Statistical and Reliable Change with Eye Movement Desensitisation and Reprocessing: Treating Trauma within a Veteran Population

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Abstract

Fifty one war veterans with post traumatic stress disorder symtomatology were randomly allocated to one of three conditions: two sessions of eye movement desensitisation and reprocessing (EMDR), an equivalent procedure without the eye movements, or a Standard Psychiatric Support control condition. There was an overall significant main effect of time from pre- to post-treatment, with a reduction in symptomatology for all groups. However, no statistically significant differences were found between the groups. Participants in the two treatment conditions were more likely to display reliable improvement in trauma sypmtomatology than subjects in the control group. By six month follow-up, reductions in symptomatology had dissipated and there were no statistical or reliable differences between the two treatment groups. Overall the results indicated that, with this war veteran population, improvement rates were less than has been reported in the past. Also, where improvements were found, eye movements were not likely to be the mechanism of change. Rather, the results imply that other non-specific or therapeutic processes may account for any beneficial effects of EMDR.

Post Traumatic Stress Disorder (PTSD) has recently become the focus of many treatment-outcome studies investigating the efficacy of intervention techniques (e.g. Foa, Rothbaum, Riggs, & Murdoch, 1991; Foa, Hearst-Ilkeda, & Perry, 1995; Wilson, Becker, & Tinker, 1995). One of the most controversial interventions is that proposed by Shapiro (1989a) involving ocular tracking of extreme hand movements while holding in mind a representative picture of the traumatic event. This approach (Eye Movement Desensitization - EMD) was first investigated by Shapiro (1989a) and the method of treatment has gradually developed to the point of workshop training level and been given the title of Eye Movement Desensitization and Reprocessing (EMDR). Until now very little systematic research has examined the effectiveness of EMDR with appropriate subject numbers, controls and outcome measures. There were three aims of the present study. The first was to provide an appropriate experimental methodology of the EMDR procedure, which tests hypothesised mechanisms of change, using veterans suffering from PTSD. The second was to ascertain the extent of its beneficial effects, and the third was to test the theory that ocular tracking is a necessary component of the treatment.

Shapiro (1989b) compared two groups of "traumatised" subjects (N=22) by giving one treatment session of either EMDR or a control condition. The subjects were survivors of trauma from either the Vietnam war or 'personal abuse'. The EMDR group was administered one treatment session of EMDR and the control group was given the same instructions as the treatment group but did not receive instructions regarding the eye movements. On the measures used (subjective verbal reports), immediate outcome indicated superiority for the treatment condition and one month and three month follow-up evaluations indicated no relapse. In reference to the original "EMD" process, Shapiro (1989b) claimed that this outcome "clearly indicates that a single session of the EMD procedure is effective in

desensitizing memories of traumatic incidents and changing subjects' cognitive assessments of their individual situations" (p. 216). However, this study had many methodological flaws that have been outlined by other authors (e.g. Acierno, Hersen, Van Hasselt, Tremont, & Meuser, 1994), such as a small sample size, lack of diagnostic clarity, lack of standardised measures, an inadequate control condition and therapist demand effects.

The vast majority of case study reports have supported the efficacy of EMDR, but this may be a product of the impetus to publish and the preference for the reporting of 'successful' case studies. Furthermore, although this method of evaluation may be useful in the description of the therapeutic intervention and in the generation of hypotheses for future, large scale, research (Wilson, Becker, & Tinker, 1995), many of the case studies were poorly described, contained methodological flaws, and provided very little in the way of strong empirical support for EMDR. Lohr, Kleinknecht, Conley, Dal Cerro, Schmidt, & Sontag (1993) and Acierno, Hersen, Van Hasselt et al. (1994) succinctly offered critiques of the procedure and methodology in these case studies. Acierno, Tremont, Last, & Montgomery (1994) reported one of the few "unsuccessful" case studies using a classical design, with a procedural control phase. This research emphasises the need for procedural controls which test for the importance of non-specific effects.

