reprocessing (R) component. Outcome was measured using standardized instruments administered by blinded independent assessors. Although both groups made significant pretest to posttest improvements on each outcome measure, there were no differences between groups. While these results do not question the efficacy of EMDR, they do question the necessity of its cognitive component. Cusack and Spates interpret their findings as implying “that at its core, EMDR consists of an imaginal exposure treatment” (p. 94). Cusack and Spates nonetheless suggest that EMDR is a unique imaginal exposure treatment that holds promise for reducing PTSD symptoms in a relatively brief time span. Yet they also note that a consistent pattern has emerged in that two of the EMDR components (eye movements and cognitive reprocessing) deemed essential by EMDR advocates have failed to make a unique contribution to outcome. Skeptics may therefore see the Cusack and Spates findings as additional evidence for questioning the degree to which EMDR really constitutes something dramatically more than a variant of imaginal exposure treatment.

Some have attempted to refute this skepticism by arguing that the use of exposure in EMDR is quite different than the way it is used in other exposure therapies (Rogers & Silver, 2002; Shapiro, 1999). For example, in EMDR clients are exposed to the disturbing image for brief and interrupted periods, whereas the exposure is prolonged and uninterrupted in traditional exposure therapies. In contrast with flooding, EMDR’s application of exposure is in sequentially applied small doses, “with only a relatively small amount of clients’ attention directed at the most unpleasant part of the memory and no deliberate exacerbation of their distress by concentrating on details of the traumatic experience” (Shapiro, 1999, p.48). In Shapiro’s view, according to theories guiding the use of traditional exposure therapies, the type of exposure present in the EMDR protocol would be contraindicated.

However, even if we do not call EMDR an exposure therapy, we do not yet appear to have sufficient evidence for deeming EMDR more effective than alternative therapies using exposure techniques. In their meta-analysis, Davidson and Parker (2001) conclude, “In sum, EMDR appears to be no more effective than other exposure techniques, and evidence suggests that the eye movements integral to the treatment, and to its name, are unnecessary” (p. 305). More research on this issue is needed.

Methodological Issues

No study supporting or questioning the effectiveness of EMDR has been flawless. Among some of the earliest studies supporting EMDR’s efficacy were flaws in measurement, internal validity, data analysis and interpretation of findings. However, there appear to have been enough relatively rigorous experiments using unbiased measures supporting its effectiveness to consider – for the time being -- EMDR to be an empirically-supported treatment for treating civilian adults with single trauma, non-combat PTSD – at least until future rigorous studies emerge to question its efficacy.

The most heated controversy pertains to studies questioning whether EMDR is really different from exposure therapy or merely the same thing with a gimmicky new package more likely to induce placebo effects. This controversy often focuses on the necessity of the bilateral stimulation component of EMDR and studies that found exposure therapies to be at least as effective as EMDR in the treatment of PTSD. One issue involves the incorporation into the EMDR protocol of the option to use alternative forms of bilateral stimulation other than eye movements. Thus, after some studies found that alternatives such as right and left hand taps worked just as well as bilateral eye movements, the EMDR procedure was modified to allow clinicians to use these alternatives instead of eye movements. EMDR proponents argued that those studies therefore don't undermine the empirical base of EMDR. EMDR critics countered that such changes make EMDR a moving target that becomes impossible to refute (Rosen, 1999). Shapiro (2002) counters this criticism -- made by advocates of traditional exposure therapies -- by pointing out that those therapies too have undergone analogous modifications to their initial protocols from study to study on their efficacy.

