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# A Critical Evaluation of Current Views Regarding Eye Movement Desensitization and Reprocessing (EMDR): Clarifying Points of Confusion



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EMDR is an active psychological treatment for PTSD that has received widely divergent reactions from the scientific and professional community. This article examines points of confusion in the published literature on EMDR, including the theoretical, empirical, and historical issues around EMDR and placebo effects, exposure procedures, the eye movement component, treatment fidelity issues, and outcome studies. It also examines historical information relevant to the scientific process and charges of "pseudoscience" regarding EMDR. We conclude that the confusion in the literature is due to (a) the lack of an empirically validated model capable of convincingly explaining the effects of the EMDR method, (b) inaccurate and selective reporting of research, (c) some poorly designed empirical studies, (d) inadequate treatment fidelity in some outcome research, and (e) multiple biased or inaccurate reviews by a relatively small group of authors. Reading the original research articles frequently helps to reduce the confusion arising from the research review literature. © 2002 John Wiley & Sons, Inc. *J Clin Psychol* 58: 77–97, 2002.

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Eye movement desensitization and reprocessing (EMDR) is an active psychological treatment for Posttraumatic Stress Disorder (PTSD) that has received widely divergent reactions from the scientific and professional community. On one hand, EMDR is a widely

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accepted treatment that has received the endorsement of various scientific and professional groups and numerous reviewers. On the other hand, a significant number of reviewers continue to reject it. For example, some authors refer to EMDR's "impressively large and enduring effects" (Azrin, 1996, p. 82) while others have dismissed EMDR as ineffective, even inferring that "further research on EMDR qua EMDR is unnecessary" (McNally, 1999c, p. 3). Indeed, it has been noted that "many people praise its power for overcoming traumatic memories, whereas others view it as little more than a deftly packaged placebo, a variant of exposure therapy, or both" (McNally, 1999a, p. 225).

The authors of this article have successfully utilized EMDR in the treatment of patients for several years. That experience can add bias as well as expertise to a review of the literature on EMDR. However, the current confusion regarding EMDR requires clarification because of its effects on a wide number of therapists, consumers, and researchers. The purpose of this article is to explore the literature in an attempt to reduce the confusion surrounding EMDR.

### Empirical Validation of EMDR and Foci of Confusion

EMDR was designated an "effective" treatment and received an A/B rating from the Treatment Guidelines Committee of the International Society for Traumatic Stress Studies (ISTSS) which was charged with evaluating treatments for PTSD (Chemtob, Tolin, van der Kolk, & Pitman, 2000). Two years previously, independent reviewers working under the auspices of the American Psychological Association's (APA) Division 12 Task Force on Psychological Interventions recognized EMDR as a "probably efficacious treatment" for civilian PTSD (Chambless et al., 1998) based upon the results of two empirical studies (Rothbaum, 1997; Wilson, Becker, & Tinker, 1995). The only other treatments cited by Chambless et al. as probably efficacious for PTSD were exposure treatment, again based upon the results in two studies (Foa, Rothbaum, Riggs, & Murdock, 1991; Keane, Fairbank, Caddell, & Zimering, 1989), and Stress Inoculation Training (SIT), based upon one study (Foa et al., 1991). Chambless and colleagues (1998) found no "well established treatments" for either civilian or combat-related PTSD.

In addition to the studies reviewed by Chambless et al. (1998), other well-designed outcome studies have supported the efficacy of EMDR with civilian PTSD (Marcus, Marquis, & Sakai, 1997; Scheck, Schaeffer, & Gillette, 1998) and combat-related PTSD (Carlson, Chemtob, Rusnak, Hedlund, & Muraoka, 1998). A meta-analysis of interventions for PTSD, including pharmaceuticals, found that both exposure therapy and EMDR were effective but that EMDR was more rapid in achieving its effects (Van Etten & Taylor, 1998). Long-term follow-up studies, however, were largely lacking. In one of the few studies of its kind, Wilson, Becker, and Tinker (1997) conducted a 15-month follow-up of their 1995 study cited by Chambless et al. (1998), reassessing subjects on the same measures. They found that treatment gains remained intact over the 15-month period.

In addressing the misunderstandings regarding EMDR, we first will address the major points of confusion regarding EMDR results, then will identify the statements leading to confusion and false conclusions, and finally will attempt to clarify the confounding issues in the data and in data interpretations. Specifically, we will examine the role of placebo effects, exposure procedures and EMDR, eye movements themselves, and comparison research. Finally, we feel compelled to address issues related to personal accusations and slurs against researchers and clinicians supporting the use of EMDR (e.g., see McNally, 1999a; Herbert et al., 2000a, 2000b). While we view such behavior as unfortunate and on the order of "antiscientific attitudes" (Beutler & Harwood, 2001), we do not wish to let such statements go unchallenged.

Table 1 gives a brief overview of the confusion in the review literature regarding EMDR, with the left column illustrating sources of the confusion and the right column providing clarifying information. The contents of the right column are coded as follows: "M" represents misreported or misstated data; "O" signifies data or contextual information which was omitted; and "C" indicates data which is contradictory to the information given in the source of confusion. More comprehensive examples and explication are contained in the following sections.

### Placebo Effects

In a review of the EMDR literature, McNally (1999c) dismissed the two PTSD wait-list control group studies (Rothbaum, 1997; Wilson et al., 1995, 1997) cited by Chambless et al. (1998) with statements such as one "cannot exclude the possibility that whatever benefits achieved are merely the consequence of nonspecific ('placebo') factors common to all psychotherapies" (p. 1). To lend support to his placebo hypothesis, McNally (1999c) stated that, "Consistent with this possibility, response to EMDR is strongly related to suggestibility in patients with PTSD ( $r = .86$ ; Forbes et al., 1994)" (p. 1). McNally omitted the statement by Forbes, Creamer, and Rycroft (1994) that this correlation was only due to decreased avoidance and that at three-month follow-up "that relationship was no longer significant" (p. 117). Likewise, Lohr, Lilienfeld, Tolin, and Herbert (1999) expressed a similar concern over placebo or nonspecific treatment effects. However, both McNally (1999c) and Lohr et al. (1999) failed to note that previous reviewers have found an "unresponsiveness of PTSD to placebo" (Solomon, Gerrity, & Muff, 1992, p. 634), and that Van Etten and Taylor (1998) concluded from their meta-analysis that "the effect sizes of EMDR tended to be larger than those of control conditions, such as pill placebo and supportive psychotherapy" (p. 140), indicating that placebo is not a plausible interpretation of the achieved results. For instance, the Rothbaum (1997) and the Wilson et al. (1995, 1997) studies cited previously demonstrated the elimination of 84 to 90% of the PTSD diagnoses in three treatment sessions, and these response rates were maintained over three- and 15-month follow-up periods. In other words, the treatment effects of EMDR are much larger and longer lasting than placebo effects in PTSD, and the empirical evidence does not support the placebo hypothesis.