Quantitative studies of treatment outcome using EMDR currently conflict in their conclusions. As a result, much debate surrounding their methodology has occurred. Wilson, Becker , & Tinker (1995) outlined some of the issues and attempted to rectify these problems with their own treatment-outcome investigation. They administered three 90-minute sessions of EMDR to 80 participants displaying PTSD symptomatology (according to the PTSD-Interview). These participants were assigned to either an immediate treatment group or a delayed treatment group. Outcome measures comprised SUD scores, the Impact of Events

Scale, the Spielberger State-Trait Anxiety Inventory, and the Symptom Check List. The treatment condition displayed significant decreases in presenting complaints of anxiety, and increases in positive cognitions when compared to the wait list controls, eliciting an average treatment effect size of 1.82. It was also found that the wait list group, when subsequently treated with EMDR, similarly improved and that treatment efficacy was not mediated by trauma type. However, this investigation did not control for placebo effects. Furthermore, these participants were seen as part of the therapists' private practice, which may have introduced extraneous effects into this study.

Foley & Spates (1995) reported a dismantling design with EMDR, designed to investigate the necessity of the eye movements using students who displayed anxiety for public-speaking as subjects. These participants received one of four conditions: EMDR; EMDR with a moving audio stimulus to replace the eye movements; EMDR with the eyes kept stationary (focused on the therapist's hand); or a no-treatment control condition. Foley & Spates (1995) concluded that eye movements were not necessary and that a decrease in symptomatology was not uniform across measures. It appeared that participants improved in all conditions except the no treatment condition, suggesting the importance of demand and expectancy effects or the influence of imaginal exposure to anxiety related stimuli. This sample, however, was not drawn from a clinical population. The results of Foley & Spates (1995) support those of Sanderson & Carpenter (1992) who did use a clinical population, comparing EMDR to image confrontation. Unfortunately, Sanderson & Carpenter (1992) used SUD scores as the their only outcome measure. These scores are open to demand effects, which limits the comparability of their findings with those from other studies.

One of the major issues in the EMDR procedure, as it currently stands, is whether it is

necessary to include eye movements as an element of treatment. However, to date, there has been only two controlled trials assessing the necessity of eye movements in EMDR utilising a clinical sample with the inclusion of appropriate controls. Renfrey & Spates (1994) reported upon twenty-three PTSD subjects who received either the EMDR procedure, the EMDR procedure facilitated by a light tracking task, or a variant of the EMDR procedure without the eye movements. The results indicated that both the EMDR condition and the EMDR procedure without the eye movements were effective in reducing PTSD symptomatology. This led the authors to suggest that eye movements were not a necessary part of the procedure. These results are strengthened by Pitman et al. (1996), who utilised a cross-over design with 17 Vietnam veterans with chronic PTSD, who received either an eye-fixed or eye-movement condition. This study displayed only moderate improvements in both conditions and again suggested that eye movements may not be the therapeutic mechanism of the EMDR procedure.

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

Likewise, Foa & Meadows (1997) proposed standards for the evaluation of treatments. After a review of the evidence, they reached different conclusions with respect to the efficacy of EMDR, stating "The test of the efficacy of this much-discussed treatment awaits adequately

controlled studies" (p. 470). Similarly, Keane (in press) is less convinced than Shapiro with regard to the currently available research data.

The primary purpose of the current study was to investigate the efficacy of EMDR and the necessity of eye movements. Subjects were treated with EMDR are compared with subjects treated with a procedure which was equivalent to EMDR in all respects except that it lacked the eye movements. Based upon past research (Foley & Spates, 1995; Renfrey & Spates, 1994) it was expected that both procedures would be effective in producing reduced distress in clients with PTSD symptomatology. A 'Standard Psychiatric Support' condition (SPS) was included to provide a control to evaluate the efficacy of both methods compared to having no additional intervention above standard psychiatric support.

This study used a sample of combat veterans, displaying PTSD symptomatology as measured by a battery of standardised measures. The study also attempted to control for therapist demand effects (using a postal follow-up), treatment expectancy and the credibility of the intervention. Furthermore, physiological indices (when imagining the trauma) were recorded and correlated with outcome and also provided an ecologically valid measure of change in arousal.