Another commonly cited issue regarding studies with results questioning the effectiveness of EMDR pertains to whether some studies used EMDR therapists who were inadequately trained in EMDR and/or who implemented the EMDR procedure improperly. For example, as discussed earlier in this paper, the therapists in the Jensen (1994) study were psychology interns who had no experience in treating clients with EMDR prior to the clinical trial. Likewise, they had received Level I EMDR training, but not Level II. Shapiro criticized the Jensen study because the therapists had not received Level II training. But Rosen (1999) pointed out that at the time Jensen conducted his study only Level I training was required for an official certificate of completion of EMDR training. In their review of the EMDR research literature, Cahill et al. (1999) found no association between outcome and level of EMDR training. Likewise, in their meta-analysis, Davidson and Parker (2001) found that the provision of EMDR only by therapists trained by the EMDR Institute did not change conclusions about the effectiveness of EMDR.

In contrast, in their review of 12 controlled randomized experiments evaluating the efficacy of EMDR with PTSD, Maxfield and Hyer (2002) found a significant correlation between effect size and ratings of treatment fidelity. Greater fidelity was associated with greater EMDR efficacy. They did not look at the correlation of effect size and level of EMDR training per se. Maxfield and Hyer also found a significant correlation between effect size and ratings of methodological rigor, in which greater rigor was associated with greater EMDR efficacy. The extent to which the Maxfield and Hyer ratings of methodology and fidelity might have been biased, however, is unclear. Maxfield and Hyer hypothesized that more training and methodological rigor would be correlated with larger effect sizes supporting EMDR efficacy, and Maxfield (who is an EMDR Institute Facilitator and frequent collaborator with Shapiro) was one of the raters. Her ratings conceivably could have been influenced by her awareness of the effect sizes and her own hypotheses. To offset that bias, two additional raters were used, both of whom were blind to the study's hypotheses. Inter-rater reliability was not reported. "Differences between raters were resolved by consensus and/or by assigning the lower rating" (p.33). Thus,

although the two additional raters were blind to the study's hypothesis, we cannot be sure that the ratings of rigor and fidelity were not biased. For example, if the two blind raters gave the studies with low EMDR effect sizes higher rigor ratings than did Maxfield, Maxfield's lower ratings would have been used. Also, Maxfield's predilections could have influenced the process of reaching "consensus." In addition, if the two blind raters themselves strongly believed in EMDR's efficacy, they could be biased without knowing the study's hypotheses. Knowing whether a particular study's results did or did not support EMDR's efficacy may have been enough to bias the way they rated that study's fidelity or rigor. Although the most recent EMDR experiments appear to be making great efforts to ensure treatment fidelity, this issue may continue to be debated in the near future. (The foregoing comments are not meant to trash the Maxfield & Hyer study, which has many strengths. They merely raise some questions about it, just as Maxfield and Hyer raised questions about the treatment fidelity and methodological rigor of many published studies whose results indicated relatively low effect sizes for EMDR.)

Those in the EMDR camp as well as those in the anti-EMDR camp sometimes react to studies with results that don't support their camp's point of view by attempting to discredit those studies because they are not perfect. Studies with tiny sample sizes and low statistical power that produce results supporting the effectiveness of EMDR are commonly cited by EMDR proponents to argue that EMDR is effective. But when studies with findings that EMDR proponents do not like have used similarly small samples, their findings may be dismissed on grounds of statistical power. It may indeed be the case that inadequate power is the reason for the insignificant findings, but to accept desired findings from studies with low power (and perhaps other methodological problems) while dismissing undesired findings from studies with similar levels of power is to stack the deck in favor of one's predilections and vested interests.