### EMDR, Exposure, and Flooding Procedures

A group of coauthors in various publications (e.g., Herbert et al., 2000a, 2000b; Lohr, Lilienfeld, Tolin, & Herbert, 1999; McNally, 1999b; Rosen, Lohr, McNally, & Herbert, 1998; Rosen, McNally, Lohr, Devilly, Herbert, & Lilienfeld, 1998a, 1998b) advance the thesis that EMDR is nothing more than a simple exposure therapy. Indeed, Lohr et al. (1999) stated that if EMDR had been presented as an exposure therapy "much of the controversy . . . could have been avoided" (p. 201). In support of this thesis, Herbert et al. (2000a) stated that the clinical effects of EMDR "have been addressed by alternative conceptual analyses based on well-established learning processes that subsume exposure and cognitive-behavioral treatments (Dyck, 1993; MacCulloch & Feldman, 1996)" (p. 949). This position fails to notice that both articles (Dyck, 1993; MacCulloch & Feldman, 1996) cite reasons why EMDR seems distinct or unique, could be effective, and deserves attention (especially the rapid "relaxing (de-arousing) reflex," MacCulloch & Feldman, 1996, p. 577). This novelty of effect, of course, raises the question of whether EMDR could be simply another derivative of exposure and CBT treatments, or if it is something distinct.

Table 1  
*Examples of Confusion in Substantive Scientific Issues*

Placebo Effects	
<p>Source of Confusion</p> <p>1. McNally (1999c) stated that “response to EMDR is strongly related to suggestibility in patients with PTSD (<math>r = .86</math>; Forbes et al., 1994)” and rejected two wait-list control studies (Rothbaum, 1997; Wilson et al., 1995) on the basis of possible “nonspecific (‘placebo’) factors” (p. 1). This involves a misreporting and an omission of the reported data.</p>	<p>Contents of Data and Literature</p> <p>1. (M) Forbes et al. (1994, p. 117): Correlation at posttest was to the “‘avoidance’ symptom cluster” only; at 3-month follow-up, “that relationship was no longer significant.”</p> <p>1. (O) Rothbaum (1997) and Wilson et al. (1995): Large effect sizes obtained (90 and 84% elimination of PTSD, respectively) would not be expected from “suggestibility” or placebo (see also Van Etten and Taylor, 1998). Studies also included 3-month follow-ups, and Wilson et al. (1997) provided a 15-month follow-up showing no deterioration of effects.</p>
Exposure Procedures	
<p>Sources of Confusion</p> <p>1. Herbert et al. (2000a) suggested that EMDR is best explained by “learning processes that subsume exposure and cognitive-behavioral treatments (Dyck, 1993; MacCulloch et al., 1996)” (p. 949). This statement omits the position of MacCulloch et al.</p> <p>2. McNally (1999b) raised the possibility that EMDR is a variant of exposure therapy by stating: “Van Etten and Taylor conclude that EMDR is no more effective than standard cognitive-behavioral methods for PTSD—a conclusion wholly congruent with the view that the novel component of EMDR (eye movements) adds nothing to the traditional imaginal exposure component. . . . Therefore, <i>what is effective in EMDR is not new, and what is new is not effective</i>” (p. 619). This omits Van Etten and Taylor’s position and contradictory data.</p> <p>3. Devilly (2001) hypothesized that the “comparative inefficacy [of EMDR] to exposure-based interventions is due to distraction during exposure, which impedes extinction of the fear response” (p. 18). This omits contradictory data.</p>	<p>Contents of Data and Literature</p> <p>1. (O) MacCulloch et al. (1996) stated: “Until the serendipitous discovery of EMDR by Shapiro, no powerful relaxing (de-arousing) reflex was known. . . .” (p. 577), and “EMDR might be the most important development . . . since Wolpe’s (1958) initial reports of desensitization. . . .” (p. 578).</p> <p>2. (O) Van Etten and Taylor (1998) stated that EMDR “is more efficient. . . . which suggests that another treatment component specific to EMDR is active” (p. 140).</p> <p>2. (O) Boudewyns and Hyer (1996): “In a strict exposure therapy the use of many of these [EMDR] techniques is considered contrary to theory” (p. 192).</p> <p>2. (C) Chemtob et al. (2000) noted that the efficacy of the “multiple, brief, interrupted exposures to traumatic material [typical of EMDR] calls for a reexamination of traditional theoretical notions that prolonged, continuous exposure is required (Eysenck, 1979)” (pp. 151–152).</p> <p>3. (O) Civilian studies (e.g., Marcus et al., 1997; Rothbaum, 1997; Scheck et al., 1998; Vaughan et al., 1994; Wilson et al., 1995) reported EMDR treatment effects approximately equivalent to exposure-based interventions with significantly less treatment time (see also Van Etten &amp; Taylor, 1998).</p>
Eye Movements	
<p>Sources of Confusion</p> <p>1. Lohr et al. (1998, p. 145): “early . . . single-subject designs suggested that eye movements are not necessary for reduction of verbal reports of symptoms (Acierno et al., 1994; Lohr 1995, 1996; Montgomery &amp; Ayllon, 1994a, 1994b).” This misreports the data.</p> <p>2. Herbert et al. (2000a, 2000b), McNally (1999b), Rosen et al. (1998), and Rosen et al. (1998a, 1998b) cited Lohr et al. (1998) and concluded that eye movement “adds nothing to the traditional imaginal exposure component” of EMDR (McNally, 1999b, p. 619).</p>	<p>Contents of Data and Literature</p> <p>1. (M) Lohr et al. (1995): “The addition of the eye movement component appeared to have a distinct effect in reducing the level of [SUD] ratings” (p. 149).</p> <p>1. (M) Lohr et al. (1996): “Only when the eye movement was added was there a substantial reduction of SUD ratings” (p. 85).</p> <p>1. (M) Montgomery et al. (1994b): “The addition of saccadic eye movements [in 5 of 6 subjects] . . . resulted in the significant decreases in self-reports of distress previously addressed” (p. 228).</p> <p>2. (M) Multiple citations of Lohr et al. (1998) misstate the original data reported earlier.</p>

Table 1 (Continued)

