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Efficacy of Eye Movement Desensitization and Reprocessing: Implications for Behavior Therapy

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The commitment of behavior therapy to empiricism has led it to a prominent position in the development of validated methods of treatment. The recent development and rapid expansion of Eye Movement Desensitization and Reprocessing (EMDR), a treatment that bears a resemblance to behavioral techniques and that has been proposed as an alternative to such techniques for numerous psychological disorders, raises important questions for the field of behavior therapy. In this article, we examine 17 recent studies on the effectiveness of EMDR and the conceptual analysis of its mechanisms of action. The research we review shows that (a) the effects of EMDR are limited largely or entirely to verbal report indices, (b) eye movements appear to be unnecessary for improvement, and (c) reported effects are consistent with non-specific procedural artifacts. Moreover, the conceptual analysis of EMDR is inconsistent with scientific findings concerning the role of eye movements. Implications of the empirical and theoretical literature on EMDR for behavior therapy are discussed.

The adherence to methodological rigor in the empirical validation of assessment and intervention procedures has distinguished behavior therapy since its inception nearly 4 decades ago. The benefits of that methodological rigor have recently been recognized by the report of the Division of Clinical Psychology (12) of the American Psychological Association on validated treatments (Chambless, 1995) in which behavioral and cognitive-behavioral treatments are among those that received consistent support. The same

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methodological rigor has served to limit the clinical and market promotion of techniques prior to their validation. In one of the most extreme cases, experimental methods have been used to demonstrate the invalidity of a procedure, Facilitated Communication, which was widely promoted as a marked improvement over behavioral procedures in the treatment of autism and developmental disabilities (Delmolino & Romanczyk, 1995; J. W. Jacobson, Mulick, & Schwartz, 1995).

Although adherence to methodological behaviorism (Craighead, Kazdin, & Mahoney, 1981) has served to limit the inappropriate promotion of clinical procedures, market pressures have gained greater force in recent years (Cone, Alexander, Lichtzajn, & Mason, 1996; Strosahl, 1994, 1995). As a consequence, behavior therapy may find itself involved in the premature popularization of prescriptive, structured, time-limited procedures that bear a resemblance to cognitive behavioral interventions. According to some authors (e.g., Acierno, Hersen, Van Hasselt, Tremont, & Mueser, 1994), the clearest current example of this involvement is EMDR.

The dissemination of the clinical procedure of EMDR (F. Shapiro, 1995) has been remarkably successful. EMDR Institute (1997) reports that over 22,000 licensed clinicians have been trained since the initial published account of its application (F. Shapiro, 1989). Moreover, this technique has been applied to a wide variety of conditions in clinical and research contexts. These include conditions ranging from Posttraumatic Stress Disorder (PTSD) and Attention-Deficit/Hyperactivity Disorder to dissociative disorders and self-esteem issues (EMDR Institute, 1995, 1997). Among the claims made regarding EMDR's clinical efficacy are its purported rapidity, permanence, range of applicability, and superior efficacy relative to extant treatments (F. Shapiro, 1995, 1996a; F. Shapiro & Forest, 1997).

In this article, we review research addressing the efficacy of EMDR and the way in which the data address its putative mechanisms of change. We place particular emphasis on the question of whether the efficacy of EMDR exceeds that of extant treatments or of nonspecific (placebo) conditions. As Klein (1996a, 1996b) noted, the onus lies with the proponents of psychological interventions to demonstrate that their interventions are more efficacious than nonspecific manipulations (Grünbaum, 1985). The theoretical analysis used to explain EMDR's mechanism of change and justify EMDR's treatment procedures will then be evaluated. Finally, the implications of our findings and analysis for behavior therapy will be discussed.

Controlled Validation Research

The research in this review comprises 17 studies that have been published (or are in press) and presented at behaviorally oriented professional meetings since a methodological review by Lohr, Kleinknecht, Tolin, and Barrett (1995) was conducted. Since Lohr, Kleinknecht, et al., the quantity and methodological rigor of research reports on EMDR have dramatically

increased. These studies are all group-design experiments investigating the outcome and process of EMDR treatment. The studies included in this review have employed improved procedural controls and have been applied across a considerably broader range of clinical problems than those examined in previous reviews (Acierno, Hersen, et al., 1994; DeBell & Jones, 1997; Herbert & Mueser, 1992; Lohr et al., 1992; Lohr, Kleinknecht, et al.). The design improvements have been in the control for the nonspecific effects of treatment and the functional significance of eye movements as an "active ingredient" of EMDR. Consequently, this review addresses in considerably more detail than previous reviews the question of whether EMDR's effects exceed those of nonspecific treatments.

The current review follows earlier evaluations of EMDR that were published by behavioral researchers soon after the publication of the initial experimental outcome study (F. Shapiro, 1989) and a large number of uncontrolled case studies (Herbert & Mueser, 1992; Lohr et al., 1992). Subsequent reviews of experimental outcome studies (Acierno, Hersen, et al., 1994; Lohr, Kleinknecht, et al., 1995) noted the improvement in objective measurement of outcome variables and subject selection procedures. Procedural controls for nonspecific effects and essential components of treatment, however, were limited. Thus, Lohr, Kleinknecht, et al., concluded that although the EMDR treatment protocol frequently reduced verbal report and observer ratings of distress, psychophysiologic and motoric indices showed little effect of treatment, and were not assessed in most studies. Moreover, no studies adequately controlled for nonspecific effects, no substantive comparisons had been made with other treatments, and the findings of a few studies suggested that eye movements were not an essential component of treatment. The weight of the results led to the conclusion that EMDR's popularity among clinicians was not justified by the data. Similar conclusions have been arrived at independently (Acierno, Hersen, et al.) and others have expressed caution regarding the widespread adoption of EMDR based on the research evidence (DeBell & Jones, 1997).

The popularity of EMDR among mental health professionals is due, at least in part, to its proponents' assertions, including rapidity, permanence, and generality of its effects (F. Shapiro, 1989, 1995, 1996a; F. Shapiro & Forest, 1997) for a wide variety of disorders, including PTSD and related traumatic memories (EMDR Institute, 1995, 1997). Such claims are often made on the basis of clinician testimony (e.g., workshop training) and uncontrolled case studies (J. G. Carlson, Rusnak, Chemtob, & Hedlund, 1996; Goldstein & Feske, 1994; Kleinknecht & Morgan, 1992; Marquis, 1991; McCann, 1992). These claims include assertions that EMDR can cure PTSD in one treatment session (F. Shapiro, 1989) and that EMDR is substantially more efficacious than existing treatments for PTSD (F. Shapiro, 1995, 1996a). Nevertheless, as the philosopher David Hume (1748/1977) noted, extraordinary claims require extraordinary evidence (p. 75). In this review, we examine the evidence that addresses the following question: Is the evi-

dence for EMDR's efficacy commensurate with the extremely strong assertions that have been made for it (F. Shapiro, 1995, 1996a; F. Shapiro & Forest, 1997)?

The empirical criteria for psychosocial treatment efficacy of PTSD were recently elucidated by Foa and Meadows (1997), and can be extended to other disorders treated by EMDR. Their criteria include clearly defined target symptoms, reliable and valid measures of the symptoms, use of blind assessors in applying the measures, training of assessors in applying the measures, specific (manualized) treatment procedures, unbiased assignment to treatment conditions, and adherence to treatment protocol (fidelity). To Foa and Meadows' criteria we add the necessity of comparing EMDR with treatment conditions that control for the nonspecific effects of treatment (Bootzin, 1985; Borkovec, 1985; Kazdin & Wilcoxon, 1976; Keane, in press). These are the same types of controls that were used in the empirical validation of systematic desensitization (Davison, 1968; Lang & Lazovik, 1963; Lang, Lazovik, & Reynolds, 1965; Paul, 1966). It should be noted that some proponents of EMDR (Greenwald, 1997; Rogers, 1996) have claimed that this procedure has been held to higher standards of validation than other treatments for the same conditions. We argue, as have others (Foa & Meadows; Keane), that the standards for the validation of EMDR should be the same as for other procedures. Consequently, this review will apply the procedural standards proposed by Foa and Meadows to evaluate PTSD treatments in general, as well as standards concerning experimental controls for nonspecific and essential components, to the most recent research on EMDR.

Behavioral research has used a number of strategies to control for nonspecific effects (Borkovec, Kaloupek, & Slama, 1975; Mahoney, 1978; O'Leary & Borkovec, 1978). One tactic is the use of additive or subtractive experimental strategies to identify the functional significance of specific aspects of treatment procedures (Nezu, 1986; Nezu & Perri, 1989; Rehm et al., 1982). These manipulations attempt to identify the active ingredients of a treatment by either introducing or removing specific components of an intervention. Thus, control conditions for the complete EMDR protocol include designs that substitute eye movements with finger tapping, auditory stimulation, and no stimulation.