Method

Design

This study used a mixed groups experimental design with three conditions: (a) EMDR treatment, (b) EMDR treatment minus the eye movements (REDDR) and (c) Standard Psychiatric Support (SPS). Time was treated as a repeated measure. Longitudinal measures were collected at pre-treatment, end of the second treatment session, post-treatment (postal, 2 weeks after the second treatment session) and 6 month follow-up (postal). The same therapist administered both types of intervention in an effort to control for therapist variables, and had

been fully trained in EMDR (see section on Treatment Procedures). Participants were assigned to their experimental condition using a stratified randomization technique. However, random assignment was not complete. The first 20 referrals were randomly assigned in blocks of 5 to either EMDR or REDDR. Following receipt of ethical approval to include a no additional treatment condition, the next 10 participants were allocated to the SPS group. Subsequent cases were randomly assigned to EMDR, REDDR or SPS.

Participants

Participants were either self-referred (7.8%) or referred by psychologists, psychiatrists and social workers from a Vietnam veterans counselling service or a repatriation hospital (90.2%). One subject was also referred by an armed forces referral source.

Inclusion criteria for all groups were (a) aged over 18 years, (b) having served in the armed forces and been involved in combat, and (c) having undergone a traumatic experience during this service and currently displaying DSM-III-R symptomatology for PTSD. Independent diagnostic interview checks were not performed as the PTSD-I was administered as part of the reseach protocol. Exclusion criteria included (a) currently involved in a medico-legal claim with regard to their psychiatric complaints, (b) depression with suicidal ideation, judged severe enough to warrant concern for the participant's life, (c) current psychosis or organic mental dysfunction, and / or (d) had previously received EMDR.

All participants were advised that the individual results of this research would be confidential and that the Department of Veterans' Affairs would not be informed that they were taking part in the study. In this way, it was hoped that the issue of secondary gains would be considerably diminished.

The participants were male with a mean age of 50.1 years (sd = 6.48). The mean age at which these men experienced their trauma was 21.68 years old (sd = 2.45). All participants

satisfied the criteria for a lifetime diagnosis of PTSD as measured by the DSM-III-R based, clinician administered, PTSD Interview (Watson et al., 1991; psychometrics provided below). Furthermore, all subjects except two received a current diagnosis of PTSD as measured by the PTSD Interview. These two subjects both displayed severe PTSD symptomatology and were in the treatment conditions (one in EMDR and one in REDDR). It was decided to include these two subjects as both had received a diagnosis of PTSD from psychiatrists and psychologists outside of the research and it was believed that their non-classification on the PTSD-I was due to them not understanding the last question, which asks whether they had experienced the symptoms a few times each week over the past month. Both participants took this to mean whether they experienced all the symptoms (entire PTSD criteria) rather than just the symptoms to which they had responded yes (which were enough to satisfy a diagnosis of PTSD).

This study did not necessitate the participants, in any condition, to terminate any current treatment. Of those responding, 82.4% were currently on psychotropic medication with 82.4% seeing a psychiatrist, 29.4% seeing a psychologist and 73.5% being engaged with other health care professionals (social worker, day hospital, Vietnam Veterans Counselling Service, etc.). While none of the participants were or had ever received EMDR, the nature of their treatment with these other professionals / centers is not known in detail. However, none of the participants mentioned any exposure therapies to the interviewer. Checks conducted after assignment to groups confirmed that these variables were balanced across groups.

