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Eye Movement Desensitization and Reprocessing: An Analysis of Specific versus Nonspecific Treatment Factors

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Abstract—Incremental validity and incremental efficacy have become important issues in the evaluation of psychological assessment and intervention procedures. Incremental validity in assessment is that shown by novel measures over and above established ones. Incremental efficacy is that shown by novel treatments over and above nonspecific and established treatment effects. In this paper, we critically examine the question of whether Eye Movement Desensitization and Reprocessing (EMDR) possesses efficacy above and beyond nonspecific treatment effects and components that are shared with well-established interventions. A review of recently published efficacy studies reveals that (a) the effects of EMDR are largely limited to verbal report indices, (b) eye movements and other movements appear to be unnecessary, and (c) reported effects are consistent with nonspecific treatment features. Examination of individual studies shows that control procedures for nonspecific features have been minimal. We analyze EMDR for nonspecific treatment features and suggest experimental controls to examine the incremental efficacy of EMDR.

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EYE MOVEMENT DESENSITIZATION AND REPROCESSING: PROMOTION AND CLINICAL POPULARITY

The past 10 years have witnessed a rapid expansion of treatments for anxiety and trauma. Numerous novel treatments for these problems have arisen and are known as Power Therapies (Figley, 1997). These include Thought Field Therapy (TFT; Callahan, 1985, 1987; Figley, 1995; Figley & Carbonell, 1996; Gallo, 1995), Emotional Freedom Therapy (EFT; Craig, 1997), Traumatic Incident Reduction (TIR; Gerbode, 1995), and Visual/Kinesthetic Dissociation (Bandler & Grinder, 1979). The most visible of these treatments is Eye Movement Desensitization and Reprocessing (EMDR). The EMDR Institute, Inc. (1997) reports that over 22,000 licensed clinicians have been trained since the initial published study of EMDR (Shapiro, 1989). Strong claims of EMDR's clinical efficacy include its purported rapidity, permanence, range of applicability, and superiority relative to extant treatments (Shapiro, 1995, 1996b; Shapiro & Forrest, 1997).

Reviews of Literature

The first independent reviews of experimental outcome studies of EMDR (Acierno, Hersen, Van Hasselt, Tremont, & Mueser, 1994; Lohr, Kleinknecht, Tolin, & Barrett, 1995) noted clinical improvement in objective outcome variables but also noted minimal experimental controls for the nonspecific effects of treatment. Foa and Meadows (1997) and Keane (1998) have concluded that the methodological limitations of EMDR outcome studies on Posttraumatic Stress Disorder (PTSD) make EMDR as yet an unvalidated treatment. Others have expressed caution regarding the widespread adoption of EMDR based on the research evidence (DeBell & Jones, 1997). Indeed, the limitations in EMDR theory and research are used as an object lesson for research methods in a widely adopted introductory psychology textbook (Bernstein, Clarke-Stewart, Roy, & Wickens, 1997).

A comprehensive review of 17 recent studies (Lohr, Tolin, & Lilienfeld, 1998) evaluated EMDR across a broad range of clinical problems. Some of these studies have made substantial methodological improvements over earlier research in the control of procedural and nonspecific artifacts (e.g., Devilly, Spence, & Rapee, 1998; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996; Rothbaum, 1995). The conclusions of Lohr et al. bolster and extend the conclusions of earlier reviews: the effects of EMDR are limited largely or entirely to verbal report indices, eye movements (or other bilateral stimulation) appear to be unnecessary for improvement, and the theoretical analysis of EMDR is inconsistent with the data regarding its clinical efficacy.

In this paper, we focus on the question of whether the efficacy of EMDR exceeds that produced by nonspecific factors. In doing so we are not holding

EMDR to an unfairly higher standard (cf. Greenwald, 1997; Rogers, 1996). Because many of EMDR's proponents (e.g., Shapiro, 1995; Shapiro & Forrest, 1997) maintain that EMDR is superior to extant treatments and works by means of qualitatively distinct and novel mechanisms of change, it is necessary to ascertain whether EMDR's clinical effects exceed those of established treatments (e.g., flooding), that are based on well-established mechanisms of change (e.g., exposure). If EMDR is no more effective than such treatments, it would call into question the extremely strong claims that have been made for its efficacy (Fensterheim, 1996; Greenwald, 1997; Hyer & Brandsma, 1997; Shapiro, 1995; Shapiro & Forrest, 1997).