Outcomes	
Sources of Confusion	Contents of Data and Literature
<ol style="list-style-type: none"> <li>1. Rosen (1999) stated that “no study subsequent to Shapiro (1989[a]) has matched the spectacular finding found in that original report” (p. 181). This misstates the data.</li>   <li>2. Lohr et al. (1998) stated “that there is little ordinary evidence and no extraordinary evidence to support the efficacy of EMDR” (p. 144). Lohr et al. (1998) did not mention Carlson et al. (1998) or Marcus et al. (1997) and fail to delineate for the reader the treatment outcomes of Rothbaum (1997) and Scheck et al. (1998). This omits relevant data.</li>   <li>3. Devilly (2001) hypothesized that positive EMDR treatment outcomes in certain studies might have been heightened due to “strong [researcher] allegiance to EMDR” and that “it is possible that demand and the biasing effects of face-to-face interviews influenced the outcome of these studies” (p. 20). However, Devilly ignored the results of studies cited in his own article which contradict this hypothesis (e.g., Carlson et al., 1998; Rothbaum, 1997).</li>   <li>4. Devilly (2001) described a pattern of “declines in effect sizes from posttreatment to follow-up” (p. 19), citing studies such as Rothbaum (1997) and Carlson et al. (1998), and “The Poor Long-Term Outcome of EMDR” (p. 18), citing studies such as Devilly et al. (1998) and Pitman et al. (1996). This entails misstatements and omissions.</li> </ol>	<ol style="list-style-type: none"> <li>1. (M) Shapiro (1989a) reported on “anxiety desensitization” and one symptom using no standardized measures and “an average treatment time of five sessions” for full PTSD elimination (p. 221). Subsequent studies (e.g., Marcus et al, 1997; Rothbaum, 1997; Scheck et al., 1998; Wilson et al., 1995, 1997) reported eliminating the majority of civilian PTSD diagnoses and symptoms in two to three sessions.</li> <li>2. (O) Carlson et al. (1998) found significant effects using EMDR with combat veterans (e.g., 77% elimination of PTSD at three-month follow-up).</li> <li>2. (O) Marcus et al. (1997) found significant EMDR effects in an HMO population (e.g., 100% elimination of single trauma PTSD).</li> <li>2. (O) Rothbaum (1997) reported a 90% elimination of PTSD with rape victims in three treatment sessions.</li> <li>2. (O) Scheck et al. (1998) reported large effect sizes and the EMDR group within one standard deviation of the norm after two sessions.</li> <li>3. (M) Carlson et al. (1998) reported positive treatment effects using therapists who “shared no particular expectations with respect to the outcomes of EMDR therapy” (p. 7) and had backgrounds in biofeedback, behaviorism, and psychodynamic psychotherapy. The therapists conducted the pretreatment interviews, and blind independent reviewers were used at follow-up.</li> <li>3. (M) Rothbaum (1997) reported positive results with EMDR even though she has an affiliation with CBT. Blind independent assessors were used.</li> <li>3. (O) Devilly (2001) cited studies by Foa et al. (1991) and Marks et al. (1998) as support for exposure therapy, yet these principal investigators advocate exposure therapy. Devilly made no mention of possible allegiance effects or that the same assessment strategy was used as in the EMDR studies.</li> <li>4. (O) Many civilian studies show good maintenance of treatment effects at follow-ups ranging from three months (e.g., Rothbaum, 1997; Scheck et al., 1998; Vaughan et al., 1994; Wilson et al., 1995) to 15 months (Wilson et al., 1997).</li> <li>4. (M) Rothbaum (1997): elimination of 90% of PTSD diagnoses in rape victims; psychometric evaluation at three-month follow-up revealed good maintenance of treatment effects on all primary measures.</li> <li>4. (M) Carlson et al. (1998): 12 EMDR sessions with multiple targets for combat veterans; elimination of 77% of PTSD diagnoses at three-month follow-up; elimination of 75% of PTSD diagnoses at nine-month follow-up (Chemtob et al., 2000; change due to elimination of one subject).</li> <li>4. (O) Pitman et al. (1996) utilized EMDR to treat only one memory with multiply traumatized veterans, resulting in weak main treatment effects that dissipated at five-year follow-up (Maklin et al., 2000). (As noted by Devilly, 2001, Devilly et al., 1998, used only two sessions with the same population and achieved similar results at six-month follow-up.)</li> </ol>

(continued)

Table 1 (Continued)

Outcomes (continued)	
Sources of Confusion (continued)	Contents of Data and Literature (continued) 4. (C) Van Etten and Taylor (1998): "Across all self-report and observer-rated measures of PTSD symptoms, depression and anxiety, both behaviour therapy and EMDR demonstrated a maintenance of treatment effects at follow-up. . . . Differences in effect size from posttreatment to follow-up were nonsignificant for all measures across both conditions, except that EMDR demonstrated a significant increase in effect size for observer-rated total PTSD symptoms at follow-up, making it equal to behaviour therapy" (p. 138).
Theory	
Source of Confusion	Contents of Data and Literature
1. Herbert et al. (2000a) charged that EMDR does not provide falsifiable hypotheses but instead retreats to "auxiliary hypotheses" (p. 958). This omits mention of material presented by Shapiro (1991a, 1995).	1. (O) Chapter 12 of Shapiro (1995) gives multiple falsifiable hypotheses reiterated from earlier publications (e.g., Shapiro, 1991a) along with specific parameters for adequate testing.

M = misreporting or misstatement of data; O = omission of data or context; C = contradictory data.

The publications cited previously (i.e., Herbert et al., 2000a, 2000b; Lohr et al., 1999; McNally, 1999b; Rosen et al., 1998; Rosen et al., 1998a, 1998b) attempt to dismiss EMDR as simply a variant of exposure combined with inconsequential eye movements. However, as Boudewyns and Hyer (1996) point out, "In strict exposure therapy the use of many [EMDR treatment components] is considered contrary to (exposure) theory" (p. 192). Also ignored is a body of research and theory which clearly calls for prolonged, uninterrupted, and undistracted stimulus exposure (e.g., Chaplin & Levine, 1981; Eysenck, 1979; Foa, Steketee, & Rothbaum, 1989; Lyons & Keane, 1989; Lyons & Scotti, 1995; Marks, 1972; Marks, Lovell, Noshirvani, Livanou, & Thrasher, 1998; Rodriguez & Craske, 1993) rather than the short bursts of attention and free association used in EMDR procedures.

According to a strict exposure definition, EMDR's procedures should sensitize rather than desensitize its recipients (Marks et al., 1998):

In vertebrates and invertebrates, exposure gradually reduces defensive responses to cues to which the subject is exposed; this habituation depends on the dose of exposure. Continuous stimulation in neurons and immune and endocrine cells tends to dampen responses, and intermittent stimulation tends to increase them. (p. 324)

The use of short bursts of exposure to the traumatic material during treatment sessions is well documented in numerous EMDR client transcripts (Manfield, 1998; Shapiro, 1995; Tinker & Wilson, 1999) and contrasts sharply with the expected minimum of 25 to 100 uninterrupted minutes recommended for exposure procedures (Chaplin & Levine, 1981; Foa et al., 1989; Keane, 1995). According to accepted exposure models (Marks et al., 1998), EMDR's brief, interrupted exposure to traumatic memories should sensitize clients and make their condition worse. However, no such evidence exists. Numerous authors (e.g., Boudewyns & Hyer, 1996; Chemtob et al., 2000; Pitman et al., 1996; Rogers

et al., 1999; Shapiro, 1995, 1999; Tinker & Wilson, 1999) have articulated the fact that EMDR deviates from the traditional use of exposure.