Measures

Severity of presenting complaints was assessed throughout the study (pre-, post-, and 6-month follow-up) using the following measures; the trait measure of The Spielberger State-Trait Anxiety Inventory (STAI-Y2; Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983), The

Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961), The Personal Problem Definition Questionnaire (PPD), The Mississippi scale for combat related PTSD (M-PTSD; Keane, Caddell, & Taylor, 1988). The PTSD Interview (PTSD-I; Watson, Juba, Manifold, Kucala, & Anderson, 1991), was administered at pre-test only, to provide an independent validation of diagnosis and to document problem severity. A measure of the degree of distress caused by the mental picture of the noxious memory / situation was also taken before and after intervention using the Subjective Units of Disturbance Scale (SUD; Wolpe, 1969). The Credibility of Therapy Questionnaire (COT; Borkovec & Nau, 1972) was also administered to ensure that the two treatment rationales were of equal validity, from the participants' perspective. It should be noted that the therapist (Devilly) conducted the initial interviews and this may raise concern that he was, therefore, not blind to the participants' initial presentation. However, much of the EMDR training involves taking an appropriate history, and, together with the follow-up assessments being returned by post, it is believed that the effects of this on outcome were small.

The PTSD Interview (PTSD-I; Watson et al., 1991). The PTSD-I is a DSM-III-R based diagnostic interview for PTSD and was administered at pre-treatment to give a severity / frequency measure of symptomatology and to aid in a clear diagnosis of PTSD. It demonstrates high internal consistency (alpha = .92) and test-retest reliability (Total score r = .95; diagnostic agreement = 87%), as well as showing a high level of agreement with the NIMH DIS (specificity = .94, sensitivity = .89, overall hit rate = .92). Furthermore, this measure provides a comparative index with others who have investigated the efficacy of EMDR (Wilson et al., 1995).

The Spielberger State-Trait Anxiety Inventory (STAI; Spielberger et al, 1983). A test of state (Y1) and trait (Y2) anxiety, this measure has a good level of reliability for trait anxiety (test-retest reliability coefficient of .73) and a low level for state anxiety (.33), as one would expect,

and good concurrent validity (Spielberger, 1983). As state anxiety would be expected to change over time regardless of intervention, trait anxiety was used as the outcome measure in this study.

The Beck Depression Inventory (BDI; Beck et al., 1961). The BDI was completed by the subjects to assess mood and depressive symptomatology. This measure demonstrates high internal consistency, with alpha coefficients of .86 and .81 for psychiatric and non-psychiatric populations respectively. Concurrent validity of the BDI with respect to clinical ratings and the Hamilton Psychiatric Rating Scale for Depression with non-psychiatric populations is .60 and .74, respectively (Beck, Steer, & Garbin, 1988).

The Mississippi scale for combat related PTSD (M-PTSD; Keane, Caddell, & Taylor, 1988). The Mississippi Scale for combat-related PTSD is a DSM-III-R based, 35-item, self rating questionnaire, which provides a measure of PTSD symptomatology and change in severity. This scale has attracted much support, displaying specificity of between .89 and .80, sensitivity between .93 and .94, and an overall hit rate of between .91 and .87 (Keane et al., 1988; Schlenger & Kulka, 1987; respectively). Showing a 1-week test-retest reliability of .97 and internal consistency of .94 for the whole scale (Keane et al., 1988), this measure was selected as the measure of clinical change in PTSD symptomatology.

The Personal Problem Definition Rating Scale (PPD).The PPD is a five-item rating scale designed to elicit a subjective perspective of the major problems individuals face. Participants, under the instruction of the interviewer, define the five main problems that they perceive to be currently interfering with their day to day functioning. These problems are then rated on a 9-point Likert-type scale (0-8) for severity. Although this scale is still being investigated, it is believed to be a qualitative description of individual problems that can be quantified. Therefore, it is proposed that this scale is particularly useful when working with specifically

distressed groups (e.g. PTSD).

Subjective Units of Disturbance (SUD; Wolpe, 1969). This measure comprises an 11-point Likert-type scale (0-10) to give an indication of the degree of distress caused by the mental picture of a noxious memory / situation. It has been shown to possess concurrent validity with physiological indicators of stress, such as pulse rate ($r = .39$; $p<0.05$) and peripheral vasoconstriction ($r = -.84$; $p<0.01$; Thyer, Papsdorf, Davis, & Vallecorsa, 1984). This measure was taken at all fixed time periods and during the actual treatment phase.