MEANINGS OF SPECIFIC AND NONSPECIFIC TREATMENT FACTORS

Historical Context

The importance of placebo or nonspecific effects in psychosocial interventions for psychological disorders has been a long-standing and recurring issue in psychotherapy research (Brody, 1988; Critelli & Neuman, 1984; Frank, 1961, 1971; Grencavage, Bootzin, & Shoham, 1993; Grünbaum, 1985; Kazdin, 1979a,b; Roberts, Kewman, Mercier, & Hovell, 1993; Prioleau, Murdock, & Brody, 1983; A. K. Shapiro, 1971; Strupp, 1986; White, Tursky, & Schwartz, 1985; Wilkins, 1979, 1984). Much of this interest has been generated by the oft-cited "Dodo Bird verdict" (Carroll, 1936) that all treatments are equally effective (Beutler, 1991; Luborsky, Singer, & Luborsky, 1975; Wampold et al., 1997). The presumption of equal efficacy has led to the hypothesis that shared factors account for the effects of all psychological treatments. For example, the expectation for improvement engendered by plausible treatment rationale, attention by a credible professional, etc., have been suggested as important common factors across various schools of psychotherapy (Critelli & Neuman, 1984; Grencavage et al., 1993; Kazdin, 1979a,b; Kirsch, 1978, 1990, 1997a,b; Ross & Olson, 1981).

Research on behavioral (and cognitive-behavioral) treatments has sought to identify and control for the nonspecific effects of treatment procedures as a means of empirical validation (Bowers & Clum, 1988; Grencavage et al., 1993; Jacobson & Baucom, 1977; Kazdin, 1979b; Kazdin & Wilcoxon, 1976; Kirsch, 1978). That is, there is an assumption that an efficacious treatment must produce effects greater than those produced by nonspecific factors alone. This control is important for two reasons. The first is related to Eysenck's (1994) reanalysis of earlier meta-analytic research that had led to the Dodo Bird verdict, and his subsequent conclusion that behavioral treatments were in fact superior to other interventions for disorders of fear and anxiety. The second reason is a current trend in research to focus on specific treatments for specific

disorders. This trend has gained visibility with the publication of the American Psychological Association's Division of Clinical Psychology's report on criteria for empirical validation and a listing of treatments that approximate or meet those criteria (Chambless, 1995; Chambless et al., 1996, 1998).

The nature of nonspecific effects has been extensively debated. Nonspecific effects can be grouped into three broad categories: effects without specific activity, unspecified but active effects, and common factors (Critelli & Neuman, 1984; A. K. Shapiro, 1971). In their discussion of nonspecific effects, Critelli and Neuman (1984) introduced the concept of incremental efficacy as a means of evaluating the relative contribution of specific and nonspecific factors in psychosocial treatments. Incremental efficacy has its roots in the concept of incremental validity (Meehl, 1959; Sechrest, 1963) in the personality and clinical assessment literature. The latter concept refers to the extent to which a measure contributes useful information above and beyond information that has already been collected (e.g., demographics, currently available psychometric data). As the number of personality tests with at least some degree of validity has proliferated, the field of clinical psychology has increasingly moved from the demand of simple zero-order validity (i.e., the demand that a test exhibit convergent validity with a criterion or quasi-criterion) to the more rigorous demand of incremental validity relative to other measures that are available. In other words, new tests that are entirely redundant with extant tests of comparable length and ease of administration are of little or no utility to either the clinician or the clinical researcher.

We argue by extension that the incremental efficacy of a treatment must be demonstrated relative to nonspecific treatment factors and established procedures with common elements that have shown to possess clinical efficacy. In the case of EMDR, this demonstration requires the use of control conditions for such nonspecific factors as treatment credibility, expectation for improvement, experimental demand, and therapist-experimenter allegiance effects (Evans, 1985; Gaffan, Tsaousis, & Kemp-Wheeler, 1995; Grünbaum, 1985). In addition, incremental efficacy can involve comparisons with procedural analogues that manipulate essential components of treatment such as eye movements or other external stimulation (Boudewyns & Hyer, 1996; Boudewyns, Stwertka, Hyer, Albrecht, & Sperr, 1993; Devilly et al., 1998; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996; Sanderson & Carpenter, 1992). Lastly, incremental efficacy can be tested through direct comparison with other validated techniques such as in vivo and imaginal exposure (Foa & Meadows, 1997; Muris & Merckelbach, 1997; Muris, Merckelbach, van Haaften, & Mayer, 1997; Muris, Merckelbach, Holdrinet, & Sijsenaar, 1998).

These controls represent what Borkovec (1985; Borkovec & Bauer, 1982) refers to as strong-inference research. The essential feature of strong-inference research is the disconfirmation of crucial rival hypotheses derived from competing theories (Platt, 1964). In addition to testing competing theories,

such research provides for tests of competing procedural elements (treatment components) across and within treatments. Similar strategies have been employed in the empirical validation of other treatments for anxiety disorders, such as systematic desensitization (Kazdin & Wilcoxon, 1976; Kirsch, 1990), exposure treatments for dental fear (Bernstein & Kleinknecht, 1982), and cue-controlled relaxation for test anxiety (Marchetti, McGlynn, & Patterson, 1977).