Some EMDR proponents may counter that the foregoing comments on statistical power are inappropriate, correctly noting that if a study with low power still manages to find significant effects for EMDR, then the lack of power is no longer relevant because a Type II error is not being risked. Moreover, they may argue that relatively large effect sizes are often needed to obtain significant findings in studies with low power. Nevertheless, dismissing studies with null findings due to their low statistical power, while accepting as methodologically adequate studies with comparably low power but with findings supporting the effectiveness of EMDR, smacks of a double standard. We cannot be certain that the null findings represent a Type II error. Moreover, when studies with low statistical power obtain null findings that fail to replicate the significant findings of other studies with low power, the lack of replication calls attention to the possibility that the significant findings may have resulted from a Type I error, measurement bias, or other methodological limitations. Low statistical power is a methodological weakness that can lead to incorrectly failing to reject the null hypothesis. However, other sorts of methodological weaknesses – such as experimental demand or measurement bias -- can lead to incorrectly rejecting the null hypothesis regardless of statistical power. The point here is that most studies evaluating the effectiveness of EMDR – even some of the relatively stronger ones – have had some important methodological imperfections. To not question those imperfections when the results support our predilections, while dismissing the results of studies with analogous imperfections whose results question our predilections, is to be using a double standard that falls short of good science.

A recent example of such a double standard seems to appear in the way some EMDR proponents have criticized the Devilly and Spence (1999) study. For example, Lee et al. (2002) and Maxfield & Hyer (2002) fault the way Devilly and Spence assigned participants to EMDR versus TTP. Devilly and Spence randomly assigned the entire block of the first ten participants to one treatment condition (which turned out to be TTP), with the following ten then going to the other condition (EMDR). The remaining participants were assigned alternatively to the two conditions. They also criticize the Devilly and Spence study for not using multiple therapists in both conditions and for deviating from the standard EMDR treatment protocol by rating and repeating the negative cognition during treatment and by not targeting future and anticipated distressing material. Lee et al. further criticize Devilly and Spence for using the same fidelity rater for both treatments and for not indicating whether the rater had sufficient advanced qualifications in EMDR.

With regard to the latter point, Lee et al. cite Greenwald (1996) to claim that standard training in EMDR is not enough to ensure treatment fidelity. They also cite a claim by the EMDR International Association (1999) that “an additional period of supervised practice is required for a *basic* level of competence” (p.1084, italics added). One wonders why this caveat has not been emphasized in the many flyers and advertisements marketing EMDR trainings that have succeeded in getting over 30,000 mental health practitioners to spend almost \$400 (not counting travel and lodging) in registration fees for *each* of two weekend EMDR trainings, with the latter being called *advanced*, and with trainees receiving certificates for framing.

But I digress; let's return to the question of double standards. Maxfield and Hyer (2002) included a list of “strengths” in their review of the Lee et al. study, without using any terms such as “limitations” or “weaknesses.” Yet when reviewing the Devilly and Spence study, they provided a list of “limitations” and no mention of terms such as “strengths.” Among their list of limitations, they criticized Devilly and Spence for not having an independent blind assessor. However, Devilly and Spence had symptom ratings done by blind evaluators at posttest, whereas most of the ratings for one of the two statistically significant symptom findings at posttest in the Lee et al. study were done by the treating practitioner!

Likewise, Maxfield and Hyer criticized Devilly and Spence for conducting a large number of statistical analyses with no Bonferroni correction for an inflated Type I error. Yet when reviewing the Lee et al. study Maxfield and Hyer omitted mentioning that Lee et al. did the very same thing in their many data analyses!

In connection to data analysis, it is interesting that Lee et al. noted that the interview schedule they used for the clinician ratings assessed *various* PTSD symptoms; however, when focusing on specific symptoms after the difference at posttest on the global ratings were not significant, they analyzed only two symptoms: intrusion and avoidance. No explanation was provided as to what other symptom comparisons could have been analyzed or reported and why they were not. Likewise, Lee et al. acknowledged that the Impact of Events Scale (which they

used in addition to the clinician ratings) assesses avoidance, numbing and intrusion symptoms, yet they reported only intrusion and avoidance symptoms in their analysis (with significant differences on intrusion symptoms, only). Why did they omit numbing symptoms? Why was there no mention of this in the Maxfield and Hyer review?