Several studies have adopted this tactic in evaluating the effect of EMDR on traumatic memories, PTSD symptoms, panic disorder, public speaking fear, test anxiety, spider phobia, and various anxiety disorders. Because EMDR was originally applied to the treatment of traumatic memories and PTSD symptoms, we begin our review with recent studies examining the effect of EMDR on these conditions, and then extended the review to other conditions. A summary of the findings is presented in Table 1.

Traumatic Memories

S. A. Wilson, Becker, and Tinker (1995) randomly assigned 80 participants with a history of trauma to either EMDR or delayed treatment. Forty-

TABLE 1

SUMMARY DESCRIPTION OF PROCEDURES AND FINDINGS OF TREATMENT OUTCOME STUDIES ON EMDR

Clinical Problem	Authors	Trained in EMDR?	Results of Comparisons Between EMDR and Control Conditions					
			Comparison Condition(s)	SUD	VoC	Standardized Measures	Psychophysiologic Data	Behavioral Assessment
PTSD	Rothbaum (1995)	Trained	No treatment	–	–	Yes	–	–
	Boudewyns & Hyer (1996)	Trained	Closed eyes	No	No	No	No	–
			No additional treatment	Yes	Yes	Yes	Yes	–
	Devilly et al. (in press)	Trained	Flashing light	No	–	No	No	–
			No additional treatment	–	–	No	–	–
	Pitman et al. (1996)	Trained	Finger tapping	No	–	No	No	–
Traumatic memories	Wilson et al. (1995)	Trained	No treatment	Yes	–	Yes	–	–
	Hazlett-Stevens et al. (1996)	Trained	Stationary eyes	No	No	No	–	No
			Non-direction	Yes	No	No	–	No
	Dunn et al. (1996)	Trained	Stationary eyes	Yes	–	–	No	–
	Scheck et al. (in press)	Trained	Active listening	–	–	Yes	–	–
Various anxiety disorders	Wilson et al. (1996)	Trained	Stationary eyes	Yes	Yes	–	Possible	–
			Finger tapping	Yes	Yes	–	Possible	–
Panic disorder	Feske & Goldstein (1997)	Trained	Stationary eyes	–	–	Possible (2 of 5)	–	–
			No treatment	–	–	Yes	–	–
Public speaking anxiety	Foley & Spates (1995)	Trained	Lateral auditory stimulation	No	No	No	–	No
			Stationary eyes	No	No	No	–	No
			No treatment	Yes	Yes	No	–	No
Test anxiety	Gosselin & Mathews (1995)	Trained	Stationary eyes	Yes	No	No	–	–
Spider phobia	Bates et al. (1996)	Not Trained	No treatment	Yes	Yes	–	No	No
	Muris & Merckelbach (1997)	Trained	*Imaginal exposure*	–	–	No	–	No
			In-vivo exposure	–	–	No	–	No
	Muris et al. (1997)	Trained	In-vivo exposure	–	–	No	No	No
	Muris et al. (in press)	Trained	In-vivo exposure	–	–	No	–	No
			Computer control	–	–	Possible (1 of 4)	–	No

No = EMDR not superior to control condition, yes = EMDR superior to control condition, possible = EMDR appears superior but critical statistical comparisons not made, – = not obtained.

six percent met the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-IV*; American Psychiatric Association, 1994) criteria for PTSD using the Posttraumatic Stress Disorder Interview (PTSD-I; Watson, Juba, Manifold, Kucala, & Anderson, 1991). Compared with participants in the delayed treatment condition, participants who received EMDR demonstrated significant improvement on several dependent measures, including Subjective Units of Discomfort (SUD) ratings, the Symptom-Checklist-90-R (SCL-90-R; Derogatis, 1992) Anxiety, Somatization, Depression, and Interpersonal Sensitivity scales, the State form of the State-Trait Anxiety Inventory (STAI; Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983), and the Impact of Event Scale (IOES; Horowitz, Wilner, & Alvarez, 1979), an index of avoidance behavior and intrusiveness of traumatic memories. Similar effects were noted in the delayed treatment condition following administration of EMDR. In addition, gains on a number of these measures were maintained at 90-day follow-up. Methodological strengths of S. A. Wilson et al. include the use of independent assessors to measure anxiety symptoms, standardized assessment of treatment fidelity, and computation of treatment effect size indices.

Although S. A. Wilson et al.'s (1995) study has been identified by some (e.g., F. Shapiro, 1995) as providing definitive support for the efficacy of EMDR, several limitations concerning their design and analyses should be noted. First, all of the dependent measures collected by S. A. Wilson et al. were based upon self-report. S. A. Wilson et al.'s independent assessors only collected SUD ratings* and administered questionnaires, and did not collect either structured or semi-structured interview data. Confidence in their results would be strengthened by the inclusion of overt behavioral measures, such as improved sleep pattern (Lichstein & Johnson, 1991; Lichstein & Riedel, 1994). The inclusion of psychophysiologic measures (Boudewyns & Hyer, 1996; Boudewyns, Stwertka, Hyer, Albrecht, & Sperr, 1993) and social validation procedures (Kazdin, 1977), such as ratings by significant others, would have also strengthened confidence in their findings. The inclusion of the tripartite assessment strategy (Acierno, Tremont, Last, & Montgomery, 1994) would have provided a test for claims of generality of treatment effects (F. Shapiro, 1995, 1996a; F. Shapiro & Forest, 1997).

Second, as the authors (S. A. Wilson et al., 1995) themselves note, the PTSD outcome measure was modified to increase its sensitivity to symptomatic change. Because this alteration produced a significant decrease in the number of individuals diagnosed with PTSD in the control condition fol-

* It should be noted that SUD ratings are technically a process rather than an outcome measure, and that they are collected within, rather than outside, treatment. As a consequence, they may be more susceptible to demand characteristics and procedural artifacts (i.e., they may be more reactive) than most outcome measures (Lohr et al., 1992; Lohr, Tolin, & Kleinknecht, 1995). This possibility should be borne in mind when interpreting the results of other studies that employ SUD ratings as a dependent measure. See Lohr et al. (1992, p. 193) for a discussion of these measurement issues.

lowing the treatment phase, and because no other dependent measure exhibited a comparable decrease, "it was concluded that the revised wording invalidated the scale as a PTSD diagnostic instrument" (p. 391). Thus, contrary to the claims of F. Shapiro (1995, p. 331), S. A. Wilson et al. do not provide information on remission rates of PTSD, although the decreases they reported on the IOES are consistent with an effect on PTSD symptoms.

Third, because the delayed treatment group initially received no intervention, the findings are potentially attributable to nonspecific factors (Bootzin, 1985; Evans, 1985). S. A. Wilson et al. (1995) included no procedural control condition that would have manipulated nonspecific effects or isolated the essential ingredients of EMDR (Bates, McGlynn, Montgomery, & Mattke, 1996; Foley & Spates, 1995; Gosselin & Matthews, 1995). Foa (1996) argued that S. A. Wilson et al.'s study is "far from conclusive" because subjects in the delayed treatment were aware that they first received no treatment, thereby introducing the potential for measurement bias. S. A. Wilson et al. dismissed these procedural issues by appealing to large effect sizes for EMDR in comparison to other treatment conditions. We agree with N. Jacobson (1996), who concluded that the "effect size argument" for the validity of EMDR is unjustified. Conclusions about specific effects of EMDR are interpretable only through experimental comparison with nonspecific interventions (Klein, 1996a, 1996b). It is worth noting that the mean effect size for EMDR (d = .90) reported by S. A. Wilson et al. is equivalent to the mean effect size reported by Otto, Penava, Pollock, and Smoller (1996) in their meta-analysis of (non-EMDR) exposure treatments for PTSD, calling into question F. Shapiro's (1996a) claim that EMDR is markedly more efficacious than extant treatments.

Scheck, Schaeffer, and Gillette (in press) recruited 60 women, from community sources, between the ages of 16 and 25, who had reported traumatic memories associated with past experiences. Ninety percent reported physical or emotional abuse as a child, and more than 50% reported rape or child molestation. Seventy-seven percent met all of the *DSM-IV* criteria for PTSD using the PTSD-I (Watson et al., 1991). Independent ratings of Criteria A (traumatic event) showed 92% agreement using Cohen's Kappa. Subjects were randomly assigned to two sessions of either EMDR or an Active Listening (AL) treatment modeled after Rogerian therapy (Gordon, 1974). EMDR was administered by 13 therapists who had received Level I and Level II training. Level I training involves the basic EMDR procedure, while Level II training is intended to provide for the application of specific treatment "protocols" for specific problems (see Rosen, 1996). AL was administered by 11 different therapists who reported "extensive experience with the paradigm." All therapists held graduate degrees in psychology, social work, marriage and family therapy, or professional counseling.