Placebo and Treatment

Definitions of placebo. Brody (1985) and Grübaum (1985), provided contrasting views on the nature of placebo and nonspecific factors in psychosocial treatments that serve as the basis for our evaluation of the incremental efficacy of EMDR. Grübaum's analysis provides a terminology that facilitates a comparison of the two views and allows for the derivation of concepts that facilitate a synthesis. In Grübaum's terms:

D = target disorder
t = treatment modality (procedure)
Y = theory advocating use of t for D
F = characteristic constituents of t according to Y (i.e., for a certain treatment process to be viewed by Y as an authentic example of t, F are required, but are not the only features of t)
C = incidental constituents of t according to Y (i.e., for a certain treatment process to be viewed by Y as an authentic example of t, F are required but C need not be present)
P = dispensing practitioner
V = the person receiving the treatment (Brody, 1985).

Grübaum (1985) differentiated between two types of placebo procedures. An *intentional placebo* occurs if and only if t is applied to the disorder in the following manner:

1. None of F are remedial for D;
2. P believes that none of F are remedial for D;
3. P believes that t is remedial for D by virtue of C;
4. P causes or allows V (patient) to believe that t is remedial for D by virtue of F.

Such would be the case, for example, in the planned application of an inactive placebo control condition in a treatment outcome experiment where V is unaware of the fact that they are receiving the placebo. An *inadvertent placebo* occurs if and only if t is applied in the following manner:

1. None of *F* are remedial for *D*;
2. *P* believes that *t* is remedial for *D* by virtue of *F*;
3. *V* (patient) believes that *t* is remedial for *D* by virtue of *F*.

Thus, the major distinction between the two types of placebo rests on the beliefs of the practitioner: In the intentional placebo, the practitioner believes that the characteristic features of treatment are ineffective but leads the patient to believe they are. In inadvertent placebo, the practitioner erroneously believes that the characteristic features are effective and leads the patient to believe it also. The former is an experimental deception on the part of the practitioner, and the latter represents either self-deception or a lack of knowledge regarding the treatment.

Using similar concepts, Brody (1985) specified the concept of placebo as it relates to two types of therapy. In its primary form, a "placebo" is a form of therapy, or an intervention designed to simulate therapy, that at the time of use is believed not to be a specific therapy for the condition for which it is offered. Rather, it is used either for its psychological effect or to eliminate observer bias in an experimental setting. By extension, a placebo can be a form of medical therapy now known to be ineffective, though believed effective at the time of use. Thus, Grübaum's (1985) intentional placebo is most similar to Brody's primary definition of the term. The inadvertent placebo is most similar to the extension of Brody's term, but only in retrospect.

Definitions of Treatment

Brody (1985) asserted that *t* is a "therapy" for *D* if and only if it is believed that the administration of *t* to a person with *D* increases the probability that *D* will be cured, relieved, or ameliorated, as compared with the probability that this will occur without *t.* As with Brody's concept of placebo, the definition of therapy is predicated upon the beliefs of the persons administering and receiving the treatment.

Although Grübaum (1985) specified inadvertent and intentional placebos, he did not specify the nature of inadvertent or intentional treatment. Building upon Grübaum and Brody's (1985) analyses, these treatments can be derived using the same constituents used to define placebos. *Inadvertent treatment* occurs where:

1. *t* is remedial for *D*;
2. None of *F* are remedial for *D*;
3. *P* believes that some or all of *F* are remedial for *D*;
4. *P* does not believe that *t* is remedial for *D* by virtue of only *C*;
5. *P* causes or allows *V* (patient) to believe that *t* is remedial for *D* by virtue of *F* rather than *C*.

Such is the case when a therapist inadvertently administers some of the right procedures for the wrong reasons. It is also the case in which, unbeknownst to the therapist, the incidental features of a treatment are responsible for its therapeutic effects. In the case of EMDR, the therapist uses imagery exposure, which we argue is an incidental feature, but incorrectly attributes treatment efficacy to the characteristic feature of eye movement.

Intentional Treatment occurs where:

1. *t* is remedial for *D*;
2. Some or all of *C* may be remedial for *D*;
3. Some or all of *F* are remedial for *D*;
4. *P* believes that *t* is remedial for *D* by virtue of some or all of *F*;
5. *P* causes or allows *V* (patient) to believe that *t* is remedial for *D* by virtue of *F*.

Such is the case when a therapist administers the correct procedures for the right reasons. It is exemplified by the circumstance in which the characteristic features do have demonstrable influence and the incidental features *might* have demonstrable influence. In the case of systematic desensitization, the therapist uses relaxation and imagery exposure, but correctly attributes efficacy to exposure rather than relaxation.

Thus, Brody's (1985) definition of treatment is consistent with the concept of intentional treatment. These derivations from both Brody's and Grübaum's (1985) analyses include attributions of the patient and therapist, but Grübaum's derivation also presumes that the treatment is empirically effective (*t* is remedial for *D*) and treatment efficacy is due in part to the *characteristic* features of the treatment.