The Credibility of Therapy Questionnaire (COT; Borkovec & Nau, 1972). The COT was administered after the treatment method and rationale had been explained to the subject, at the start of the first treatment session. The measure was included to ensure that treatment effects were not due to subject expectancy and a greater belief in one treatment rationale compared to another. The questionnaire was designed to assess both the credibility and the expectancy for improvement generated by treatment research rationales. The results of Borkovec & Nau (1972) suggest that treatment and control condition rationales are rarely equal in credibility and that this may impact upon the non-specific factors of the treatment, even when the proposed placebo is to control for factors such as expectancy and demand.

Physiological measures. Heart rate and blood pressure (systolic and diastolic) were also recorded by a calibrated automatic monitor. Recordings were taken at rest three times and then averaged as were recordings when imagining the trauma, in that order. The averages were used for later analysis. When at rest the participant was instructed to sit in a comfortable position and "just relax". The same seating position was used when imagining the trauma and the participant was instructed to "bring to mind the representative picture of the trauma". These measures were taken at pre-treatment, before the first therapy session and at the end of the second treatment session. Measures were taken during baseline prior to commencement of the

therapy and also when the subject was imagining a "representative image" from the traumatic event. While systolic blood pressure and heart rate have been found to significantly differentiate PTSD subjects from matched controls when presented with auditory or visual stimuli (Blanchard et al., 1982; Malloy et al., 1983), no research to date has looked at these measures when the subjects are imagining a trauma. It is proposed that this method of assessment is more representative of the subjects' actual symptoms and is, therefore, more ecologically valid than the use of trauma scripts (Jensen, 1994).

The BDI, STAI-Y2, PPD and SUDs were also administered at the start of the first treatment session, prior to the actual treatment, two-weeks following the intake session. This was conducted to ascertain stability of symptomatology prior to treatment.

Treatment Procedures

Shapiro (1992) suggests that any therapist using EMDR should be properly trained by herself, or a designated trainer, to an advanced standard and that any research carried out by untrained therapists would be invalidated by this lack of 'skill'. Although there is currently no evidence to back these claims, the therapist for this study was trained by Shapiro to the full Advanced Standard (level 2) that would enable research into the technique (Shapiro, 1992). As the therapist is an active clinician and treated all subjects between 1992 and December 1995, the authors believe it is unlikely that this training had become 'dated'. Furthermore, an outline of the interventions are presented here to facilitate replication (for further detail see Shapiro, 1995).

Eye Movement Desensitisation and Reprocessing (EMDR). The rationale provided to the client was designed to be appropriate to his level of understanding, but was adapted from the following: "Traumas cause a pathological change in the brain at the neural level resulting in these incidents becoming 'locked' in the nervous system and not being processed in the normal

way and, therefore, not being dealt with. Eye movements may be the body's natural way of desensitising the person to the memory and so, inhibiting anxiety, the traumatic 'overload' becomes resolved". The actual process was as follows: Initially clients described the traumatic scenario and stated their beliefs about the event. A belief which was judged to be unrealistic or detrimental was rated upon a Validity of Cognition scale (VOC; 0 to 7, with 0 equal to completely untrue and 7 equal to completely true and believed). Next an alternative positive (realistic and adaptive) cognition was elicited from the client and this was likewise rated. While the client concentrated on an imaginal picture, representative of the traumatic event, and the accompanying body sensations, he also concentrated on the eye movements. These eye movements were induced by the therapist using bi-lateral sweeps of his first two fingers across the line of vision 12 - 14 inches away from the participant's face. Each sweep covered the extreme left and extreme right of the field of vision (at least 12 inches) at the rate of about two back and forth movements per second, although this was made slightly faster or slower depending upon the individual's ability to track the movements. Initially the direction was on a horizontal plane but if this showed to have little effect then it was changed to a diagonal, vertical or circular motion, accommodating individual client differences. On average, 24 back and forth sweeps were given for each set, although if increased anxiety was noticed then these movements were continued until a plateau in affect was reached. Throughout this procedure nurturing prompts were given; e.g., "good", "well done", "that's it". At the end of one set of eye movements the client was then instructed to "blank it (the image or thought) out and take a deep breath". Following this the client was asked whether "anything else came up". If so then this was concentrated upon for the next set of eye movements until it was desensitized. If not the client was instructed to "bring the picture / feeling / cognition up again" and assign it a SUD rating. This process was repeated until a SUD rating of 0 was obtained (no anxiety) and the