Whereas Devilly and Spence were criticized for not using multiple therapists in both conditions (they used one therapist to administer EMDR and TTP to 20 participants, and a second therapist to administer EMDR to 3 participants), Lee and his second author treated 21 of their 24 participants (11 SITPE, 10 EMDR), with a third therapist treating the remaining three participants. One of the first two therapists in the Lee et al. study became an EMDR trainer during their study, suggesting the possibility of a bias and vested interest favoring the EMDR condition. Although both of the Lee et al. co-authors/therapists were trained and experienced in exposure therapy before undergoing EMDR training, and the suggestion of potential therapist bias in the Devilly and Spence study is reasonable, is the Lee et al. study really significantly less vulnerable to the same sort of criticism?

Whereas Devilly and Spence were criticized for deviating from the standard EMDR treatment protocol by rating and repeating the negative cognition during treatment and by not targeting future and anticipated distressing material, Lee et al. reduced the SITPE condition in their study from nine sessions to seven and acknowledge that this might have put the SITPE condition at a disadvantage. This is particularly noteworthy in light of the criticism leveled by EMDR proponents that various studies with results not favoring EMDR provided too few EMDR sessions.

Readers are urged not to misconstrue the foregoing comments regarding double standards as suggesting that the Lee et al. study was methodologically weak. I think that both it and the Devilly and Spence study were relatively strong methodologically, but -- like all psychotherapy outcome studies -- not perfect. It is easy for well trained researchers to find imperfections in any study. Neither should readers misinterpret my remarks to imply that only those in the EMDR camp use a double standard. My point is simply that authors with vested interests in EMDR as well as authors with vested interests in exposure therapies frequently use a double standard when praising the studies that support their interests and finding fault with those studies that do not. In my illustration of this double standard I have focused on the above three articles in response to a reviewer's criticism that an earlier version of this article overvalued the Devilly and Spence study and should rely more on the Maxfield and Hyer and the Lee et al. articles.

Vested interests may also play a role in tilting the meta-analysis playing field in favor of studies that support the effectiveness of EMDR being more likely to be submitted for publication. After reading a draft of my study (Rubin et al., 2001) containing primarily null findings regarding EMDR's effectiveness with children, several of my colleagues with strong vested interests in EMDR informally urged me to refocus my report on the process aspects of the study, delete or downplay its outcome findings, or delay its submission for publication altogether until other studies appeared in the literature that supported the effectiveness of EMDR with children. I have encountered other EMDR colleagues who agonize over what to do when they

obtain null findings that they didn't expect and do not like. One worried about falling out of favor with the EMDR establishment. It is not beyond some to reanalyze the findings repeatedly, fishing for some way to slice the data that produces an outcome that casts a better light on the possible effectiveness of EMDR. How many studies with null results, one wonders, have gone unreported by such colleagues? Of course, this concern applies to meta-analyses of any intervention, not just EMDR.

In concluding this discussion of methodological issues, one issue, discussed above, merits revisiting. It concerns the limited number of EMDR treatment sessions used in most of the studies involving clients with combat PTSD or other multiple traumas. Another issue in these studies pertains to how outcome is measured. If only one memory of a single trauma is treated in a small number of sessions, and outcome measures pertain to distress or cognitions regarding only that specific memory, then EMDR might appear to be effective even with multiply traumatized clients. But when only one of many traumatic memories is treated, and outcome is assessed with global measures of PTSD or related global constructs, then EMDR proponents argue that it is unreasonable to expect positive outcomes following treatment of a single memory when many other trauma memories remain untreated. In light of these issues, Shapiro (1999) recommends limiting research reviews on the effectiveness of EMDR to single-trauma PTSD. Whether we agree with Shapiro on that point or not, we can surmise that by taking that position Shapiro is implicitly acknowledging that we do not yet have an adequate empirical base to call EMDR an empirically validated treatment for combat PTSD or for multiple-trauma PTSD. This does not imply that we have sufficient evidence for deeming EMDR to be ineffective with multiply traumatized clients – just that we need more studies with heavier EMDR treatment dosages before we can reach either conclusion.