Moreover, Grübaum (1985) argued that the empirical test of treatment efficacy is best conducted when the characteristic and incidental features of the treatment can be specified in relation to a theory on which the treatment is predicated. Determination of the functional significance of incidental features requires control of these elements and comparison with the effects of the characteristic component(s). Because the incidental factors can include the beliefs and attributions of the therapist and patient, our derivation of Grübaum's analysis encompasses that of Brody (1985). Borkovec (1985), in his commentary on Brody and Grübaum, noted that only Grübaum's analysis provides the strong tests with which to determine the relative effects of characteristic and incidental features through component control experimental designs. For these reasons, we will emphasize Grübaum's concepts in our evaluation of the efficacy of EMDR. Following a review of the research, we will outline a series of experiments designed to improve the empirical tests of characteristic and incidental features of EMDR.

REVIEW OF PLACEBO-CONTROLLED OUTCOME STUDIES ON EMDR

Categorization of Controlled Factors

Wait-list controls. No treatment and wait-list control procedures provide for the assessment of statistical regression, measurement reactivity, and remission of symptoms. Some studies that have compared EMDR with no treatment or wait-list controls show greater effects of EMDR on self-report measures for the treatment of specific phobia (Bates, McGlynn, Montgomery, & Mattke, 1996), PTSD (Boudewyns & Hyer, 1996; Rothbaum, 1995), traumatic memories (Shapiro, 1989; Wilson, Becker, & Tinker, 1995, 1997), panic disorder (Feske & Goldstein, 1997), and public-speaking anxiety (Foley & Spates, 1995). These results, however, do not necessarily provide support for EMDR because they can be attributed to any number of incidental, nonspecific factors. Moreover, Cohen (1994), Meehl (1978), and others have argued that the null hypothesis is, taken literally, almost always false. Consequently, given sufficient statistical power, it will almost always be possible to reject the null hypothesis that a treatment has no effect (i.e., the hypothesis that the effect size when compared with no treatment equals exactly zero). From this perspective, the demonstration that a treatment is more efficacious than a wait-list control is not especially informative. Although this conclusion holds for all psychological treatments, it is especially pertinent to interventions, such as EMDR, whose efficacy has been claimed to greatly exceed that produced by nonspecific influences (Shapiro, 1995, 1996b).

Attention controls. Attention controls attempt to equate the amount and general nature of therapeutic contact that is common to all conditions in the conduct of an outcome experiment or in the treatment setting (Mahoney, 1978). Four studies have used procedures that approximate attentional controls.

Jensen (1994) randomly assigned combat veterans with PTSD to either a control or an EMDR group. Control subjects were provided with referral information for alternative treatment. Subjects in the EMDR condition were also eligible for these services, but, in addition, were provided with EMDR that involved one history-taking session and two treatment sessions. Outcome measures included a standardized clinical interview and self-report assessment procedures. Data analysis revealed no statistically significant differences between groups.

Boudewyns et al. (1993) recruited Veterans-hospital patients with PTSD, who were receiving standard inpatient milieu treatment. Subjects were randomly assigned to one of three groups: (a) EMDR, (b) Exposure Control (EC), or (c) milieu-only control. The EC group was procedurally similar to the EMDR group except for eye movements. Treatment efficacy was assessed with standardized self-report measures, therapist ratings of clinical symptoms,

and psychophysiologic measures. The standardized measures showed no differential effect of treatment, and no form of treatment appeared to affect the psychophysiologic measures. Although therapist ratings of treatment responders versus nonresponders favored the EMDR group, therapists were not blind to treatment conditions. Blind assessment has been identified as a critical issue in the evaluation of treatment effects with PTSD (Foa & Meadows, 1997).

Silver, Brooks, and Obenchain (1995) treated inpatient veterans with PTSD, who received milieu treatment along with one of the following treatments: (a) EMDR, (b) biofeedback, or (c) group relaxation training. A third control group received only milieu treatment. Treatment outcome was assessed with an unstandardized measure of general symptomatology. The data analyses used multiple *t*-tests on change scores of an unvalidated outcome measure. The authors reported that the subjects in the EMDR plus milieu treatment "did better than the Control group across all variables and generally at statistically significant levels" (p. 340). They also reported that the EMDR resulted in greater change than the biofeedback and relaxation groups. These conclusions, however, are not justified by a number of methodological limitations: nonrandom assignment, a nonvalidated outcome measure, and statistical tests without protection against Type 1 error (Lohr et al., 1995). Moreover, the comparison of EMDR with the other control conditions was confounded by the concurrent administration of milieu treatment. Thus, it is impossible to draw any valid conclusions regarding the incremental effect of EMDR.