Outcome measures were the Beck Depression Inventory (BDI), STAI-State, Penn Inventory for Posttraumatic Stress Disorder (Hammarberg, 1992), IOES, and the Tennessee Self-Concept Scale (Roid & Fitts, 1991). Analyses

of variance revealed significant interactions between group and pre-post assessment for all outcome variables. Alpha-protected simple main effects tests showed statistically significant improvement on all measures for both treatment conditions. The analyses showed that at posttreatment, the EMDR group was different from the AL group on four of five measures. Clinical change was assessed by comparing posttreatment means with means derived from normative comparison groups (Kendall & Grove, 1988). The EMDR group showed posttreatment means that were more comparable to normative means than did the AL group. There was no assessment with the PTSD-I following treatment. The authors concluded that EMDR produced more improvement than the nonspecific effects of supportive and empathic AL.

Although the AL condition may have provided for the assessment of nonspecific treatment effects, this comparison is obscured by the therapist by treatment procedure confound. Not all therapists provided both treatments, and such factors as therapist allegiance, enthusiasm, or involvement could have contributed to the measured effects of EMDR (see a discussion of such confounds below). It should also be noted that only verbal report measures were used and the relative effects of treatment upon psychophysiologic measures, behavioral measures, or both, is unknown. Moreover, any genuine effects of treatment were limited to collateral features of PTSD. Not all subjects met all of the criteria for PTSD, and a measure of PTSD per se was not used as an outcome measure. The same conclusions regarding the PTSD-specific effects of EMDR treatment apply to S. A. Wilson et al. (1995): in that study only 46% of the participants met the criteria for PTSD before treatment, and no assessment of PTSD diagnostic status was conducted following treatment. Finally, because the AL condition provided no explicit exposure to traumatic imagery, Scheck et al.'s (in press) findings do not exclude the possibility that the effects of EMDR are mediated entirely by exposure. The last issue is crucial in light of the claim that EMDR is not simply a variant of behavioral exposure treatments (F. Shapiro, 1995, pp. 19–27).

Hazlett-Stevens, Lytle, and Borkovec (1996) identified college students with high IOES scores for a traumatic memory, but excluded those who met the criteria for PTSD. The decision to exclude these subjects was based on the assumption that the single session treatment required by the experimental design might not result in sufficient reduction of severe distress and could produce harmful results. Forty-five subjects were randomly assigned to one of three treatment conditions: (1) Eye Movement Desensitization as described by F. Shapiro (1989), (2) an identical procedure that employed eye fixation, or (3) non-directive counseling. Like the procedure used by Scheck et al. (in press), the non-directive counseling condition provides a close approximation to a nonspecific treatment control condition. Pre-treatment assessments were obtained in the first session and were followed by the treatments 1 week later. Post-treatment assessments were obtained 1 week later. The results showed that SUD ratings were significantly lower in the EMDR group than in the non-directive group, but did not differ from the fixed-eyes

group. There were no significant differences among groups on IOES scores, a behavioral measure of cognitive intrusions, or Validity of Cognitions (VoC) ratings. A VoC rating is a measure, introduced by F. Shapiro (1989), that assesses the extent to which the client affirms belief in a positive self-statement referring to the emotional image, such as "I did all that I could do." Thus, the results show that the non-directive counseling condition produced the same effects as EMDR on three out of four measures. Unlike Scheck et al., the findings of Hazlett-Stevens et al. suggest that EMDR is not substantially more efficacious than nonspecific treatments.

Dunn, Schwartz, Hatfield, and Weigele (1996) screened college students by identifying those who reported a SUD rating of 7 and an associated physiologic reaction to the trauma memory. Subjects who scored above 2 standard deviations on the Dissociative Experiences Scale (DES; E. B. Carlson & Putnam, 1993) and above 1 standard deviation on the IOES were excluded, because they were deemed too severely distressed for research purposes, and were referred for treatment (T. M. Dunn, personal communication, March 15, 1997). Twenty-eight of the remaining subjects who scored in the "severe" range on the IOES were randomly assigned to either EMDR or to an otherwise identical procedure that used eye-fixation. The amount of imagery exposure was controlled by yoking one subject in the EMDR condition to one subject in the eye-fixation condition. Outcome measures were SUD ratings, frontalis EMG, finger skin temperature, heart rate (HR), and Skin Conductance Level (SCL) taken before treatment and 1 minute after treatment. The results showed a statistically significant reduction in SUD ratings in both conditions, but no difference between conditions. The authors reported that HR and SCL showed no statistically significant differences between groups, but that there were statistically significant reductions in HR within both groups from pre- to post-treatment. The results also show a significant reduction in SCL in the control condition (although their tabled results incorrectly indicate that this difference was nonsignificant; T. M. Dunn, personal communication, January 24, 1997). Thus, only one of four psychophysiologic indices showed change following EMDR and two of four showed change following the control condition. Although the exclusion of more severely distressed individuals from Dunn et al. and Hazlett-Stevens et al. (1996) may limit the generalizability of their findings, the random and unbiased assignment of subjects to treatment conditions should preserve the comparability of treatment groups regarding differential treatment effects.

Henry (1996) treated the traumatic memories of a non-randomly selected sample of pathological gamblers who were receiving psychotherapy. The group that concurrently received EMDR reported less gambling following psychotherapy than the group that received only psychotherapy. The adjunctive nature of the EMDR procedure, along with the procedural limitations of group assignment and uncorroborated verbal report, make it impossible to interpret the validity of the separate or additive effects of EMDR.

In summary, Table 1 suggests that the effects of EMDR are limited to

verbal report measures of distress and are most pronounced when compared with conditions involving no treatment. Nevertheless, when compared with an alternative treatment, such as a stationary eyes procedure or non-directive counseling, the effects of the treatment appear to be less pronounced and, perhaps, negligible.

PTSD

Boudewyns and Hyer (1996) compared EMDR with an imagery exposure control (EC) and a no-imagery control (C) procedure in the treatment of combat-related PTSD. All subjects received eight sessions of the standard inpatient or outpatient PTSD treatment program at a Department of Veterans Affairs hospital. Subjects in the EMDR group received between five and eight sessions of EMDR. Subjects in the EC group received the same number of sessions. The EC subjects did not engage in eye movements during individual treatment but kept their eyes closed and engaged in imaginal exposure for the same period. C subjects received only the standard group treatment. Outcome measures included the Clinician Administered PTSD Scale (CAPS), IOES, Profile of Mood States-Anxiety (POMS), SUD, and HR in response to a tape-recorded script of the subject's most disturbing memory. All measures were obtained before and after treatment by a clinician blind to experimental condition. Mixed-model 3 × 2 analyses of variance were conducted on the CAPS, IOES, and POMS. Analysis of covariance, using pretreatment heart rate as a covariate, was applied to posttreatment heart rate data. The analyses revealed that the EMDR and the EC conditions showed greater change than the C condition on SUDS, POMS-Anxiety, and HR, but that the EMDR and EC conditions did not differ from one another. In addition, the analyses indicated that the three groups showed equal change on the CAPS, and all groups showed no significant change on the IOES. Thus, it appears that neither eye movements nor any lateral (side to side) stimulation were necessary for measured change. These results call into question F. Shapiro's (1995) claim that some form of lateral stimulation is a necessary ingredient in EMDR's effectiveness. In addition, these results suggest that imagery exposure may be sufficient for change on some indices of PTSD (HR, SUD, POMS-Anxiety) but not others (CAPS). These null effects for EMDR *per se* are consistent with those of Boudewyns et al. (1993) and Jensen (1994).

Devilly, Spence, and Rapee (in press) compared EMDR with a no eye movement control condition presented to subjects as "reactive eye dilation desensitization and reprocessing" among combat-related PTSD patients. The control condition involved the same EMDR protocol except that a flashing light was substituted for lateral eye movements. Both treatments were compared with a psychiatric support condition that included the same assessment battery as the treatment conditions. Equal numbers of subjects in the treatment conditions had received or were concurrently receiving psychological services from community or governmental agencies. Treatment efficacy mea-

sures included standardized anxiety, depression, and PTSD scales, as well as pulse rate (PR) and blood pressure (BP). The results showed that both treatment groups improved by posttreatment, but that there was no difference between the two conditions. Subjects in the two conditions did not differ from the control condition on standardized measures, but did improve more than the control condition when the reliability change index on the Mississippi-PTSD scale was used as a measure of improvement. There were no differences between groups on PR and BP. At 6-month follow-up, the SUD ratings on a questionnaire assessing personal problems showed reductions as a function of time, but no differential effect for treatment groups. The standardized measures showed no statistically significant effects for any variable. Clinical improvement on the standardized PTSD scale was shown by only 3 of 9 subjects in each condition. The authors concluded that eye movements are not the mechanism of change, and that other nonspecific effects are responsible for the small amount of change shown following treatment.