Conclusion and Applications to Practice

More controlled studies have been done supporting the effectiveness of EMDR with non-combat, single-trauma PTSD than has been the case for any other psychotherapy for PTSD. Even most of the studies of EMDR with combat PTSD or multiple trauma civilian PTSD have suggested that EMDR is effective; they just failed to show that it is *more* effective than alternative exposure therapies. Consequently, despite the questions that emerged, the results of this review do not disagree with the conclusions of Division 12 (Clinical Psychology) of the American Psychological Association, the International Society for Traumatic Stress Studies, or two recent meta-analyses – all of which supported the notion that EMDR is an empirically-supported treatment for treating PTSD.

But the lack of disagreement applies primarily to civilian adults with single traumas. And it does not mean that EMDR is more effective than traditional exposure therapies in treating combat PTSD or multiple-trauma PTSD. The review does *not* imply that EMDR is ineffective with children or with adults with multiple traumas – just that we do not yet have enough support for calling it an empirically-supported treatment with them. We need more research on those populations, just as we need more research comparing EMDR to exposure therapies and more research on the effects of specific components in the EMDR protocol. It is conceivable that the

EMDR advocates are correct in insisting that EMDR is more client-friendly than traditional exposure therapies and produces more rapid effects that are also more clinically significant. But it is also conceivable that we will find that it does not and that it may even be less effective. Future studies should also examine client and practitioner characteristics that are associated with outcome so that we can attempt to ascertain the conditions under which EMDR might be more effective or less effective than exposure therapies (Ironson, 2002).

While we await additional research, practitioners are advised to use either EMDR or traditional exposure therapies when treating adults with single-trauma PTSD. Both appear to qualify as empirically supported treatments. They may also want to employ EMDR when treating children with PTSD or clients with multiple-trauma or chronic PTSD. But if they do, they should do so in light of the inadequate evidence base. They should vigilantly keep abreast of the emerging research on EMDR with these populations and should recognize that many more sessions of EMDR may be required than with single-trauma adult clients and that the effects of this treatment might be less robust than what they expect. And finally, they – and we all – should remember that in the scientific method all knowledge is provisional and subject to refutation. No matter how many prestigious professional organizations proclaim that EMDR is an empirically-supported treatment for PTSD, the consensus of experts – even scientific experts -- does not qualify as a scientific basis for ending inquiry on the effectiveness – or possible ineffectiveness - of any intervention.

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FIGURE 1: Features of Reviewed Studies

Question	Citation	Target problem/ Diagnosis	N	Design	Treatment Issues	Methodology Issues	EMDR Effectiveness?
Is EMDR effective with children?	Chemtob, et al.,2002	Children aged 6-12 with hurricane related PTSD	32	Randomized lagged-groups design examining pre to post change among two groups of EMDR recipients	3 EMDR treatment sessions with apparently good fidelity	Data analysis controlled for passage of time, but not history	Significant pre to post improvement, found for both EMDR groups on a variety of rigorously administered standardized psychological measures
	Muris et al., 1997	Children with spider phobia	26	Randomized, comparing EMDR with exposure treatment	Questionable fidelity of EMDR treatment, which was truncated by not treating memory of first distressful spider encounter. 1 EMDR session, provided by therapist who may not have been adequately trained	Possible therapist confound since different therapists were used for each group	Yes, but less effective than alternative exposure treatment as measured by behavioral observation and self-reported spider fear
	Muris et al., 1998	Children with spider phobia	22	Randomized, comparing EMDR with exposure treatment	Questionable fidelity of EMDR treatment, which was truncated by not treating memory of first distressful spider encounter	Possible therapist confound since different therapists were used for each group	Only on self-report measure of spider fear, and less effective than alternative exposure treatment. Not effective as measured by behavioral observation.
	Rubin et al., 2001	Children aged 6-14 with various diagnoses in a child guidance center	39	Randomized, comparing EMDR plus routine treatment with routine treatment, only	Improvised deviations, as recommended in EMDR literature; EMDR treatment fidelity checked and found to be good; At least 5 EMDR sessions	Low statistical power	No, as measured by parent report on Achenbach Child Behavior Checklist