Vaughan et al. (1994) recruited 36 trauma victims, 28 of whom met diagnostic criteria for PTSD. Seventeen of the subjects were first chosen for a wait list. All subjects were then randomly assigned to one of three treatment conditions: (a) EMDR, (b) Imagery Habituation Training (IHT), or (c) Applied Muscle Relaxation Training (AMT). IHT involves repetitive audio presentation of the trauma scenario accompanied by written self-monitoring of cognitions and affect, while AMT is an anxiety-management technique developed for the treatment of phobia (Öst, 1989). Neither has been identified as a validated treatment for PTSD (Foa & Meadows, 1997; Keane, 1998). Analysis of clinician ratings and self-report of symptoms indicated that all groups improved significantly compared with the wait list, but that there were no differences between treatment conditions. *Post-hoc* multiple *t*-test comparisons suggested that subjects in the EMDR condition experienced fewer flashbacks, nightmares, and avoidance symptoms after treatment relative to all treatment groups. Caution must be exercised, however, in the interpretation of a genuine effect of EMDR. It is unclear from the published report whether the multiple *t*-tests were alpha-protected to reduce the probability of Type 1 error. In addition, the improvement of symptoms in all conditions suggests the operation of nonspecific factors, such as treatment credibility, expectation of improvement, or common factors among the treatments. In summary, these studies provide little evidence that EMDR provides benefits beyond attention control conditions.

Nonspecific effect controls. The nonspecific factors in an experimental treatment procedure include the incidental effects of treatment, such as credibility, expectation for improvement, experimental demand, therapist-experimenter enthusiasm, and therapist-experimenter allegiance. An additional nonspecific factor relevant to EMDR is effort justification (Axsom & Cooper, 1985; Cooper, 1980). Derived from cognitive dissonance and self-perception theories, effort justification refers to the positive evaluation of a goal following the expenditure of effort to achieve this goal. Cooper, for example, found that snake phobics showed the same improvement from imaginal exposure and intense exercise, perhaps because subjects in both groups perceived a need to justify their efforts. The same, or similar process, may operate in EMDR exposure and other exposure-based treatments.

Two studies have been conducted with procedures that involve the control for some of the attentional artifacts and incidental effects of EMDR. Hazlett-Stevens, Lytle, and Borkovec (1996) randomly assigned participants with traumatic memories to one of three treatment conditions: (1) EMDR, (2) an identical procedure that employed eye fixation, or (3) nondirective counseling. The results showed that the nondirective counseling condition produced the same effects as EMDR on three out of four measures. These findings, in the context of the control procedures, do not rule out the possibility that any reliable change is due to nonspecific effects of treatment.

Scheck, Schaeffer, and Gillette (1998) randomly assigned women with traumatic memories to two sessions of either EMDR or an Active Listening (AL; Gordon, 1974) control. EMDR and AL were administered by different groups of therapists. Outcome measures included standardized self-report measures of trauma, depression, and self-concept. Data analysis showed statistically significant improvement on all measures for both treatment conditions. At posttreatment, the EMDR group showed more therapeutic improvement than the AL group on four of five measures. The EMDR group showed posttreatment means that were more comparable to normative means than did the AL group. 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. Such factors as therapist allegiance, enthusiasm, or involvement could have contributed to the measured effects of EMDR (Gaffan et al., 1995). Finally, because the AL condition provided no explicit exposure to traumatic imagery, Scheck et al.'s findings do not exclude the possibility that the effects of EMDR are mediated entirely by imaginal exposure. The last issue is crucial in light of the claim that imaginal exposure is an *incidental* feature of EMDR relative to eye movements and the modification of maladaptive cognitions (Shapiro, 1995, pp. 21–27).

Controls for characteristic features. The theory underlying EMDR's efficacy has been based on the characteristic features of eye movements or other lateral stimulation (Shapiro, 1994a, 1994b, 1995). However, research has shown

that imagery without eye movement results in reliable change of comparable magnitude on the same outcome measures (Boudewyns & Hyer, 1996; Boudewyns et al., 1993; Devilly et al., 1998; Feske & Goldstein, 1997; Foley & Spates, 1995; Gosselin & Matthews, 1995; Hazlett-Stevens et al, 1996; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996; Renfrey & Spates, 1994; Sanderson & Carpenter, 1992). Three studies show that substitution of eye movement with alternative stimulation results in improvement (Bauman & Melnyk, 1994; Foley & Spates, 1995; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996), and one study shows no change (Wilson, Silver, Covi, & Foster, 1996). However, with the exception of Wilson et al., change from pre- to posttreatment can be attributed to measurement artifact, attention, or nonspecific effects of the treatment procedures.