McNally (1999b) also compares EMDR to systematic desensitization to bolster his argument of EMDR as being an “exposure variant,” but then fails to notice that, unlike EMDR, systematic desensitization is not particularly helpful in the treatment of PTSD (Brom, Kleber, & Defares, 1989) and does not use free association. Furthermore, EMDR starts at the most intense stimuli and generally seeks to avoid therapist-directed, emotional-state changes to stimuli whereas systematic desensitization does quite the opposite.

EMDR also seems more efficient in that it requires much less total treatment time than previously reported in PTSD exposure treatment literature. For example, in the Van Etten and Taylor (1998) meta-analytic review of PTSD studies, the authors concluded that “the results of the present study suggest that EMDR is effective for PTSD and that it is *more efficient* [italics added] than other treatments” (p. 140). Research indicates the elimination of the majority of single-incident PTSD diagnoses in three to five hours of EMDR treatment (Marcus et al., 1997; Rothbaum, 1997; Wilson et al., 1995, 1997). In contrast, exposure treatment tends to require 16 to 60 hr of combined in-session and homework exposure (e.g., Foa et al., 1991; Marks et al., 1998; Richards, Lovell, & Marks, 1994; Tarrrier et al., 1999).

It appears that the confusion has been confounded by not only the elimination of data from some reviews, but also by selective citation. For instance, when Greenwald (1999) pointed out McNally’s (1999a) failure to cite Van Etten and Taylor (1998), McNally (1999b) replied that the article had not been published when he was writing his literature review (even though he cited seven other 1998 works). He then ignored the statement of superior efficiency reported in the meta-analysis and stated:

In any event, Van Etten and Taylor conclude that EMDR is no more effective than standard cognitive-behavioral methods for PTSD—a conclusion wholly congruent with the view that the novel component of EMDR (eye movements) adds nothing to the traditional imaginal exposure component (Lohr, Tolin, & Lilienfeld, 1998). *Therefore, what is effective in EMDR is not new, and what is new is not effective.* (p. 619)

This is an inaccurate statement resulting from the misreporting of the meta-analytic findings. Indeed, Van Etten and Taylor (1998) concluded that EMDR and exposure therapy are distinct in some fashion:

Some might argue that EMDR works through exposure and desensitization, similar to behaviour therapy. However, this is unlikely to be the case given that EMDR provides significantly less trauma exposure than behaviour therapy and is demonstrating comparable results, which suggests that another treatment component *specific to EMDR* [italics added] is active. (p. 140)

In other words, Van Etten and Taylor concluded that EMDR is not simply imaginal exposure, a conclusion which runs completely counter to McNally’s (1999a) argument and is omitted by McNally.

Some reviewers use the uncertain role of eye movements in the effects of EMDR to indicate that EMDR is an exposure therapy. It should be noted, however, on the basis of the research cited earlier, that even if eye movements do not prove central to the effects of EMDR, it still is questionable to consider EMDR a variant of exposure or systematic desensitization.

### Eye Movements

The role of the eye movement component in the EMDR process awaits empirical validation. However, the confused reporting of eye movement research has led some writers to

the premature conclusion that eye movements do not contribute to the effects of EMDR. In reality, the role of eye movements (and alternative dual-attention stimuli) in the EMDR process remains a matter for more adequate empirical testing.

Inaccurate or incomplete reviews of some of the early single-subject design data have been misleading and misinterpreted in subsequent scientific literature. For example, Herbert et al. (2000a, 2000b), Rosen et al. (1998), and Rosen et al. (1998a, 1998b) all cite Lohr et al. (1998) in their conclusion that eye movement "adds nothing to the traditional imaginal exposure component" of EMDR (McNally, 1999b, p. 619). Remember, however, that the "traditional imaginal exposure component" of EMDR claimed by McNally (1999b) was demonstrated earlier not to be traditional at all, either in theory or by procedures used in the exposure treatment they reported. But more importantly, the citation of Lohr et al. (1998) brings additional problems to the body of review literature due to the incomplete reporting of data from previous studies.

For example, Lohr et al. (1998) stated, "Early experimental research with single-subject designs suggested that eye movements are not necessary for reduction of verbal reports of symptoms (Acierno, Tremont, Last, & Montgomery, 1994; Lohr, Tolin, & Kleinknecht, 1995, 1996; Montgomery & Ayllon, 1994a, 1994b)" (p. 145). In fact, three of the five citations are studies that actually provide some support for the role of eye movements in EMDR in the reduction of verbal reports of symptoms (Lohr et al., 1995, 1996; Montgomery & Ayllon, 1994b). Furthermore, one of the remaining two citations is a single-subject outcome study that provided evidence of a positive outcome using the old EMD method, but did not investigate the specific role of the eye movement component (Montgomery & Ayllon, 1994a). The last is a single-subject design EMD study which was plagued by such severe treatment fidelity problems that main-treatment effects were absent, much less yielding the power to assess component effects (Acierno et al., 1994).

Given these discrepancies (also see Lipke, 1999), it is surprising to note that two of the studies supporting the role of eye movements (Lohr et al., 1995, 1996) even involved some of the same researchers who misreported the results in the Lohr et al. (1998) article. Although cited to the contrary, Lohr et al. (1995) provided graphic illustrations of the positive effects of eye movements in reducing verbal reports of subjective distress. This is reflected in their conclusion that, "The addition of the eye movement component appeared to have a distinct effect in reducing the level of ratings which then showed further reductions as the procedure progressed" (p. 149). Lohr et al. (1996) found that with their first subject, an analogue procedure without eye movements produced reduction in SUD ratings on contemporary images, but not on the image that they judged to be etiological. "Only when the eye movement was added was there a substantial reduction of SUD ratings" (p. 85). With their second subject, "the EMDR analogue procedure was ineffective with all images. Only when the eye movement procedure was added did SUD ratings decline" (p. 86). As further support for the utility of eye movements, Montgomery and Ayllon (1994b) conducted a study of six PTSD clients in a multiple baseline design and found that five of six needed the eye movements for positive effects. They concluded from their data that no statistically significant reduction in SUD ratings occurred without eye movements but that "the addition of saccadic eye movements to the treatment package (thereby replicating Shapiro's original protocol) resulted in the significant decreases in self-reports of distress previously addressed" (p. 228). Changes in physiological measures were in the predicted direction, but did not achieve statistical significance.

Of concern is the fact that Lohr et al. (1998) characterized the role of eye movements as "not necessary for reduction of verbal reports of symptoms" (p. 145) when citing these single-subject design studies that clearly point to an opposite conclusion. Reviews by

McNally (1999b), Herbert et al. (2000a, 2000b), Rosen et al. (1998), Rosen et al. (1998a, 1998b) evoked more confusion when they cited Lohr et al. (1998) as the source for their rejection of the importance of eye movements in EMDR treatment. This process takes readers of the research summaries progressively further away from the primary data by multiple citations in subsequent articles (by the same authors).