Pitman et al. (1996) used a cross-over design in which combat-related PTSD patients were randomly assigned to one of two treatment sequences using EMDR or a control treatment. The control procedure consisted of all the EMDR components, including movement of the therapist's hand. The subject, however, maintained eye fixation and tapped one finger to correspond to therapist hand movement. Each treatment was applied to a separate traumatic memory, and each was applied for a maximum of six sessions once per week. All therapists obtained complete EMDR training, and both treatments were observed by an EMDR expert and rated for treatment integrity. Treatment process variables included SUD ratings and four psychophysiologic indices: HR, skin conductance (SC), and two electromyographic measures. Treatment outcome measures included IOES-Intrusion and Avoidance scores on the two images, Mississippi-PTSD scores, SCL-90-R scores, CAPS, and a cued intrusive thought log. Paired *t*-test comparisons showed that SUD scores significantly decreased (habituated) in both treatment conditions within and across treatment. Analyses showed that psychophysiologic indices were significantly reduced in 9 of 16 comparisons (56.0%). In the movement condition, psychophysiologic indices were significantly reduced in 62.5% of the comparisons, and in the fixed condition they were significantly reduced in 50.0% of the comparisons. Analyses of variance between treatment conditions showed no statistically significant differences between treatment conditions on any process variable. Paired *t*-test comparisons showed that both the EMDR and the control procedure resulted in the reduction of 3 of 8 process variables. A comparison of the treatment conditions showed that the control procedure was superior to the EMDR procedure on IOES-Avoidance scores. There was one significant positive correlation between process and outcome variables in the control procedure and no significant correlations in the EMDR procedure. In both treatment conditions, there were 2 of 6 significant positive correlations between treatment integrity ratings and outcome measures (R. K. Pitman, personal communication,

October, 21, 1996). The average correlations between integrity and outcome ratings were relatively low in both the EMDR (r = .23) and the control (r = .42) conditions. Thus, the authors concluded that there was "partial emotional processing" during the treatment sequence on SUD ratings and some psychophysiological measures. The results, however, showed that on outcome variables, there was limited change (3 of 8 measures) within each of the procedures. Moreover, the analyses indicated little difference between the two procedures except for the possible superiority of the control procedure on IOES scores. The overall findings of this study show that only SUD ratings decreased following both procedures. The improvement resulting from the control procedure suggests that eye movements confer no clear advantage over other forms of stimulation. This finding is consistent with those of other studies providing procedural controls (Boudewyns & Hyer, 1996; Devilly et al., in press; Feske & Goldstein, 1997; Foley & Spates, 1995; Gosselin & Matthews, 1995). The absence of a wait-list (or baseline) control condition, however, makes it difficult to determine the contribution of nonspecific effects in the fixed eye control procedure.

Rothbaum (1995) reported that EMDR reduced rape-related PTSD symptoms more than a wait-list, measurement-only condition. Standardized self-report and interview instruments were administered by evaluators who were unaware of subjects' assignment to experimental conditions. Although this study employed all of the procedures specified by Foa and Meadows (1997), it did not employ other procedural control conditions (e.g., control for exposure or other nonspecific effects). In the absence of such controls, the change in reported symptoms, like that of similar studies on PTSD (Boudewyns et al., 1993; F. Shapiro, 1989; S. A. Wilson et al., 1995), is potentially attributable to nonspecific effects.

Of these studies and several others (Jensen, 1994; Renfrey & Spates, 1994; F. Shapiro, 1989; Silver, Brooks, & Obenchain, 1995; Vaughan, Armstrong, Gold, O'Connor, Jenneke, & Tarrier, 1994) reviewed by Foa and Meadows (1997), only Pitman et al. (1996) and Rothbaum (1995) approach methodological adequacy in terms of their standardized criteria. As a result, Foa and Meadows concluded: "Many studies failed to demonstrate efficacy of EMDR. Some found improvement, but methodological flaws rendered most though not all of these findings uninterpretable. The test of this much-discussed treatment awaits adequately controlled studies" (pp. 469–470). Keane (in press) has drawn essentially the same conclusions (pp. 13–15).

We have reviewed four studies on PTSD and traumatic memories (Devilly et al., in press; Dunn et al., 1996; Hazlett-Stevens, et al., 1996; Scheck et al., in press) not included in Foa and Meadows (1997). Although all of these studies meet at least half of their criteria, they do not attain the procedural rigor of Pitman et al. (1996) or Rothbaum (1995). Nevertheless, each contains an eye movement control procedure, and Hazlett-Stevens et al. and Scheck et al. contain a nonspecific treatment control condition. These studies show that EMDR did not differ from any of the movement control conditions on

standardized, behavioral, or psychophysiologic measures. Differences were observed only with SUD ratings (Dunn et al.; Hazlett-Stevens et al.). Pitman et al. reported parallel findings, and Rothbaum showed only that EMDR was superior to a no-treatment control condition. The findings of Scheck et al. suggest an apparent superiority of EMDR over a non-specific treatment. However, procedural confounds and a limited range of dependent variables cast doubt on the validity of comparing this control condition with EMDR.

In applying their methodological criteria to the research on psychosocial treatments on PTSD, Foa and Meadows (1997) concluded that the behavioral procedures of prolonged exposure and stress inoculation training have been shown to be effective in reducing symptoms of PTSD. The same conclusions are drawn by Keane (in press). These conclusions can be contrasted with those of F. Shapiro (1996a): "Compared to flooding and stress inoculation therapy, which have been afforded the status of 'probable efficacy' by the APA Task Force (Chambless et al., 1996), the literature on EMDR already substantially exceeds the evidence cited for those methods" (p. 216).

In summary, the recent findings on the effect of EMDR for PTSD are much the same as for traumatic memories. Table 1 suggests that any effects of EMDR are limited to verbal report measures of distress and are most pronounced when compared with no treatment. Nevertheless, when compared with control procedures that control for nonspecific effects (closed eye imagery, blinking light, or finger tapping), the comparative benefits of EMDR have not been demonstrated. Furthermore, we know of no direct comparisons between EMDR and either flooding or stress inoculation therapy.

Other Anxiety Disorders

EMDR has also been used extensively with clinical conditions that involve anxiety but do not involve explicit traumatic etiology or traumatic imagery. These conditions include panic disorder, specific phobia, public speaking fear, test anxiety, and mixed anxiety disorders.

Feske and Goldstein (1997) compared EMDR with stationary eye and wait list control conditions in the treatment of diagnosed panic disorder. Treatment integrity was addressed through the use of a treatment manual and integrity checklist that were reviewed by Shapiro and rated "midway between acceptable and high quality." Outcome was assessed with the Agoraphobia Cognitions Questionnaire, Body Sensations Questionnaire, Panic Appraisal Inventory, Mobility Inventory for Agoraphobia, Beck Anxiety Inventory, and self-report of panic frequency. Questionnaire data were factor-analyzed into four composite scales: social concerns/generalized anxiety, agoraphobia/panic/coping, physical concerns, and generalized anxiety/panic fear.

Compared with the wait list condition, the EMDR condition showed greater pretreatment to posttreatment improvement on panic frequency and three of the four composite scores. Compared with the stationary eye condition, EMDR showed greater improvement on only two (social concerns/general anxiety and general anxiety/panic fear) of the five measures. At

3-month follow-up, there were no differences on any of the outcome variables. The stationary eye condition was not compared with the wait list condition. Thus, the data suggest that although EMDR may be more effective than no treatment in reducing self-reported panic symptoms, it is no better than the eye movement control condition. The findings of this study are consistent with others using procedural control conditions (e.g., Boudewyns & Hyer, 1996; Devilly et al., 1995; Dunn et al., 1996; Hazlett-Stevens et al. 1996; Foley & Spates, 1995; Gosselin & Matthews, 1995; Pitman et al., 1996; Renfrey & Spates, 1994).

Muris and Merckelbach (1997) compared EMDR with a 10-minute imaginal exposure procedure and an assessment-only control condition in the treatment of spider phobia. Although in-vivo exposure is generally regarded as the treatment of choice for animal phobia (Hellström & Öst, 1995), imaginal exposure provides a more direct control for the effects of EMDR, which relies extensively on the use of imagery. All procedures were followed by in-vivo exposure. EMDR and imaginal exposure were carried out by the same therapist, and in-vivo exposure was carried out by a different therapist. SUD and VoC ratings were assessed only in the EMDR group before and after treatment. Subjects in all groups were assessed on a Behavioral Avoidance Test (BAT) and the Spider Phobia Questionnaire (SPQ) before and after each of the treatment conditions and after in-vivo exposure. Multiple *t*-tests were computed across the three treatment conditions and across three different phobic images. The *t*-tests suggested that both SUD and VoC ratings decreased following EMDR. Nevertheless, the multiple *t*-test analyses are inappropriate as they increase the risk of Type I error. In addition, without a comparison with a control condition, the analysis of SUD and VoC are uninformative regarding the specific effect of EMDR. All that can be said is that these variables changed. Why they changed (effects of treatment vs. demand/expectation vs. statistical regression) is unknown. The analysis of BAT scores showed only that change occurred as a consequence of repeated assessment. Post-hoc *t*-tests suggested that in-vivo exposure was followed by reductions in BAT scores in all of the original conditions. Analysis of SPQ scores showed only that change occurred as a consequence of repeated assessment.