issue desensitized. The positive cognition was then concentrated upon during the eye movements until a VOC rating of 7 was obtained (completely true). This cognition and the original issue was then linked together during the eye movements, known within the EMDR protocol as "installation" of the positive cognition. Finally a body scan was completed, checking for any physiological residue. This involved the participant checking their body for any tension or ill-feeling. If any was present then the eye movements were induced again, while he concentrated on this feeling / part of the body, until it had dissipated. All treatment sessions were of a maximum 90 minutes long, although the therapy session was terminated if the images were desensitised (SUD# 1), the positive cognitions installed (VOC = 7) and there was no residual anxiety.

Reactive Eye Dilation Desensitisation and Reprocessing (REDDR). In order to control for the phonetical "impressiveness" of the title and, therefore, the possible expectancy effects of subjects, the above title and acronym were given to this treatment. The same method as outlined for the EMDR procedure was utilized for the REDDR procedure, with the following modifications. For the rationale the words "eye movement" were replaced by the words "eye dilation". For the process, concentrating on an "opticator" was used instead of eye movements. While the client concentrated on the imaginal picture and accompanying body sensations he also concentrated on a black box which emitted a flashing light. A stationary, flashing light was kept between 12 and 14 inches away from the participant's eye level and the flashes occurred at the rate of four per second, although this rate was increased or decreased depending upon the individual's ability to concentrate on the light. Initially the light was white in color, but if this displayed little effect then it was changed (using a colored perspex slide) to green, red or yellow "accommodating individual client differences". One set of flashes was presented for about 10-20 seconds, although if increased anxiety was noticed then these flashes were continued until a

plateau in affect was reached. As with the EMDR condition, all treatment sessions were a maximum length of 90 minutes long. However, the therapy session was terminated if the images were desensitised (SUD# 1), the positive cognitions installed (VOC = 7) and there was no residual anxiety.

Standard Psychiatric Support Condition (SPS). This group was a control condition in which the individuals received no intervention or support by the research therapist. They instead completed the questionnaire package that those in the other two groups were administered at 0 weeks (pre-treatment) and again at 5 weeks (post-treatment). This did not prohibit them from continuing their ongoing treatments. Questionnaires were administered by the researcher at the Brisbane Vietnam Veterans Counselling Service (VVCS) to those clients who agreed to take part.

Results

Analyses were conducted investigating; stability of symptomatology prior to treatment, experimental participant-condition allocation, pre- to post-treatment statistical and reliable change, change in SUD levels during treatment for the two treatment conditions, 6-month follow-up statistical and reliable change, and subsidiary analyses (including analysis of physiological indeces).

Stability Of Symptomatology

Participants in the two treatment conditions were administered the BDI, STAI-Y2, PPD and SUDs before the first treatment session, and all physiological indeces were also taken to ascertain stability of their symptomatology. This was conducted at the beginning of the first treatment session, before the rational or treatment were delivered. A 2-way MANOVA (2 conditions [EMDR, REDDR] X 4 repeated measures [BDI, STAI-Y2, PPD, SUDs]) conducted upon the outcome measures displayed no significant effects for Condition, Wilks'

Λ(4, 27) = .89, ns, nor Time, Wilks' Λ(4, 27) = .76, ns, and no effect for the interaction of these two, Wilks' Λ(4, 27) = .78, ns. A further 2-way MANOVA on the physiological indeces likewise displayed no significant effects for Condition Wilks' Λ(6, 21) = .69, ns, no effect of Time, Wilks' Λ(6, 21) = .83, ns, nor the interaction of these two, Wilks' Λ(6, 21) = .66, ns. It would seem, therefore, that there were no obvious, global, shifts in symptomatology before treatment over the two week waiting period.