FIGURE 1: Features of Reviewed Studies (con't)

Question	Citation	Target problem/ Diagnosis	N	Design	Treatment Issues	Methodology Issues	EMDR Effectiveness?
Is EMDR effective with military veterans with combat PTSD?	Boudewyns et al., 1993	Combat PTSD	18	Randomized, comparing EMDR to: 1) exposure; and 2) milieu-only	Only 2 sessions of EMDR treatment; debatable treatment fidelity	Low statistical power, clinician ratings not blind	EMDR had better outcome on subjective self-reports & clinician ratings, but not on standardized psychological measures and physiological assessments
	Jensen, 1994	Combat PTSD	25	Randomized, comparing EMDR group to controls who received a list of alternative mental health treatment sites	Were therapists adequately trained? Did they end EMDR treatment sessions prematurely? Only 2 EMDR treatment sessions.	Low statistical power	No on all standardized and subjective measures except for subjective self reports of in-session anxiety
	Silver et al., 1995	Combat PTSD	100	Quasi-experimental; not randomly assigned, comparing EMDR to relaxation and biofeedback	Good EMDR treatment fidelity, but only 1+ sessions	Not randomized, inflated alpha due to multiple t-test comparisons	Yes on non-blind evaluations; not more effective on standardized psychological or physiological measures used
	Boudewyns & Hyer, 1996	Combat PTSD	61	Randomized, comparing EMDR to: 1) imaginal exposure therapy; and 2) standard group therapy	5 to 7 sessions of EMDR treatment, with good fidelity		EMDR and imaginal exposure equally effective compared to group therapy on two of four standardized psychological measures and physiological measures
	Pitman et al., 1996	Combat PTSD	17	Randomized, comparing EMDR to EMDR components with stationary finger taps instead of bilateral stimulation	6 sessions of EMDR treatment with good fidelity	Low statistical power	Modest to moderate pre-post improvement in both groups on most standardized psychological and physiological measures, but comparison condition improved more.
	Devilley et al., 1998	Combat PTSD	51	Randomized, comparing EMDR to: 1) EMDR components without bilateral stimulation; and 2) no intervention control	Only 2 sessions of EMDR treatment		Significant pre/post improvement on standardized psychological measures and subjective measure (SUDS) for all 3 groups; no between-group differences. But the 2 intervention groups combined did better than the no-intervention controls

FIGURE 1: Features of Reviewed Studies (con't)

Question	Citation	Target problem/ Diagnosis	N	Design	Treatment Issues	Methodology Issues	EMDR Effectiveness?
	Rogers et al., 1999	Combat PTSD - Inpatients	12	Randomized, comparing EMDR to exposure treatment using implosion	Only 1 session of EMDR treatment	Low statistical power	Both groups improved in intrusive symptoms, but no between-group difference, and no improvement for either group in avoidance symptoms. Standardized measures used for both.
	Macklin et al., 2000	Combat PTSD	27	Non-randomized five-year follow-up of subjects in the Pitman et al. study above.	6 sessions of EMDR treatment with good fidelity	Combined both groups from Pitman et al. study and compared to non-randomized controls.	No: Pitman et al. improvements lost at follow up, and PTSD symptoms worsened for both groups.
	Carlson et al., 1998	Combat PTSD	35	Randomized, comparing EMDR to: 1) biofeedback-assisted relaxation; and 2) routine care	12 sessions of EMDR treatment with good fidelity	Interviewer ratings were not blind	Yes, generally with large effect sizes on almost all measures except physiological measures. Other measures included self-ratings and interviewer ratings of PTSD symptoms and standardized tests of PTSD symptoms, anxiety and depression.
Is EMDR effective in treating adult civilians who have experienced multiple traumas?	Colosetti & Thyer, 2000	Women prisoners whose PTSD is apparently chronic	5	Single-case evaluation replicated across 5 subjects, using an ABC design	Only one woman received more than 3 EMDR sessions	Not a group experiment, but single-case methodology employed rigorously	No, as measured by standardized self-report instruments