Thus, these data show only that SUD and VoC ratings *might* have changed as a result of EMDR. The analyses of BAT and SPQ scores show that no original treatment (EMDR or suboptimal imaginal exposure) had an effect on outcome measures and that only repeated measurement influenced scores. The authors suggested that any change that occurred was a result of in-vivo exposure. Nevertheless, the sequential application of the in vivo procedure imposes a confound that makes it impossible to conclude that in-vivo exposure per se resulted in change. In vivo exposure was preceded by another procedure and any measured change could be the result of an interaction between the two. At best, the data show that neither EMDR nor imaginal exposure reduced spider fear and that in vivo exposure may have contributed to improvement.

Muris, Merckelbach, van Haaften, and Mayer (1997) used a crossover design in comparing EMDR with in vivo exposure in the treatment of spider phobic children. The results showed that a single session of both procedures resulted in statistically significant reductions in standardized self-report measures of fear, and there was no difference between the two procedures on SC following the two types of treatment. Nevertheless, in vivo exposure resulted in greater change on a behavioral avoidance test than did EMDR. The authors concluded that EMDR offers no benefits over in-vivo treatment, which is the treatment of choice for animal phobia. In addition, they concluded that the effects of EMDR are more pronounced on self-report than behavioral indices.

The internal validity of Muris et al.'s (1997) findings is compromised, however, by the fact that the therapist who administered EMDR was different from the therapist who administered in vivo exposure. Because the therapist who administered EMDR was considerably more experienced than the therapist who administered exposure (Muris et al., p. 85), it seems unlikely that this confound would have produced an underestimate of EMDR's efficacy.

Muris, Merckelbach, Holdrinet, and Sijsenaar (in press) randomly assigned spider phobic children to either EMDR, in vivo exposure, or computerized exposure control conditions, after which all groups received 1.5 hours of in vivo exposure. Treatment efficacy measures were obtained before treatment, after the initial phase of treatment, and after the second phase of exposure treatment. Efficacy measures included standardized self-report measures and a behavioral avoidance test. The results showed that in the initial treatment phase, in vivo exposure produced statistically significant improvement on all outcome measures. EMDR yielded significant improvement only on standardized self-report measures. Direct comparisons showed that in vivo exposure resulted in greater change than EMDR on standardized measures and on state anxiety during the behavioral avoidance test.

EMDR showed statistically significant improvement over the computerized control condition with only one of two standardized measures. Ratings of treatment effectiveness showed that the participants regarded in vivo treatment as significantly more effective than either EMDR or the computerized control condition. Analysis of efficacy measures following the application of in vivo exposure to all treatment groups revealed no differences between the groups, suggesting that EMDR did not potentiate subsequent in vivo exposure. As with Muris et al. (1997), the internal validity of Muris et al.'s (in press) findings is compromised by the fact that the therapist who administered EMDR was different from the therapist who administered in vivo exposure. Replication of Muris et al. (1997, in press) without this confound will be necessary. Nevertheless, the results of these three studies strongly suggest that in vivo exposure is the treatment of choice for spider phobia, and that the effects of EMDR on this condition are negligible.

Bates et al. (1996) randomly assigned spider phobic individuals to an assessment-only control or EMDR condition. Measures of EMDR process included subjective fear ratings (1–10) of spider imagery, VoC ratings, and

HR. Outcome measures comparing the two groups included HR, SC, a motoric proximity measure, and self-rated fear during the proximity procedure. Analyses showed that fear imagery measures were reduced and VoC ratings were increased during EMDR, but that there was no effect on HR. Group by pre-post analyses of variance on HR and SC showed no significant main effects or interactions. Analyses of proximity scores and self-reported fear scores revealed only a significant main effect of repeated assessment. Thus, the data indicate that subjects in the EMDR treatment condition changed their verbal reports of fear imagery and adaptive cognitions, but that HR did not change during treatment. In addition, EMDR had no significant effect on outcome variables compared with the assessment-only control condition. The data from three studies (Bates et al.; Muris & Merckelbach, 1997; Muris et al., in press) suggest that EMDR has little effect on a specific phobia.

Foley and Spates (1995) reported a study comparing several procedural variants of EMDR and a no-treatment control condition in their effects on public speaking anxiety. Exposure to affective imagery was a component of all three EMDR variants. One condition included lateral eye movement induced by standard finger movement, one included a lateral auditory stimulus, and one included stationary eye position. The no-treatment control condition was implemented to determine the effect of measurement, passage of time, and other extraneous variables. Measures of efficacy included a number of self-report indices and two non-self-report indices, viz., physiological and independent observer measures. The SUD and VoC ratings were measured only in the treatment conditions using imagery exposure. The results showed that both SUD and VoC ratings changed in all groups, but that there were no differences among groups after treatment. As no data were obtained from the control group, no comparisons with the treatment groups can be made. These effects, minimal as they are, are also consistent with the influence of nonspecific factors.

The standardized self-report measures were obtained from subjects in all four conditions. On a self-report measure, the Personal Report of Communication Anxiety, only subjects in groups that received imagery exposure showed changes in scores, and differed from control subjects. These effects could be due to affective imagery exposure, expectation, demand characteristics, or a combination of all three. On another self-report measure, the Personal Report of Public Speaking Anxiety, there were no effects of any manipulated variable. Subjects also rated the efficacy of their treatment on a 3-point scale. Analyses showed that subjects in the EMDR and stationary eyes conditions reported higher ratings than those in the no-imagery control condition, and that subjects' ratings in the lateral auditory control condition were no different from those in the control condition. There were no differences in self-rated efficacy among treatment groups. Besides the fact the efficacy rating is unvalidated, the pattern of results is also consistent with the influence of nonspecific factors.

Measures other than self-report also showed meager effects. HR showed no main effect of any manipulated variable or interaction. Independent observer ratings of speech performance revealed that speech fear was reduced in all treatment groups, but that there were no significant differences among groups. These results are also consistent with the effects of repeated assessment, nonspecific effects, or both. The authors concluded that the complete EMDR protocol is no more efficacious in reducing speech fear than its variants and that eye movements are not an essential component of the procedure. We concur, but in a more cautious fashion. The results show that treatment effects are consistently found with self-report measures. The design only provides for the control of measurement reactivity and collateral extraneous variables. There are no controls for such nonspecific effects as demand and placebo effects. As a consequence, any statement of efficacy for any of the treatments must be made in the light of these limitations. It would be erroneous to conclude that *any* EMDR variant resulted in change or improvement relative to the control condition. The most liberal interpretation we can make is that exposure to affective imagery reduced the self-reported aversive nature of the imagery. This process seems to be essential in all behavioral re-exposure procedures (Rachman, 1990).

Gosselin and Matthews (1995) compared the effects of EMDR and a stationary eye condition on the test anxiety of college students. Subjects were randomly assigned to one of two treatment conditions, and, in addition, were randomly assigned to high versus low treatment expectancy instructions. SUD and VoC ratings were measured before and after treatment. Test Anxiety Inventory (TAI) scores were measured before treatment and 1 month after treatment. TAI scores showed no significant main effects or interactions involving the eye movement or expectancy manipulations, but both groups showed reduced TAI scores 1 month after treatment. VoC scores showed no significant effects of any experimental variable. Subjects in the EMDR condition showed a significant reduction in SUD ratings, whereas subjects in the stationary eye condition showed no change. Nevertheless, it is unclear if raw scores or change scores were analyzed or if the necessary interaction between eye movement and repeated assessment was statistically significant. We reanalyzed the data by conducting a three-way analysis of variance on raw SUD scores and found a significant interaction between eye movement and repeated assessment. Subsequent simple main effects tests revealed that the EMDR condition showed reductions in SUD ratings and lower ratings than the stationary eye condition after treatment. Examination of cell means for the interaction that we calculated showed that the mean change was 1.77 SUD points and the mean difference at posttreatment was .90 SUD points. Moreover, the mean of the posttreatment SUD rating for the EMDR group (4.95) was far higher than the rating of 0 or 1 typically required by the clinical procedure (F. Shapiro, 1995). Indeed, Gosselin and Matthews commented that the treatment effects on SUD ratings were far less than those previously reported (p. 335).