Results For Comparisons Of The Three Groups:- EMDR, REDDR and SPS.

First Assessment: Fifty one participants completed the first assessment battery. Of these 51 subjects, 19 were assigned to the EMDR condition, 16 were assigned to the REDDR condition and 16 subjects were placed in a Standard Psychiatric Support (SPS) control condition.

A MANOVA was used to compare pre-treatment levels of PTSD between the three conditions for the PTSD-I and the M-PTSD. There were no statistical differences, at the 95% confidence interval, between the three conditions on these measures (Wilks' Λ(4, 90) = .93, ns). Also, to assess concurrent validity, the PTSD-I and M-PTSD were correlated and found to be significantly related ($r(49) = .76$, $p < .01$). Addressing the associated measures of pathology (BDI, STAI-Y2, SUD, PPD) a 1-way MANOVA again demonstrated no significant difference between the groups (Wilks' Λ(8, 88) = .78, ns). Likewise, measures of physiology (blood pressure - systolic and diastolic - and heart rate) when at rest and when imagining the trauma, displayed no statistical differences at the 95% confidence interval between the EMDR and REDDR conditions (Wilks' Λ(6, 26) = .77, ns). This suggests that there were no significant biasing effects in the selection of conditions for patients, based upon the first assessment, and that patients in these 3 conditions (EMDR, REDDR and SPS) displayed similar symptomatology at baseline.

Systolic and diastolic blood pressure and pulse rate at rest were compared to these same measures when imagining the trauma, for all subjects before group assignment, using dependent t-tests. A significant elevation from rest to imagining the trauma was indicated for heart rate ($t(32) = -2.57$, $p < .02$), systolic blood pressure ($t(33) = -4.78$, $p < .001$) and diastolic blood pressure ($t(33) = -4.78$, $p < .03$). Therefore, imagining the trauma was associated with an increase in physiological arousal compared to rest and not imagining the trauma.

Post-treatment: Thirty four participants completed and returned the postal battery. Of these, 13 were in the EMDR condition (6 drop-outs), 12 were in the REDDR condition (4 drop-outs) and 10 were in the SPS condition (6 drop-outs). The mean values for all outcome measures from pre- to post-treatment are shown in Table 1 (sections a).

A series of repeated measures analyses of variance were conducted for the dependent variables STAI-Y2, BDI, M-PTSD, SUD and PPD[1]. Bonferroni corrections were not applied to these analyses as it was seen that these data are measuring different aspects of pathology. This would also allow the experimental conditions the greatest latitude to display any differences and avoid leaving the researchers open to the criticism of a type II error. Effect sizes (Cohen's d) for interaction effects are reported after each analysis between the two treatment groups, EMDR and REDDR[3].

Symptoms of PTSD (M-PTSD) again displayed a significant effect for Time, $F(1, 31) = 7.55$, $p < .01$, but not for Condition, $F(2, 31) = .48$, ns, nor the Condition X Time interaction, $F(2, 32) = 2.87$, ns ($d = .5^{E}$). The measure of depression (BDI) revealed a significant effect for Time, $F(1, 32) = 7.79$, $p < .009$, but neither a significant effect for Condition, $F(2, 32) = .14$, ns, nor a Condition X Time interaction effect, $F(2, 32) = 2.22$, ns ($d = .63^{E}$). Trait anxiety (STAI-Y2) displayed a significant effect for Time, $F(1, 32) = 5.41$, $p < .03$, but neither a significant effect for Condition, $F(2, 32) = .73$, ns, nor for the interaction between Condition and Time,

F(2, 32) = 2.78, ns (d = .74[E]). Ratings of personal problems identified at the first assessment (PPD) evidenced a significant effect of Time, F(1, 30) = 16.80, p < .001, and again no effect for Condition, F(2, 30) = .12, ns, nor the Condition X Time interaction, F(2, 30) = .39, ns (d = .29[E]). Subjective reports of anxiety when imagining the trauma (SUD) displayed a significant effect over Time, F(1, 32) = 16.98, p < .001, but for neither the main effect of Condition, F(2, 32) = .80, ns, nor the Condition X Time interaction, F(2, 32) = .31, ns (d = .11[E]).