FIGURE 1: Features of Reviewed Studies (con't)

Question	Citation	Target problem/ Diagnosis	N	Design	Treatment Issues	Methodology Issues	EMDR Effectiveness?
Is EMDR simply a variant of exposure therapy with an unnecessary component? (Also see above combat PTSD studies)	Renfrey & Spates, 1994	PTSD (non-combat)	23	Randomized, comparing EMDR to: 1) EMDR with eye movements induced by lights; and 2) EMDR components with static visual attention instead of eye movements	Dubious fidelity of EMDR treatment – up to 6 sessions	Low statistical power; outcome ratings not blind	No differences between treatment conditions, but desired improvement found for all three groups on SUDS, physiological measures and standardized psychological measures
	Vaughn et al., 1994	PTSD (non-combat)	36	Randomized, comparing EMDR to: 1) exposure therapy; 2) relaxation; and 3) wait list control	4 EMDR sessions with apparently good treatment fidelity	Some subjects did not meet criteria for PTSD; Low statistical power	All three treatment conditions equally effective compared to wait list on standardized psychological measures. On one measure (of intrusive thoughts), the EMDR group did significantly better than the applied muscle relaxation group at posttest but not at follow-up
	Deville & Spence, 1999	PTSD (non-combat)	23	Partially randomized; comparing EMDR to Trauma Treatment Protocol (TTP)	Treatment fidelity checked and found to be good; but this is disputed by EMDR proponents	Two therapists: one treated 3 EMDR clients; one treated the other 20 clients; large number of statistical analyses with no Bonferroni correction; possible therapist bias	TTP consistently more effective than EMDR at posttest and follow-up on various standardized measures

Figure 1 is continued on next page

	Wilson et al., 1996	Mixed	18	Randomized, comparing normal EMDR to: 1) EMDR with alternate thumb taps instead of eye movements; and 2) EMDR with eyes static	1 EMDR session with good treatment fidelity	SUDS & VOC vulnerable to experimenter demand bias; no standardized measures of psychological functioning; possible clinician bias	Yes: normal EMDR group better on all measures, but no standardized psychological measures used – only subjective measures vulnerable to demand bias and physiological measures.
	Cusack & Spates, 1999	PTSD (non-combat)	27	Randomized, comparing standard EMDR to EMDR without the cognitive reprocessing component	Up to 3 EMDR sessions. Treatment fidelity checked and found to be good	Low statistical power	No between group differences; both groups made significant pre-post improvement on standardized psychological measures.
	Ironson et al., 2002	PTSD (rape and crime victims)	22	Randomized, comparing EMDR to Prolonged Exposure (PE)	Both groups received 3 preparatory sessions and then 3 sessions of either EMDR or PE	Low statistical power	Both treatments reduced symptoms of PTSD and depression, with no significant between group difference. However, the reduction in symptoms appeared to be faster in the EMDR group, and that group also had a significantly lower treatment dropout rate.
	Lee et al. (2002)	PTSD (non-combat; 29% with multiple traumas)	24	Randomized, comparing EMDR to Stress Inoculation Training with Prolonged Exposure (SITPE)	Both groups received 7 treatment sessions	Non-blind clinician ratings on some measures; large number of statistical analyses with no Bonferroni correction; possible therapist bias	At posttest, no between group difference on global measures of PTSD and depression, but significant differences on subscale scores measuring intrusion symptoms. At three-month follow-up, significant differences favoring EMDR on all but two avoidance measures.

End of Figure 1.