Group-design studies have begun to address the importance of eye movements in EMDR, but as noted in multiple reviews (e.g., Chemtob et al., 2000; Feske, 1998; Spector & Read, 1999), the results are inconclusive. Although a number of studies have attempted to assess the role of eye movements using multiply traumatized combat veterans (Boudewyns & Hyer, 1996; Boudewyns, Stwertka, Hyer, Albrecht, & Sperr, 1993; Devilly, Spence, & Rapee, 1998; Pitman et al., 1996), the results of many of these studies must be evaluated carefully because of methodological limitations. Two particular problems were present in all of these studies: inadequate sample size and insufficient duration of treatment for this population. For example, Boudewyns et al. (1993) and Devilly et al. (1998) both provided only two sessions. Boudewyns and Hyer (1996) utilized five to seven sessions, but targeted only one memory (Hyer, personal communication, August 2, 2000). Pitman et al. (1996) reported a mean total of 9.7 sessions for their combined EMDR and analogue conditions for each subject, but they utilized only one target for EMDR and one target for their analogue condition. Such small amounts of treatment in this multiply traumatized population have resulted in weak main-treatment effects which at times have not been sustained at follow-up (Devilley et al., 1998; Macklin et al., 2000). This weak main-treatment effect leaves the assessment of component effects such as eye movement almost impossible to detect. Although the cumulative results of research have shown that two to three sessions of treatment can produce powerful and lasting treatment effects in civilian populations (Rothbaum, 1997; Scheck et al., 1998; Wilson et al., 1995, 1997), multiply traumatized populations require multiple targets with many more sessions for the treatment to be complete and robust (e.g., Carlson et al., 1998). Dismantling studies with higher treatment doses are much more likely to produce main effects sufficient to provide the power needed to assess the effects of treatment components.

Sample sizes also have been inadequate to assess treatment component effects. Kazdin and Bass (1989) demonstrated that the study of expected (small to moderate) differences over controls requires a minimum of 30 to 40 subjects per cell (also see Kazdin, 1998). None of the current dismantling studies met this criterion. Studies such as Renfrey and Spates (1994) utilized only seven to eight subjects per cell, yet Lohr, McNally, Herbert, and their colleagues in multiple articles would lead us to believe that the results provide definitive proof that eye movements are superfluous.

The literature on eye movements takes an additional turn when Rosen (1995) appears to draw into question Shapiro's (1989a) description of her first use of eye movements that provided the origin of EMDR. Shapiro (1989a, 1995) asserted that the eye movements in which she engaged when first observing a possible relationship between oculomotor behavior and the reduction of negative thoughts and emotions were *saccadic*. However, Rosen (1995, 1997) argued that Shapiro's eye movements could not have been saccadic, based on his belief that people are unable to perceive such movements. Welch (1996) argued that the eye movements must have been saccadic and states that outside of the treatment literature "it was noted more than 20 years ago that saccadic eye movements often accompany such cognitive processes as problem solving and imaginal activity (e.g., Antrobus, 1973)" (p. 178). Furthermore, since Shapiro's eye movements were made in a lighted environment (in contrast to the darkened laboratory used in most research on saccadic behavior), she had available *visual* feedback to tell her that her eyes were darting about in a saccadic fashion.

From this review, it is clear that the role of eye movements in the EMDR process awaits adequate empirical research for exclusion or validation as a useful component of the treatment. Attempts to discount the reasons for the use of eye movements are inaccurate and premature. Furthermore, EMDR has received empirical validation as a treatment for PTSD, and the tested procedure includes the eye movement (or alternative dual-attention) component. Therefore, the removal of these stimuli from the validated procedure requires prior component analyses adequate to rule them out as a significant treatment element. In the absence of such studies, their removal is without empirical justification.

#### Methodological and Fidelity Issues in Outcome Studies

Some writers also cite the inaccurate reports of component analyses to dismiss the need for appropriate fidelity in EMDR research. For instance, after citing the erroneous reporting of eye movement research discussed previously, McNally (1999b) also dismisses the issue of treatment fidelity raised by Greenwald (1999): "This fact renders Greenwald's learned disposition on treatment fidelity moot: if eye movements are casually inert elements irrelevant to outcome, who cares whether they are induced 'correctly'?" (McNally, 1999b, p. 619). In taking this stance, McNally is making two very important assumptions: (a) that EMDR consists of exposure and inconsequential eye movements only; and (b) that fidelity checks are unimportant. First, EMDR is a complex procedure and has been described as such by many writers (e.g., Azrin, 1996; Boudewyns & Hyer, 1996; Edmond, Rubin, & Wambach, 1999; Fensterheim, 1996; Hyer & Brandsma, 1997; Shapiro, 1995, 1999), and it is certainly not comprehensively defined as exposure and simple eye movements. Second, McNally is rejecting the importance of treatment fidelity checks or "treatment adherence ratings" (Foa & Meadows, 1997) that are emphasized by so many researchers and authors as basic to the scientific assessment of any method (e.g., Foa & Meadows, 1997; Kazdin, 1998), including CBT (McNally, 1996) and EMDR (e.g., Greenwald, 1996; Lipke, 1999; Shapiro, 1995, 1996a, 1996b, 1999). In reality, EMDR is a complex treatment that requires fidelity to the method to achieve an accurate scientific evaluation.

While the need for treatment fidelity in outcome research should be self-evident, Rosen's (1999) article serves to thoroughly obfuscate the fidelity issue in a variety of dimensions. His premise is that fidelity ratings can be confounded with outcome, so that when sessions end without improvement, fidelity raters assume that EMDR was not conducted faithfully. Rosen then applies this hypothesized confound between outcome and fidelity to dismiss the negative fidelity ratings assigned in several studies. It is true that there are practical difficulties in assessing fidelity independently of clients' response to any treatment. However, Rosen confuses the issue by inaccurately describing the EMDR process and states, "EMDR requires repeated sets of eye movements until the patient lowers subjective ratings of distress" (p. 178). In fact, this is not the case. The EMDR method requires that the therapist continue desensitization (Phase 4) of the target incident as long as the client is reporting changes in the level of distress, not just until subjective ratings are decreased. In EMDR, unlike exposure, the lessening of disturbance is an indicator of the need for continuing treatment of the same target. The session continues to accommodate the eight phases of treatment, and premature termination of treatment while the clients are still processing the target incident is a clear violation of the protocol.

Rosen (1999) suggests that poor treatment outcome is the reason that negative fidelity ratings were assigned to studies such as Jensen (1994), who investigated the response of 25 Vietnam combat veterans to two sessions of EMDR. Contrary to Rosen's statement,

the Jensen sessions evaluated for fidelity ended “with improvement,” with reduced in-session subjective anxiety (as measured by SUD ratings). While no decrease in PTSD symptoms (as measured by global psychometrics) was reported in the published article (Jensen, 1994), the fidelity rater was blind to these test results at the time the fidelity rating was assigned (Howard Lipke, personal communication, February 13, 2001). Lipke assigned the negative fidelity rating (Jensen, 1994) because Jensen prematurely terminated desensitization. Lipke (1999) reported that sessions were stopped while the participants’ distress was still declining, and before the SUD rating reached 0 to 1. Treatment termination appeared to be based not on time limitations, but on the therapists’ uncertainty regarding “the extent to which the therapist can or should continue with the active treatment (i.e., continue to induce saccadic eye movements) with subjects” (Jensen, 1994, p. 323).