Moreover, the same studies comparing EMDR with a no-movement control show no difference in immediate efficacy (Boudewyns & Hyer, 1996; Boudewyns et al., 1993; Devilly et al., 1998; Foley & Spates, 1995; Gosselin & Matthews, 1995; Hazlett-Stevens et al., 1996; Muris & Merckelbach, 1997; Sanderson & Carpenter, 1992) or long-term efficacy (Devilly et al., 1998). Feske and Goldstein (1997) have shown short-term, but not long-term, differences in favor of EMDR. Dunn, Schwartz, Hatfield, and Weigele (1996) showed the superiority of EMDR on subjective ratings but not on standardized or psychophysiologic measures. When EMDR has been compared with an alternate stimulation control, three of four studies have shown no significant difference (Bauman & Melnyk, 1994; Foley & Spates, 1995; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996; Wilson et al., 1996).

One apparent exception to this trend derives from the study by Wilson et al. (1996), which showed that only the complete EMDR procedure resulted in improvements on verbal report and psychophysiologic indices. Nevertheless, this study is seriously flawed for several reasons. The first involves heart rate and blood pressure measures of questionable reliability and validity. The second involves procedural confounds in the administration of psychophysiologic recording and in the administration of the treatments. Lastly, the statistical analyses of pre- to posttreatment comparisons were conducted without the necessary statistically significant interaction between treatment conditions and pre–post interaction, and the comparisons among conditions at posttreatment were conducted without statistical protection for Type 1 error (D. L. Wilson, personal communication, February 10, 1997). See Lohr et al. (1998) for a more detailed analysis of Wilson et al. (1996). Given these limitations, and the results of other research manipulating EMDR components, we conclude that any apparent change following EMDR is most likely to be a function of the imaginal exposure that it shares with the various control conditions.

Comparison with effective treatments. If the novel treatment shows a stronger, more general, and longer lasting effect than an empirically supported treatment, or if it more efficiently attains the same results, it will also accrue incremental efficacy. EMDR has been compared directly with only a validated

treatment for spider phobia (Muris & Merckelbach, 1997; Muris et al., 1997; Muris et al., 1998). All three studies showed that both EMDR and exposure reduced verbal report of fear, but only exposure treatments resulted in reduction of behavioral avoidance. The authors (Muris et al., 1998) conclude exposure remains the treatment of choice for phobia, and that EMDR offers no additional benefits.

Pitman, Orr, Altman, Longpre, Poiré, & Macklin (1996) have addressed the comparative effects of EMDR and imaginal exposure (flooding) when discussing a companion study (Pitman, Orr, Altman, Longpre, Poiré, Macklin, Michaels, & Steketee, 1996). These authors suggest that EMDR is the preferable treatment because it produces similar effects as prolonged exposure, it is more easily tolerated, and produces fewer adverse complications. Cahill and Frueh (1997), however, have examined both studies and concluded that several methodological limitations (e.g., different inclusion-exclusion criteria, nonrandom assignment, treatment/medication confounds) render any suggestion of relative efficacy as premature. Moreover, the reviews of psychosocial treatments for PTSD by Foa and Meadows (1997) and Keane (1998) indicate that the methodological limitations of EMDR outcome studies on PTSD make EMDR as yet an unvalidated treatment (see Lohr et al., 1998). Thus, there is no published empirical evidence for assertions of incremental efficacy for EMDR based on comparisons with other treatments (Shapiro, 1996b). Indeed, any empirical effect of EMDR appears to be a function of imagery or *in vivo* exposure, which is the characteristic feature of behavioral and cognitive-behavioral treatments for several anxiety disorders (Hecker, 1990; Rachman, 1989).

Treatment generality. The generality of EMDR effects appear to be limited to verbal report indices. Psychophysiologic indices have been included in eight studies (Bates et al., 1996; Boudewyns et al., 1993; Boudewyns & Hyer, 1996; Devilly et al., 1998; Dunn et al., 1996; Muris et al., 1997; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996; Wilson et al., 1996), but only Wilson et al. has shown a therapeutic effect relative to a component control condition. However, the psychophysiologic indices of Wilson et al. were inadequately assessed, procedural confounds may have existed, and the data were subjected to inadequate statistical analyses that increased the risk of Type 1 error (see above and Lohr et al., 1998). The remaining studies showed no therapeutic effect of EMDR relative to a no treatment (Bates et al., 1996), *in vivo* exposure treatment (Muris et al., 1997), or component control conditions (Boudewyns et al., 1993; Boudewyns & Hyer, 1996; Devilly et al., 1998; Dunn et al., 1996; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996). Similarly, overt behavioral measures have been used in six studies and none showed a therapeutic effect relative to no treatment (Bates et al., 1996; Foley & Spates, 1995), nondirective treatment (Hazlett-Stevens et al., 1996), *in vivo* or imaginal exposure treatment (Muris & Merckelbach, 1997; Muris et al., 1997; Muris et al.,

1998), or component control conditions (Foley & Spates, 1995; Hazlett-Stevens et al., 1996).