Thus, the effects of treatment were limited to one self-report measure, and did not generalize to a standardized measure of the clinical problem. It should be noted, however, that the experimental design suggests that neither treatment condition is facilitated or impaired by experimental demand. Although this study provides the first experimental manipulation of demand characteristics, a design including a no-treatment control condition (e.g., Foley & Spates, 1995) would have provided a more definitive test of nonspecific effects.

D. L. Wilson, Silver, Covi, and Foster (1996) recruited 18 individuals with various anxiety disorders who reported emotional memories of traumatic events in an informal interview. Eleven met *DSM-III-R* criteria for PTSD, and the remainder met criteria for other anxiety disorders. Subjects were randomly assigned to either EMDR or one of two control conditions: EMDR without eye movements or EMDR with the tapping of thumbs substituted for eye movements. Treatment consisted of one session, after which subjects in the two control conditions were administered EMDR.

Dependent measures were assessed after each imagery exposure. Subjective measures included SUD and VoC ratings. Psychophysiologic measures included HR, SC, skin temperature, and respiratory rate. Participants in the EMDR group provided subjective ratings of change in intrusive thoughts, recurrent nightmares, phobic avoidance, SUD, and VoC at 3, 9, and 12 months after treatment. Pretreatment and posttreatment ratings and psychophysiological measures for the three conditions were analyzed with *t*-tests, and all showed statistically significant reductions in the EMDR group only. Follow-up assessment after EMDR treatment showed that the changes in SUD and VoC ratings were maintained at 3, 9, and 12 months.

Although D. L. Wilson et al.'s (1996) study provides perhaps the first evidence of an effect of EMDR on psychophysiologic indices, several aspects of their design and analyses suggest caution in the interpretation of their results. No standardized diagnostic procedures were administered, and, thus, the relevance of the findings to any specific anxiety disorder is unclear. In addition, the reliability and validity of the psychophysiological measures are questionable.* To measure SC, the authors used an outdated model of a Stoelting polygraph that does not meet the recommended minimum standards of the Society for Psychophysiological Research (Fowles et al., 1981).

D. L. Wilson et al. (1996) began data collection with the Stoelting polygraph to measure BP, but ceased using it after completion of the EMDR group. Thereafter, a commercial blood pressure monitoring kit was used. These procedures raise two concerns. First, such kits have not been adequately validated for research purposes, and are often unreliable in measuring between group differences (Linden & Zimmerman, 1984; D. Shapiro et al., 1996). Second, although "significant decreases were found for systolic BP in all EMDR conditions" (D. L. Wilson et al., p. 225), this finding is

* The authors wish to thank Danny G. Kaloupek, Ph.D., for his assistance in evaluating psychophysiologic data collection described in D. L. Wilson, Silver, Covi, & Foster (1996).

difficult to interpret. Because the method of measurement is confounded with treatment conditions, it is not possible to directly compare the change among conditions, nor is it possible to compare BP between conditions following treatment. The measured changes in the EMDR group may be an artifact of a particular measurement device. The nature of the SC measures is also problematic. D. L. Wilson et al. used point measurements for SC (i.e., they measured SC once at the end of imagery exposure). This procedure is inconsistent with standard measurement procedure (Fowles et al., 1981), as it is unstable and reactive to such extraneous variables as movement, apnea, and other factors that could mimic treatment effects. Because of logistical constraints, EMDR was administered prior to the administration of the two control procedures (D. L. Wilson, personal communication, April 1, 1997). Despite formal random assignment, the sequential administration of experimental conditions could have introduced procedural confounds that compromised the effects of random assignment. Specifically, D. L. Wilson et al.'s findings are potentially susceptible to an instrumentation confound (Campbell & Stanley, 1963), because the Stoelting polygraph was used with the EMDR group only.

D. L. Wilson et al.'s (1996) analyses were problematic in several respects. The pre- post-treatment comparisons should not have been conducted unless an ANOVA had demonstrated a significant interaction among treatment conditions and pre-post assessment. In addition, comparisons among conditions at posttreatment were not conducted with statistical protection for Type I error (D. L. Wilson, personal communication, February 10, 1997). The use of questionable measures and analyses thus casts doubt on the authors' conclusions regarding the efficacy of EMDR and the necessity of eye movements.

It is worth noting that if D. L. Wilson et al.'s (1996) findings are replicable, they contradict F. Shapiro's (1995) assertion that alternate stimulation can function as the equivalent of eye movements. The finger tapping procedure was apparently no more effective than a no-movement control condition. In any case, these apparent positive effects must be reconciled with studies that have shown no effect of EMDR relative to a control procedure on psychophysiological indices (Bates et al., 1996; Boudewyns & Hyer, 1996; Boudewyns et al., 1993; Devilly et al., in press; Dunn et al., 1996; Pitman et al., 1996).

Finally, the experimental procedure failed to provide for the measurement or control of experimenter/therapist expectancy effects. Although D. L. Wilson et al. (1996) dismissed this explanation on the grounds that "the strength of such expectancy effects has been shown to be slight" (p. 226), this counter argument is not convincing. The expectancy literature to which they refer (Rosenthal, 1966; Rosenthal & Rosnow, 1991) deals with experimental studies in which two groups of researchers are randomly assigned to conditions that differ in terms of a single item of information designed to influence their expectancies (e.g., see Rosenthal, pp. 143–157). Such investigations differ markedly from quasi-experimental studies in which an unbiased group of clinicians is compared with another group of clinicians, who

may possess strong allegiances to, and communicate strong expectancies about, a given intervention (see below). Moreover, D. L. Wilson et al. are incorrect that the typical magnitudes of experimenter expectancy effects are weak. Rosenthal's meta-analysis of 464 experimenter expectancy studies across a variety of domains (e.g., verbal conditioning, personality test responses) revealed a mean effect size (*d*) of .63, which is far from trivial. Although the extent to which this effect size is generalizable to the anxiety treatment literature requires investigation, dismissal of experimenter expectancy or allegiance effects (Gaffan, Tsaousis, & Kemp-Wheeler, 1994) as explanations for the outcome of EMDR studies appears premature. As we concluded from the analysis of S. A. Wilson et al. (1995), effect sizes cannot be used as a counter argument against the possibility of nonspecific effects. Nonspecific effects of a treatment must be assessed against strict experimental controls for such effects (N. Jacobson, 1996; Klein, 1996a, 1996b;).

Examination of Table 2 reveals that, except for D. L. Wilson et al. (1996), all studies meet at least a majority of the criteria applied to PTSD (Foa & Meadows, 1997). A number of procedural ambiguities were clarified by contacting the researchers (personal communications: T. M. Dunn, March 14, 1997, March 19, 1997; U. Feske, March 14, 1997; E. B. Foa, March 18, 1997; T. Foley, March 19, 1997; L. H. Bates, March 21, 1997; H. Hazlett-Stevens, March 19, 1997; W. J. Matthews, March 17, 1997; P. Muris, July 31, August 18, 1997; B. Rothbaum, March 18, 1997; M. Scheck, May 16, 1997; D. L. Wilson, March 14, 1997, March 27, 1997, April 1, 1997), but some remain (Muris & Merckelbach, 1997; Scheck et al., in press; D. L. Wilson et al., 1996). Only Foley and Spates (1995) meet all of the criteria, and their results showed that EMDR was superior only to no treatment and only using subjective ratings of the treatment process, rather than treatment outcome. Thus, conclusions regarding the efficacy of EMDR on other disorders of anxiety can be no more positive than those drawn by Foa and Meadows and Keane (in press) regarding PTSD: EMDR reduces subjective distress during treatment, but extremely strong claims regarding the efficacy of EMDR (e.g., F. Shapiro, 1995, 1996a; F. Shapiro & Forest, 1997) await studies that adequately control for nonspecific factors (Bootzin, 1985; Grünbaum, 1985).