Other issues of fidelity have to do not only with within-session breaches of protocol but also between-session breaches. For example, EMDR is an eight-phase treatment approach, with Phase 8 being reevaluation (Shapiro, 1995, 1999). The PTSD protocol of EMDR also has three stages—the targeting of past events, present stimuli, and templates for future action (Shapiro, 1995, 1999)—and is the protocol validated in EMDR civilian studies. In the case of multiply traumatized clients, if this procedure is truncated, the effects of EMDR may be incomplete.

The problem of truncated protocols, through an insufficient course of treatment (e.g., two sessions) or inadequate numbers of targets, has seriously confused the EMDR group-design outcome studies of multiply traumatized combat veterans (Boudewyns et al., 1993; Boudewyns & Hyer, 1996; Devilly et al., 1998; Jensen, 1994; Macklin et al., 2000; Pitman et al., 1996). Weak or absent treatment effects are hardly a surprising result in these truncated studies. If the EMDR PTSD protocol is an effective treatment strategy, then truncated protocols may produce incomplete effects. To obtain robust and stable treatment effects with multiply traumatized veterans, targeting multiple traumatic events is necessary with the use of any procedure (e.g., Fairbank & Keane, 1982). When a more robust EMDR treatment (12 sessions of EMDR) was applied to a similar population, much better results (the elimination of 77% of the PTSD diagnoses) were obtained (Carlson et al., 1998).

An additional problem is the use of inexperienced and inappropriate fidelity assessors. For instance, Devilly and Spence (1999) provided a controlled outcome study that found their Trauma Treatment Protocol to be superior to EMDR in the treatment of PTSD. However, in addition to a variety of methodological problems (see Chemtob et al., 2000; Maxfield & Hyer, in press) and despite fidelity being rated as “good” (p. 150), readers were referenced to Devilly et al. (1998) for a description of procedures, revealing extensive problems in EMDR treatment delivery including inaccurate instructions, rating the negative rather than the positive cognition during assessment, reexposure to the negative cognition during desensitization, frequent SUD ratings during desensitization, inappropriate focus on the positive cognition during eye movements before it was paired with the picture or incident for installation, and a lack of attention to the standard PTSD protocol. These are clear deviations from the standardized method (Shapiro, 1995).

Issues related to fidelity in outcome research on EMDR are further complicated by the selective or inaccurate reporting of outcome research and historical data by a small group of coauthors. For example, Rosen, McNally, and Lilienfeld (1999), McNally (1999a), and Herbert et al. (2000a, 2000b) make references to the evolving or “protean” procedures of EMDR when in fact the EMDR method has been stable since 1991 (Shapiro, 1991a). They also state that Shapiro refers to her initial EMD study (1989a) as EMDR, so that she must have confused fidelity and outcome. This is simply untrue. Shapiro states

clearly that additional procedures were used in the study (1989a, 1989b), and these procedures constitute the basis for additional treatment elements taught in EMDR trainings. The codified EMDR procedures have been taught since 1991 (Shapiro, 1991a, 1991b, 1991c) and were published in 1995 (Shapiro, 1995). Therefore, they are not only stable, but should be used unequivocally to assess the EMDR method in outcome research.

Rosen (1999) also argues against the need for fidelity in EMDR research by stating, "It also is most unclear why EMD is portrayed as inferior to EMDR when no study subsequent to Shapiro (1989[a]) has matched the spectacular findings found in that original report" (p. 181). This statement is inaccurate. Shapiro (1989a) stated that the EMD procedure "serves to desensitize the anxiety . . . not to eliminate all PTSD-related symptomatology and complications, nor to provide coping strategies for the victims" (p. 221) and reported "an average treatment time of five sessions" (p. 221) for the elimination of PTSD. As stated by Lipke (2000), "It is essential to note that Shapiro's [1989a] initial report only claimed that the subjects she treated were able to resolve a *single episode of trauma* in one session" (p. 2) as measured primarily by the report of subjective anxiety in SUD ratings. Subsequent studies (Marcus et al., 1997; Rothbaum, 1997; Scheck et al., 1998; Wilson et al., 1995, 1997) reported the elimination of the majority of civilian PTSD diagnoses and symptoms in two to three sessions using a wide range of psychological indicators, thus meeting or exceeding Shapiro's (1989a) initial report on EMD. Evidence from subsequent controlled research has replicated the decrease in SUD ratings in the first session (Rogers et al., 1999; Wilson, Silver, Covi, & Foster, 1996; Wilson et al., 1995), which constitutes a decrease in verbal reports of subjective discomfort, but, as noted by Shapiro (1989a), is not a single session treatment for PTSD. It should be noted, however, that current EMDR research studies and practices use the SUD ratings to identify distress caused by any negatively impacting emotion (e.g., sadness, guilt, anger) and not merely the "anxiety" ratings taken by Shapiro (1989a).

Readers must familiarize themselves with the EMDR method to know when treatment fidelity has been breached in any given study and be alert to the presence of fidelity checks or "treatment adherence ratings" (Foa & Meadows, 1997). Readers also should be aware of the fact that only appropriately trained experts in the treatment being studied should be used as therapists and fidelity raters (see Ethical Standards 1.04b, c, & 6.07c, American Psychological Association, 1992, pp. 1600, 1608).

#### Historical Misinformation, Slurs, and Charges of "Pseudoscience"

In the last year, scientific debate has begun to degenerate into slurs, innuendo, and ad hominem attacks. Because these statements have been made publicly on the Internet and in published articles (e.g., McNally, 1999a; Herbert et al., 2000a), a public reply is necessary. For example, when EMDR was cited as "probably efficacious" for civilian PTSD by independent reviewers working under the auspices of the APA Division 12 Task Force, members of the Task Force were themselves criticized (e.g., as being "EMDR puppets"), unsubstantiated claims were made (subsequently investigated by the committee and determined to be unfounded) that "the research results were faked or unduly influenced by Francine Shapiro," and those critical of the task force decision argued that the criteria be systematically altered to exclude EMDR while not effecting the other more traditional treatments. The recommendations made by those opposed to EMDR generally seemed to confound their allegations of ethical breaches with empirical research findings (Beutler & Harwood, 2001, pp. 48–50).