Wilson, Becker, and Tinker (1997) report maintenance of EMDR treatment effects on verbal report and interview indices over a period of 15 months, but they did not compare these effects with any control condition. Moreover, no long-term effects of EMDR have been found when comparing EMDR to component control conditions (Devilly et al., 1998; Feske & Goldstein, 1997). For a more detailed analysis of these studies, see Lohr et al. (1995) and Lohr et al. (1998).

STRONG TESTS OF THE INCREMENTAL EFFICACY OF EMDR

The published research on EMDR indicates there is little evidence for the *incremental* efficacy of EMDR beyond nonspecific factors or common processes involved in imaginal exposure treatments. We believe, however, that these studies have not been conducted with full consideration of the nature and potential influence of nonspecific effects (Borkovec, 1985; Brody, 1985; Grübaum, 1985) of EMDR. Strong tests and strong inferences require rigorous control of the characteristic and incidental features of the treatment.

Procedural Requirements for Outcome and Process Research

Foa and Meadow's (1997) review of psychosocial treatments for PTSD identified several procedural variables against which the empirical validity of treatments should be judged (pp. 453–455): the use of clearly defined target symptoms, use of reliable and valid measures of outcome, use of trained, "blind" outcome assessors, use of manualized and specific treatments, use of measures of treatment adherence, and unbiased assignment to experimental conditions. Although these procedures are necessary for internal validity, they are insufficient for the control of nonspecific treatment effects. Standard experimental procedures (wait-list and attentional controls) provide for the assessment and manipulation of only the most general, nonspecific effects of treatment (Borkovec, Kaloupek, & Slama, 1975; Mahoney, 1978; O'Leary & Borkovec, 1978).

Experimental Controls

EMDR is a clear example of a treatment that is based on an explicit and testable theory (Shapiro, 1995). If the theory of treatment considers eye movements or other lateral stimulation (e. g., finger taps, auditory stimulation) to be the most characteristic feature of the treatment, then control conditions manipulating eye movements and alternate stimulation are necessary. In a more recent statement, however, Shapiro (1996a) has asserted that:

> 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. . . . Remove the eye movement and there is still a very powerful method.

If eye movements (or other lateral stimulation) are not necessary, or if other stimulation is somehow fungible with eye movements, then it is incumbent on the treatment's proponents to specify the characteristic and incidental features of the treatment. If the proponents are unable to specify these characteristics (Fensterheim, 1996; Hyer & Brandsma, 1997; Parnell, 1996), the task falls to independent researchers to conduct strong experimental tests of EMDR's incremental efficacy. A number of such potential experiments are described below.

Experimental Series Assessing Specific and Nonspecific Factors

Examination of the EMDR procedure (Shapiro, 1991, p. 133; Shapiro, 1995, pp. 17–27; Tolin et al., 1995) reveals two characteristic features of the treatment: external stimulation (eye movements, lateral touching), and the modification of attributions regarding the significance of the affective image. In the latter case, the therapist evokes a negative self-referential cognition that habitually accompanies the memory, and helps construct an alternative positive cognition that is "installed" following the reduction of negative affect (reattribution). It is this component of the procedure that served as the rationale for the name change from Eye Movement Desensitization to Eye Movement Desensitization and Reprocessing (Shapiro, 1991, p. 133; Shapiro, 1995, pp. 26–27). The remaining components (Shapiro, 1995, pp. 55–62), including imagery exposure, self-monitoring of emotional disturbance (Subjective Units of Discomfort [SUD] ratings) and physical sensations are incidental in that they are common to other cognitive behavioral treatments, such as systematic desensitization, flooding, and *in vivo* exposure. The following experiments are designed to test the characteristic features of EMDR as a specific treatment.

The most important experimental controls for strong tests would rely on additive and subtractive designs (Nezu, 1986; Nezu & Perri, 1989; Rehm, Kornblith, O'Hara, Lamparski, Romano, & Volkin, 1982) to identify and test the characteristic and incidental features of EMDR. These experimental procedures and design elements also will provide for the ethical treatment of research participants (O'Leary & Borkovec, 1978).

I. A strong test of the effect of eye movement per se would require random assignment of participants to the following conditions:
   1. Wait-List Control
   2. Partial EMDR (deleting only eye movements)

3. Pseudo-EMDR (deleting eye movements but including static stimulation such as a blinking light; Devilly et al., 1998)
4. Complete EMDR

Comparison of groups 2, 3, and 4 with group 1 would provide for the assessment of the most basic incidental effects of treatment. The comparison of group 4 with groups 2 and 3 would provide for the assessment of the characteristic effects of eye movements.

II. A strong test of the effect of alternative stimulation would require:
    1. Wait-List Control
    2. Partial EMDR (deleting only eye movements)
    3. Alternative EMDR (deleting eye movements and substituting alternative stimulation such as finger-tapping; Foley & Spates, 1995; Pitman, Orr, Altman, Longpre, Poiré, & Macklin, 1996)
    4. Complete EMDR

Comparison of groups 2, 3, and 4 with group 1 would provide for the assessment of the most basic incidental effects of treatment. The comparison of group 4 with groups 2 and 3 would provide for the assessment of the characteristic effects of alternative stimulation and for the equivalence of eye movements and alternate stimulation.