Research on the efficacy of EMDR (or any treatment for which extremely strong claims are made) may require methodological procedures to control for certain procedural artifacts. Presuming that some therapists may have strong allegiances for or against a particular treatment, the possibility of strong experimenter expectancy effects may be unavoidable. For example, Scheck et al. (in press) assigned only EMDR-trained therapists to EMDR treatment only. It is possible that allegiance to EMDR may have resulted in nonspecific effects or procedural artifacts that contributed to the apparent superiority of EMDR. Thus, we recommend that, wherever possible, such therapists train individuals without strong a priori biases for or against EMDR, who can then administer both the experimental and control treatments. A second strategy is to develop measures of treatment integrity and

TABLE 2

SUMMARY DESCRIPTION OF EXPERIMENTAL PROCEDURES USED AS CRITERIA SPECIFIED BY FOA AND MEADOWS (1997)

Clinical Problem	Authors	Clear Target Symptoms	Reliable & Valid Measures	Blind Assessors	Assessor Training	Manualized Specific Treatment	Unbiased Treatment Assignment	Treatment Adherence
Traumatic memories	Dunn et al. (1996)	No	No	No	Yes	Yes	Yes	No
	Hazlett-Stevens et al. (1996)	Yes	Yes	No	Yes	Yes	Yes	No
	Scheck et al. (in press)	Yes	Yes	Yes	?	Yes	Yes	No
PTSD	Devilly et al. (in press)	Yes	Yes	No	No	Yes	Yes	No
Various anxiety disorders	Wilson et al. (1996)	No	No	No	Yes	Yes	?	No
Panic disorder	Feske & Goldstein (1996)	Yes	Yes	No	No	Yes	Yes	Yes
Spider phobia	Bates et al. (1996)	Yes	No	Yes	No	Yes	Yes	Yes
	Muris & Merckelbach (in press)	Yes	Yes	No	Yes	Yes	Yes	No
	Muris et al. (1997)	Yes	Yes	Yes	Yes	Yes	?	No
	Muris et al. (in press)	Yes	Yes	Yes	Yes	Yes	Yes	No
Public speaking anxiety	Foley & Spates (1995)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test anxiety	Gosselin & Matthews (1995)	Yes	Yes	No	No	Yes	Yes	No

incorporate them into the analysis. For example, the videotaping of all treatment sessions could be reviewed by unbiased observers to derive such measures as enthusiasm, expressed levels of positive affect, expectations communicated to the client/subject, and attempts to direct the confrontation with traumatic imagery content. These videotapes ideally should be edited to exclude SUD and VoC ratings, which might bias observer ratings of these variables. The measures derived from these tapes could then be correlated with measures of treatment effectiveness within and across treatment conditions. A third, logistically more complicated, strategy is to conduct multi-site studies of the efficacy of EMDR, with certain sites directed by proponents of EMDR and other sites directed by skeptics of EMDR (for a similar strategy, see Van Ommeren, 1996). As Klein (1996b) noted, this strategy allows investigator bias to be examined systematically and forces sites "to expose or abandon outcome-relevant research idiosyncracies" (p. 83). All three of these strategies are absent from EMDR research.

It should be noted (see Table 1) that, with one exception (Bates et al., 1996), all of the therapists administering EMDR had been trained under the auspices of the EMDR Institute. This fact meets the most basic criterion needed for the demonstration of treatment fidelity (Moncher & Prinz, 1991) although it does not ensure it. Moreover, treatment fidelity does not necessarily ensure positive treatment outcomes for EMDR. Feske and Goldstein (1997), for example, showed no long-term difference between EMDR and the control procedure despite the use of a treatment manual and checklist approved by Shapiro. In addition, Pitman et al. (1996) reported relatively weak relationships between treatment fidelity ratings of an EMDR expert and outcome measures.

The effect of EMDR on other forms of anxiety is similar to those for traumatic memories and PTSD. Table 1 suggests that any effects of EMDR are limited to verbal report measures of distress and are most pronounced when compared with no treatment. However, when compared to control procedures, the comparative effects of EMDR have not been demonstrated. Table 2 suggests that outcome studies have incorporated improved methodological controls; and, when such controls are applied, the measured effects of EMDR appear unremarkable (Bates et al., 1996; Devilly et al., in press; Feske & Goldstein, 1997; Foley & Spates, 1995; Hazlett-Stevens et al., 1995); nor enduring (Devilly et al.; Feske & Goldstein). When EMDR has been compared to in vivo exposure (Muris et al., 1997, in press), it has shown no additional clinical effect on subjective and physiological indices and has been less efficacious on behavioral indices. Nevertheless, further examination of EMDR's efficacy relative to in vivo exposure is warranted.

Summary of Treatment Efficacy Literature

It is clear from the review of these 17 studies that there is little ordinary evidence and no extraordinary evidence to support the efficacy of EMDR. There are no additional data to substantively alter the original conclusions

of Lohr, Kleinknecht, et al. (1995) and Foa and Meadows (1997) regarding EMDR. Only verbal report measures are altered, there is little evidence of an effect on physiological and behavioral indices, and little or no evidence for efficacy above and beyond nonspecific effects (Gosselin & Matthews, 1995; Hazlett-Stevens et al., 1996; Scheck et al., in press). In the only direct comparisons with demonstrably valid treatment (Muris et al., 1997, in press), EMDR's behavioral effects were negligible. In addition, this review has applied the same criteria for experimental procedures, nonspecific effects, and treatment artifacts for EMDR as for other treatments (Foa & Meadows, 1997; Keane, in press). Thus, there appears to be little support for the assertion (Greenwald, 1997; Rogers, 1996) that EMDR has been held to different or higher standards than other behavioral treatments for the same disorders (Hellstrom & Öst, 1995; Muris & Merckelbach, 1997; Muris et al., 1997, in press).

Finally, we should note that measures of treatment efficacy have largely neglected the mechanisms to which eye movements and information reprocessing are directed. These mechanisms are purported to involve cognitive content and organization and the manner in which information is processed. McNally (1996a) reviewed the experimental psychopathology literature that included consistent attentional bias in anxiety disorders measured by such techniques as the emotional Stroop task, dichotic listening, and other interference procedures. Alternative treatments, such as exposure, anxiety management training, and cognitive behavioral procedures, yield reductions in attentional bias following effective treatment (Lavy, van den Hout, & Arntz, 1993; Mathews, Mogg, Kentish, & Eysenck, 1995; Mattia, Heimberg, & Hope, 1993). Research on the effects of EMDR has yet to incorporate such measures to show an alteration or acceleration of the processing of affective information. Specific measures of emotional processing are necessary in inquiries that test not only the efficacy of the treatment but the validity of the theory that justifies its application. This applies equally to EMDR and other treatments that target the emotional or cognitive processing of information related to traumatic events (e.g., Resick & Schnicke, 1992; Roth & Newman, 1991, 1993).

Treatment Components

Early experimental research with single-subject designs suggested that eye movements are not necessary for reduction of verbal reports of symptoms (Acierno, Tremont, et al., 1994; Lohr, Tolin, & Kleinknecht, 1995, 1996; Montgomery & Ayllon, 1994a, 1994b). The findings of the initial group-design studies indicated the same (Renfrey & Spates, 1994; Sanderson & Carpenter, 1992). F. Shapiro (1994a, 1995) argued that alternate stimuli, such as finger tapping (Bauman & Melnyk, 1994; D. L. Wilson et al., 1996), have the same therapeutic effect as eye movements. The research reviewed here, however, strongly suggests that no alternate stimulation is necessary for the reduc-

tion of clinical symptoms (Boudewyns & Hyer, 1996; Devilly et al., 1995; Dunn et al., 1996; Feske & Goldstein, 1997; Foley & Spates, 1995; Gosselin & Matthews, 1995; Pitman et al., 1996). Further, the findings of D. L. Wilson et al. suggest that alternate lateral stimulation is no more effective than a control condition without eye movements. Any reductions in these measures may be a function of imagery re-exposure (Muris & Merckelbach, 1997; Vaughan et al., 1994). Another purported mechanism of change is the induction of relaxation, with subsequent anxiety reduction, by eye movements (Hedstrom, 1991; Welch, 1996). Feske and Goldstein (1995) tested this hypothesis by comparing eye movement with eye fixation in two groups of subjects with panic disorder. The results showed no differences in the alteration of anxiety symptoms derived from the State Form of the STAI. In response to the data showing no effect of alternative stimulation, F. Shapiro stated:

> EMDR is not simply eye movement. Eye movement, or other stimulation is merely one component of a complex method that combines aspects of many of the major modalities. That is why behaviorists, cognitivists, psychodynamic [sic], etc. . . . are able to find EMDR useful. Remove the eye movement and there is still a very powerful method (1996b).

We contend that this statement and others (F. Shapiro, 1994a, 1995) obscure the empirical issues. If eye movements are no longer necessary, then it is incumbent on the treatment's proponents to specify the essential features of the treatment in order to conduct controlled experiments assessing the relative effects of procedural artifacts and the substantive clinical procedure. To quote Pitman et al.:

> Scientific theories are meaningless if they are not falsifiable. The finding that eye movements may be deleted from the EMDR procedure without loss of emotional processing (and therapeutic benefit) necessarily falsifies neurologic (including REM) theories of eye movements in EMDR's mechanism of action (1996, p. 426).

Thus, F. Shapiro's (1994a, 1995, 1996b) comments raise the critical question of whether the theory and procedure of EMDR, at least as currently proposed, are truly falsifiable.

Without a clear specification of what elements of treatment are necessary, any auxiliary hypothesis (Meehl, 1978, 1990) may be advanced in the face of disconfirming evidence. We concur with Grünbaum (1985), who argued that the empirical test of the characteristic features (e.g., eye movements) and the incidental features of a treatment should best be conducted in relation to the theory that justifies the treatment. If the theory of treatment considers

eye movements to be the most characteristic feature of the treatment, then control conditions manipulating eye movements and alternate stimulation are necessary. Determination of the functional significance of incidental features (e.g., imagery re-exposure, re-attribution) requires control of these elements and comparison with the effects of the characteristic components. If the characteristic elements cannot be specified, it may be impossible to apply the essential feature of scientific inquiry, namely, experimental disconfirmation (O'Donohue & Thorp, 1996). Scientific inquiry then becomes irrelevant in the evaluation of the treatment procedure.