In another example of this phenomenon, McNally (1999a) proceeds to make statements that readers could misconstrue as insinuations of serious ethical violations by

Shapiro. This occurs when he discusses the inclusion of a quote in the publisher's materials of a book by Shapiro and Forrest (1997) and implies a studied misrepresentation by Shapiro. In fact, the editor retained the quote (along with others from different sources) as publisher's materials and included it in a list of quotes preceding the title page of the book. It read, "EMDR 'comes of age.' . . . Recent independent studies have found it up to 90 percent successful" (McNally, 1999a, p. 230). The publisher attributed the quote to the "American Association for the Advancement of Science" (AAAS) because it was retained from a radio program in which the reporter summarized what he believed subsequent to an interview and signed off with "for the American Association for the Advancement of Science" (*Science Update*, September 1996). In a subsequent letter to AAAS, the Basic Books staff accepted full responsibility for the quote as well as its propriety and informed AAAS that since Shapiro had asked for the removal of the quotation, they would comply by ceasing to use it (Christopher Goff, June 9, 1997). In a similar account of another event, McNally (1999a) inaccurately describes an interaction between Shapiro and the then-president of AABT regarding a "potentially misleading" citation (Zeiss, 1998, p. 28) on an EMDR Institute brochure. Again, McNally infers a deliberate misrepresentation on the part of Shapiro and fails to note that Zeiss (1998) indicated that Shapiro reported attending to the problem before they spoke and concluded that "Dr. Shapiro has responded to the concern of myself and the Board openly and in a constructive spirit, and I believe that this issue is resolved" (p. 28). Incomplete information and innuendo only serve to cloud the ethical issues rather than to clarify them. Furthermore, confounding ethical assumptions regarding a treatment's originator with the empirical findings of independent researchers can lead to erroneous scientific conclusions (Beutler & Harwood, 2001).

One last article (Herbert et al., 2000a, 2000b) requires separate attention since it addresses matters of some import from a scientific standpoint (even regarding the scientific process itself), makes charges of "pseudoscience" in EMDR, and has been published in a major professional journal. In fact, this article serves as an example of the very pseudoscientific practices it decries and the "antiscientific attitudes" described by Beutler and Harwood (2001). This global attempt to malign the reputations of respected scientists and clinicians who support EMDR should not remain unanswered.

Herbert et al. (2000a) stated that "the EMDR Institute, Inc. distributes promotional literature that alleges effective application of this treatment for the distress associated with myriad conditions, including Posttraumatic Stress Disorder (PTSD), Attention-Deficit/Hyperactivity Disorder [ADHD], dissociative disorders, self-esteem issues, and personality pathology (EMDR Institute, 1995, 1997; Festerheim, 1996)" (p. 947). To begin, Fensterheim's work (1996) was in a peer-reviewed professional journal and was not promotional literature from the EMDR Institute. Second, the objection of Herbert et al. (2000a, 2000b) to the use of EMDR across multiple clinical populations raises the issue of certain tensions in the scientist-practitioner model of psychology (Fensterheim, 1994). There is overlap between the research and the clinical areas, but it is not complete. For example, only 12 methods are on the empirically well-established list compiled by Chambless et al. (1998), and none for the diagnoses that Herbert et al. (2000a) mentioned. Therefore, psychological methods applied to these conditions, by definition, are not well established, and to demand that as a treatment standard would require refusing to treat these individuals. The attempt to apply therapeutic techniques to diverse problems is not poor practice if one keeps in mind the empirical issues and employs an appropriate treatment rationale. (This would also include the attempt to treat the social and emotional effects of living with ADHD, but not an attempt to reverse the effects of the biological condition.) When reviewing another diverse list of possible applications for EMDR, Azrin (1996) stated, "Research studies are as yet absent regarding these possible extensions, but

the underlying conceptualization of the process makes the extensions theoretically plausible" (pp. 83–84). This is not to suggest that empirical research is unimportant, but to demonstrate the limitations of current research regarding certain clinical populations.

In the Internet version of this article, Herbert et al. (2000b) claimed that EMDR "has been promoted" to a range of professionals which include "massage therapists, and chiropractors" (p. 8). This is patently inaccurate. A review of the EMDR Institute brochures and the EMDR International Association materials makes clear that licensure in a mental health profession is a prerequisite for EMDR training. The only exceptions are students in licensing tracks who provide written proof of supervision by licensed mental health professionals.

Herbert et al. (2000a) also made an inaccurate statement when they claim that "the original published account of EMDR (Shapiro, 1989[a]), touted this intervention as a single session treatment for the distress associated with the memories in PTSD" (p. 949). This is misleading, as noted earlier. Herbert et al. (2000a) continued by stating, "Such claims are often made on the basis of clinician testimony (workshop training and word-of-mouth) and published case studies" (p. 949). This also involves a false implication. Shapiro's article was the report of a controlled outcome study published in a peer-reviewed journal. Herbert et al. (2000a) then stated, "In the definitive book on EMDR, Shapiro (1995a) presented the extant research in a light favorable for commercial promotion" (p. 954). However, Shapiro (1995) presented all the controlled PTSD research which had been completed at that time and indicated the flaws in the studies (chapter 12). A past president of the Association for the Advancement of Behavior Therapy (Azrin, 1996) describes this text by saying:

A separate section describes the results of the studies, controlled and uncontrolled, conducted to date, and evaluates the conclusions with appropriate and welcomed regard to the methodological features of each. The conceptual basis for the effectiveness of the induced eye movements is discussed in appropriately tentative terms, descriptively rather than persuasively, as a working hypothesis, yet useful in providing a rationale to the patient and guiding the therapists. (p. 83)

Shapiro's (1995) book is a professionally written account of EMDR theory, the method, and the available research at the date of its writing, and "the method is not portrayed as a cure-all" (Azrin, 1996, p. 83).

Herbert et al. (2000a, 2000b) based much of their argument on the work of Popper (1965) and the need for falsifiable hypotheses in scientific research, arguing that EMDR does not provide falsifiable hypotheses but instead retreats to "auxiliary hypotheses . . . to explain away results that would otherwise place the original hypothesis in doubt" (2000a, p. 956). This is simply untrue. For example, besides providing a research summary, chapter 12 of Shapiro's text (1995) supplies multiple falsifiable hypotheses (reiterated from Shapiro, 1991a). Herbert and colleagues (2000a) attempted to bolster their argument by stating that "controlled tests of EMDR showed no effects of eye movements," citing seven studies and claiming that disconfirming experimental results "were explained away by reinterpreting the EMDR technique as a complex method" (p. 956). However, the status of eye movement research summarized previously demonstrates the inaccuracy of representing the research as conclusive in either direction. Second, EMDR did not retreat to auxiliary hypotheses in the face of disconfirming experimental evidence regarding eye movements since the possible role of alternative forms of stimulation was introduced into EMDR clinical practice and documented (e.g., Shapiro, 1991b, 1994) before any of the dismantling studies mentioned by Herbert et al. (2000a, 2000b) were even performed.