III. A strong test of the effect of reattribution would require:
    1. Wait-List Control
    2. Partial EMDR (deleting only reattribution)
    3. Pseudo EMDR (deleting reattribution but including an alternative neutral statement matched for credibility [i.e., "I am a human being."])
    4. Complete EMDR

Comparison of groups 2, 3, and 4 with group 1 would provide for the assessment of the most basic incidental effects of treatment. The comparison of group 4 with groups 2 and 3 would provide for the assessment of the characteristic effects of reattribution per se.

IV. A strong test of the combined effects of eye movements and reattribution would require:
    1. Wait-List Control
    2. Non-EMDR (deleting both eye movements and reattribution [imaginal exposure])
    3. Partial EMDR (deleting only eye movements)
    4. Partial EMDR (deleting only reattribution)
    5. Complete EMDR

Comparison of group 5 to groups 2, 3, and 4 would provide for the assessment of the combined and separate effects of the characteristic features of eye

movements and reattribution. Comparison of groups 3 and 4 would provide for the assessment of the relative effects of the characteristic features. Direct comparison of groups 5, 2, and 1 would provide for the assessment of the relative effects of EMDR and imagery rehearsal, which is the characteristic feature of imaginal exposure treatments. Direct comparisons would be predicated on equating the duration of imagery exposure by means of a yoking procedure (Dunn et al., 1996; Lohr, Kleinknecht, Conley, Dal Cerro, Schmidt, & Sonntag, 1992).

V. A strong test of the specific, relative effect of EMDR and a validated treatment would require:
   1. Wait-List Control
   2. Attention Control (Active Listening; Scheck et al., 1998)
   3. Imaginal or *in Vivo* Exposure (for phobia: Muris & Merkelbach, 1997; Muris et al., 1997; Muris et al., 1998)
   4. Complete EMDR

Comparison of groups 3 and 4 with groups 1 and 2 would provide for the assessment of nonspecific effects in both treatments, and the comparison of group 3 with group 4 would provide for the assessment of the efficacy of EMDR relative to a validated treatment.

Although these five experiments would provide for strong inferences regarding the characteristic and incidental features of EMDR, they would also impose a substantial burden upon scientific and human resources. Thus, it might superficially appear that EMDR is being held to a higher empirical standard than other treatments (Greenwald, 1997; Rogers, 1996). However, the philosopher David Hume (1748/1977) cautioned that remarkable claims require remarkable evidence. We believe that the control conditions suggested here are commensurate with the very strong claims that have been made for EMDR's efficacy. These claims include its rapidity of effect, permanence, range of applicability, and superiority relative to extant treatments (Shapiro, 1995, 1996b; Shapiro & Forrest, 1997). Without such research it is impossible to subject these unusually strong claims of efficacy to the risk of falsification, much less test the validity of the theoretical processes by which efficacy is reported to occur.

THE SCIENTIFIC SCRUTINY OF EMDR

The most essential feature of science is the maximization of criticism (Bartley, 1984) through skeptical inquiry. Genuine science does not seek confirmatory evidence, but rather seeks errors in theories and procedures so that such errors can be eliminated. It is through the systematic elimination of those errors that scientific knowledge grows. Thus, the best and most efficient way of

rooting out error in our beliefs is to expose them to severe criticism and strong empirical tests.

It could be argued that Shapiro (1989, 1995) adopted existing elements of cognitive-behavioral therapies and guided imagery techniques, and then added finger and eye movements to make EMDR appear distinctive. This distinctiveness may largely account for EMDR's extraordinary popularity. However, EMDR has also become the focus of critique and controversy (Carrigan & Cahill, 1995; DeBell & Jones, 1997; Foa & Meadows, 1997; Lohr, 1996; McNally, 1996, 1999; Metter & Michelson, 1993; O'Donohue & Thorp, 1996; Rosen, 1996; Singer & Lalich, 1996). Had EMDR been put forth simply as another variant of extant treatments, we suspect that much of the controversy over its efficacy and mechanisms of action could have been avoided.

Treatments that purport to be novel or unique in their effects may adventitiously incorporate exposure and other components that are characteristic of other effective treatments. The onus lies with the proponents of these techniques to demonstrate that their efficacy does not derive entirely from nonspecific factors (Klein, 1996a, 1996b) or well-established mechanisms of change that are incidental to the novel theory and procedure. We believe that the current research has not established the incremental efficacy of EMDR. Unless and until more rigorous tests of the nonspecific and characteristic features of EMDR are conducted, there is insufficient empirical support to justify its clinical application. We hope that our analysis of nonspecific effects and experimental procedures will help to improve the quality of the scientific research on EMDR and other treatments for anxiety disorders.

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