EMDR Theory and Model of Treatment

Since the original publication reporting the beneficial effects of EMDR (F. Shapiro, 1989), little in the way of theoretical analysis has been published. Speculation has centered around the possible physiological effects of eye movements on relaxation and anxiety (Hedstrom, 1991; Welch, 1996), the orienting response (Armstrong & Vaughan, 1996), and more traditional learning processes (Dyck, 1993). With the publication of the first book on the subject (F. Shapiro, 1995), we have the opportunity to examine the substance of the conceptual analysis. This conceptual analysis has been reviewed elsewhere (Lohr, 1996; McNally, 1996b, in press; O'Donohue & Thorp, 1996), and only a summary is presented here.

Consistency With Psychological Research

The model is flawed in several ways, including the information processing metaphor of psychopathology, the physiologic mechanisms used to explain disorder, and the Accelerated Information Processing mechanism used to explain the process of treatment. The model does not incorporate the contributions of such behaviorally oriented scholars as Barlow, M. Eysenck, Foa, Hollon, Keane, MacLeod, and Mathews (see Martin, 1991), who have applied the concepts of cognitive neuroscience to clinical problems. Lang's (1985) bioinformational analysis of affective imagery, with its use of propositional networks, is mentioned only in passing. Substantial analyses of fear and anxiety, such as those by Rachman (1990), are not considered. These limitations extend directly to the mechanisms of therapeutic change. There is little consideration of habituation, extinction, or other learning processes. Instead, Accelerated Information Processing is proposed as a means by which pathological conditions are altered by eye movements in the treatment protocol. Keane (in press) has also commented on the limitations of the conceptual analysis:

> Unlike exposure therapy which has a long tradition of ameliorating a range of anxiety mediated clinical problems and which is embedded in the rich conceptual tradition of experimental psychology, EMDR falters seri-

> ously at the theoretical level and has limited scientific support . . . The primary weakness of EMDR stems from a distinct lack of integration with existing models of psychopathology and psychotherapy. While existing models may have their own failings, it is incumbent upon the proponents of EMDR to postulate how their view of the problems associated with PTSD differs from others', and how this technique can allay specific targeted symptoms of this multidimensional disorder (p. 14–15).

Nature and Necessity of Eye Movements

Although eye movements are accorded a central role in EMDR's mechanism of action (F. Shapiro, 1995), the analysis of the psychological function and therapeutic necessity of eye movements is problematic. There are a number of terminological confusions regarding movements of the eye, including rapid eye movement (REM), nystagmus, and "saccades" (F. Shapiro, 1989). Although all are conjugate eye movements, the induced (tracking) movements used in EMDR are voluntary smooth pursuit movements. The other eye movements are involuntary. Saccadic movements are ballistic in nature, and occur as the point of fixation changes. Optokinetic nystagmus is involved in motion detection, and consists of quick movements followed by a rapid return to the point of fixation. Vestibular nystagmus results from stimulation of vestibular organs and is involved in orientation and posture. These last three have nothing to do with smooth pursuit movements, and none is related to REM during sleep. Nonetheless, these terms are related by F. Shapiro (1995) to common pathological and therapeutic processes, as in the discussion of nystagmus (p. 24) and dreaming sleep (p. 39).

The general function of the analysis is to argue that eye movements are the essence of the therapeutic process. Nevertheless, F. Shapiro suggests elsewhere (1994a, p. 89; 1995, p. 31) that eye movements can be supplanted with alternate stimulation, such as finger taps. As a consequence, the reader is asked to conclude that the changes in verbal reports of distress, in the absence of eye movements, are genuine effects of the treatment, rather than nonspecific artifacts. The weight of the research reviewed here, however, strongly suggests that eye movements or other methods of lateral stimulation are not necessary for changes in the verbal report of symptoms (Boudewyns & Hyer, 1996; Devilly et al., in press; Feske & Goldstein, 1997; Foley & Spates, 1995; Gosselin & Matthews, 1995; Pitman et al., 1996; Renfrey & Spates; 1994). The research on the functional significance of eye movements has turned the orienting response (Armstrong & Vaughan, 1966), physiological (Hedstrom, 1991; Welch, 1996), and accelerated information processing (F. Shapiro, 1995) models of EMDR into explanations in search of a phenomenon. Such theorizing appears to have occurred with little concern for limitations in both the methodology and data of EMDR research. These

models appear to have implicitly assumed the procedure's efficacy prior to any convincing empirical evidence.

Despite the limitations of theory and data, EMDR is referred to as the first paradigm shift in psychology since Freud (Shapiro, 1994b, pp. 153–155; 1995, pp. *v*, 12–17). This claim neglects the cognitive revolution that began in the late 1950s and spawned (in part) cognitive behavior therapy. In addition, this claim is inconsistent with Kuhn's (1970) analysis, which describes the way in which normal science gives way to epistemological change when the emerging data no longer fit the models describing the phenomenon. As the research reviewed here suggests, there is no emerging body of data on EMDR treatment that is any different from data on other forms of therapeutic exposure. There is no paradigm to which we can shift, nor is there any compelling reason to do so.

Implications for Behavior Therapy

As noted earlier, the relationship between the development of EMDR and behavior therapy may not be coincidental. The use of the term "desensitization" in EMDR is not a semantic accident. Indeed, Wolpe (1990) argued that EMDR may be only a variant of more established behavioral change procedures. Fortunately, the reliance of behavioral researchers upon experimental methodology to validate cognitive behavioral treatments has provided a means by which the validity and theoretical rationale of EMDR can be judged (Acierno et al., 1994; Herbert & Mueser, 1992, 1995; Lohr et al., 1992, Lohr, Kleinknecht, et al., 1995).

The process of empirical validation has revealed a disparity between the data justifying the application of EMDR and its current widespread use. We may use a comparison with systematic desensitization to illustrate this point. Wolpe's first book describing systematic desensitization for fear-related disorders was published in 1958. Five years later it was known that its effects were not measurement artifacts (Lang & Lazovik, 1963; Lang et al., 1965). Eight years later it was known the effects were not due solely to non-specific effects of attention, were greater than those of insight-oriented psychotherapy (Paul, 1966), and were long-lasting (Paul, 1967). Ten years later it was known how the complete treatment compared to its constituent structural elements (Davison, 1968). Forty years later, it is now known that the most essential feature of fear reduction is re-exposure, either in-vivo or imaginal (Rachman, 1990). In contrast, 8 years of research on EMDR tells us that, although the treatment effects are not measurement artifacts (S. A. Wilson et al., 1995), they are limited to only some verbal report measures, that eye movements appear to be superfluous, and that the mechanisms of action are likely to be nonspecific effects, imagery re-exposure, or both. Thus, EMDR appears to represent a re-engineering of the "fear reduction wheel," rather than a novel or unique treatment. Clinicians treating fear-related conditions

are now faced with a clinical procedure that is alleged to be more efficacious, efficient, and long-lasting than behavioral procedures, although extant evidence fails to support that claim (DeBell & Jones, 1997; Foa & Meadows, 1997; Keane, in press; McGlynn, 1997).

Nevertheless, we believe that the empirical and theoretical literature on EMDR imparts a valuable lesson for the field of behavior therapy. Because many treatments that purport to be novel or unique in their effects may adventitiously incorporate exposure, the burden falls on the developer of such techniques to demonstrate that their efficacy does not derive entirely from well-established mechanisms of change. Consequently, both researchers and clinicians may need to become more vigilant to the possibility that exposure and other nonspecific processes account for the effects of newly developed treatment methods that have been asserted to operate by means of change mechanisms that are qualitatively different from those of other interventions. From this perspective, we and others who have found the evidence for EMDR's efficacy to be less than convincing (e.g., Acierno, Hersen, et al., 1994; DeBell & Jones, 1997) have not been responsible for holding EMDR to a higher standard than extant treatments (c.f., Greenwald, 1997; Rogers, 1996). Instead, it would appear that those proponents of EMDR who have argued that it is not a variant of exposure treatment (e.g., F. Shapiro, 1995) have, perhaps unwittingly, brought a higher standard for EMDR's clinical validation upon themselves. 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. Although our review strongly suggests that EMDR falls considerably short of the high standard that its most vocal proponents have set for it, we believe that this standard must be exceeded by any treatment that is purported to operate by means of novel or unique mechanisms of change. The field of behavior therapy, which has traditionally insisted on high standards for empirical validation, should settle for nothing less.

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