Herbert et al. (2000a) stated that pseudosciences typically do not ‘ground (their) doctrines . . . in our scientific heritage’ (Bunge, 1967, p. 36). In other words, pseudosciences tend not to draw or build on existing scientific concepts, but instead purport to create entirely novel paradigms” (p. 957). In making this statement, they ignore varied and continued amplifications of the interface of EMDR with traditional notions of behavioral learning theory and the newer cognitive neurobiological approaches (Armstrong & Vaughan, 1996; De Jongh, Broeke, & Renssen, 1999; Fensterheim, 1996; Levin, Lazrove, & van der Kolk, 1999; Lipke, 1999; MacCulloch, & Feldman, 1996; Rogers et al., 1999; Shapiro, 1989a, 1995, 1999). However, as the research on EMDR and exposure treatments presented earlier indicates, it is clear that EMDR does not follow the definition or procedures outlined for traditional exposure therapies. The actual problem is perhaps best articulated by Lohr et al. (1998) when they stated, “Had EMDR been put forth as simply another variant of extant behavioral treatments, we suspect that much of the controversy concerning its efficacy and use would have been avoided” (p. 150). In failing to acknowledge the research literature that differentiates EMDR from exposure techniques, Lohr et al. (1998) and Herbert et al. (2000a, 2000b) are refusing to accept the implications of hypotheses which fail to support one of their own hypotheses.

Herbert et al. (2000a) continue and (a) portray the demand for treatment fidelity and the appropriate training of researchers as a pseudoscientific strategy of a “degenerating research program” (pp. 956–957), apparently ignoring the call for treatment fidelity by many experts (e.g., Foa & Meadows, 1997; Kazdin, 1998; McNally, 1996) as well as the adequate training of researchers prescribed by professional ethical standards (see Ethical Standards 1.04b, c, & 6.07c, American Psychological Association, 1992, pp. 1600, 1608); (b) imply that a controlled outcome study (Shapiro, 1989a) was part of a “marketing tactic” touting a single session “cure” (p. 958) when in fact “an average treatment time of five sessions” was reported (Shapiro, 1989a, p. 221), the word “cure” was never used, and the study was published in a peer-reviewed journal; (c) disparage independent case reports (e.g., McCann, 1992) published in peer-reviewed journals as mere persuasion tools based upon “vivid appeals” (p. 958); (d) portray as a pre-persuasion tactic they call a “rationalization trap” when the APA mandated use of informed consent is employed with trainees about to undergo a psychotherapeutic procedure (p. 959; see Ethical Standards 1.14 & 4.02, American Psychological Association, 1992, pp. 1601, 1605); and (e) disparage EMDR professional associations and clinical terminology as indicative of a “granfalloon” while failing to acknowledge that other professional organizations such as the Association for the Advancement of Behavior Therapy, International Association for Cognitive Psychotherapy, and the American Psychological Association clearly conduct themselves in the same manner that they are describing (pp. 959–960; e.g., shared or specialized terminology, shared beliefs or assumptions, purchased memberships, and membership benefits).

A final pseudoscientific strategy according to Herbert et al. (2000a, 2000b) is to devalue skepticism. They stated “that skeptics are often attacked by pseudoscientists through innuendo and character assassination rather than reasoned argumentation. In this way, the debate is quickly removed from the theoretical and empirical issues at hand . . . and instead moves to [the] personal arena of ad hominem assault” (2000a, p. 960). We agree that innuendo and ad hominem attacks hinder scientific investigation and urge that further discourse on both sides of the question be elevated above that level.

## Conclusions

Although the research regarding the necessity of the eye movement component is currently inconclusive, EMDR is a psychological treatment for PTSD which has received con-

siderable empirical validation (Carlson et al., 1998; Marcus et al., 1997; Rothbaum, 1997; Scheck et al., 1998; Wilson et al., 1995). However, despite the empirical validation, confusion still exists in the literature regarding EMDR. Some of the confusion is theoretical and due to the current lack of empirical validation of Shapiro's (1991a, 1995) information processing model and the continued inability of other models (e.g., exposure) to convincingly explain EMDR methods and effects. Other sources of confusion stem from methodological difficulties in the empirical research and inaccurate research reviews.

Readers of research reviews will find it helpful to address the original research to ensure accurate and complete reporting. This means reviewing the studies cited in any given source as well as being familiar with the general body of literature that is currently available. Readers also will find it beneficial to have a working knowledge of research design to assess the construction of empirical studies and the generalizability of their findings. Finally, it is useful to know the EMDR method to assess the fidelity issues in empirical research studies. This unfortunate situation, in part, appears to be necessitated because of multiple biased and inaccurate reports of empirical research and historical data authored by a relatively small group of individuals (e.g., Herbert et al., 2000a, 2000b; McNally, 1999a) who have coauthored multiple reviews (e.g., Lohr et al., 1999; Lohr et al., 1998; Rosen et al., 1998; Rosen et al., 1998a, 1998b).

A prominent characteristic of the literature cited in this article is the contrast between the positive and the negative reactions to novelty. In particular, there appears to have been a considerable reaction both for and against the novelty of EMDR's effects, theoretical model, and method (especially the eye movements). An example is MacCulloch and Feldman's (1996) enthusiasm regarding EMDR's "powerful relaxing (de-arousing) reflex" (p. 577) on one hand and, on the other hand, Herbert et al.'s (2000a) response to the same reported rapidity of effect as a "phantom" constructed in the service of a "marketing tactic" (p. 958). It is even more striking when one realizes that Herbert et al. (2000a, 2000b) cite MacCulloch and Feldman (1996) for theoretical support of their position.

The extant body of knowledge in science tends to be self-defining because it largely determines the research questions we ask, the methods we employ, and that which we will accept as truth. As knowledge increases, we build traditions of knowing, orientations toward truth, scientific theories, and paradigms that then shape our questions and mold our perceptions of our experience. We then perceive and select new information for incorporation based on its fit with extant knowledge in the accepted tradition. One consequence of this process is that knowledge becomes self-limiting and resistant to ideas and evidence that are radically different from that which is predicted by the fundamental ideas governing the prevailing epistemological system or theory. The process of science has been traditionally conceived as requiring falsifiability (Popper, 1965), but human beings generally find it uncomfortable when their belief systems are challenged (Fisch, 1965). As a result, when new information is introduced which is discrepant with the extant body of knowledge and the prevailing theoretical framework, an "essential tension" is created between the two (Kuhn, 1959/1977), and "very often the successful scientist must simultaneously display the characteristics of the traditionalist and of the iconoclast" (p. 227).

For the process of science to go forward through hypothesis formation and falsification, we must remain in the essential tension between established ways of thinking and the openness to new or conflicting information when it presents itself. Opting out of the tension with an iconoclastic indifference to tradition will stop the process of scientific investigation, as will an overconcern with tradition and a disregard of new and conflicting information. We must not allow this tension to degenerate from scientific debate into ad hominem attacks. Scientific inquiry must proceed with respectful discourse and the clear sharing of ideas and empirical data. We encourage researchers and scientist-

practitioners of all theoretical orientations to remain in this tension and to carry the process of scientific investigation forward. The pursuit of truth is seldom comfortable.

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