Planned contrasts between the two treatment conditions and the SPS condition displayed no significant differences for any of the outcome measures (α=.05).

Reliable Change. Although statistical significance may show a main effect for Time or Condition, this may not translate into a meaningful change clinically, when the reliability of the instrument is taken into account (Jacobson & Traux, 1991). In order to judge whether individuals displayed improvement, a reliable change index was developed to measure those who improved or deteriorated (p<.05) over time. The methods used for these analyses were those suggested by Jacobson & Traux (1991)[2]. As the M-PTSD was the only direct outcome measure for traumatic symptomatology, this scale was used in the determination of reliable change. Other measures were not utilized for clinical change to reduce the probability of a type I error. Also these other measures are not trauma specific and hence have less bearing on the presenting complaints of this very distinctive population.

In the EMDR condition 8/12 (66.67%) subjects reliably improved compared to 5/12 (41.67%) in the REDDR condition, and only 1/10 (10%) in the SPS group. The 'improved' group was compared to the 'unimproved' group for the two treatment groups using a two tailed Fisher's exact test with Bonferroni corrections (α < .025) for two analyses. No significant group differences appeared (p = .41), implying that neither procedure was more effective in producing 'general improvement'. The two treatment conditions (EMDR and REDDR) were collapsed

together on these same two variables generating a 'treatment condition' for 'improved' and 'unimproved'. As it was expected a priori that the treatment conditions together would produce more change than the control condition, this 'treatment condition' was then compared to the control group (SPS) using a Bonferroni corrected ($\alpha < .025$), one tailed, Fisher's exact test. The control condition was found to display significantly more 'unimproved' cases than the combined treatment groups ($p = .019$).

Results For Comparison Of The Two Treatment Groups:- EMDR and REDDR.

Changes in SUD levels during treatment: Thirty one participants received the second and final treatment session, of which 16 were in the EMDR condition and 15 were in the REDDR condition. A repeated measures analysis of variance was applied to SUD scores for imagined trauma scenes, comparing baseline levels with those at the end of session 2.

SUD levels dropped significantly from baseline measures, $F(1, 29) = 88.92$, $p < .001$, for subjects in both treatment conditions, but there was no difference for the main effect of condition, $F(1, 29) = 0.142$, ns, and no interaction effect, $F(1, 29) = 2.70$, ns ($d = .57^{R}$). SUD levels decreased from $\bar{O}$=7.31 (SD=1.7) to 3.56 (SD=3.10) for the EMDR condition and from $\bar{O}$=7.87 (SD=1.51) to 2.53 (SD=2.13) for the REDDR group.

6 Month Follow-up: Nineteen participants completed and returned the 6 month follow-up battery, of whom 10 were in the EMDR condition and 9 were in the REDDR condition[4]. Table 1 (sections b) represents the mean values for all dependent variables, for the subjects in the two treatment conditions who completed the assessments at pre-treatment and 6 month follow-up.

Repeated measures analysis of variance indicated no significant differences for the main effects of Condition or Time and the interaction of these two, for STAI-Y2 ($d = .42^{E}$), BDI ($d = .07^{R}$) and M-PTSD ($d = .15^{R}$). The only effects noted were for the subjective ratings of the Personal Problem Definition Questionnaire and Subjective Units of Disturbance Scale[5]. The

PPD displayed a significant effect of Time, $F(1, 17) = 15.93$, $p < .001$, but no significant difference on the main effect of Condition, $F(1, 17) = .16$, ns, nor the Condition X Time interaction, $F(1, 17) = 1.35$, ns ($d = .53^{E}$). Likewise, SUD ratings revealed a significant effect for Time, $F(1, 17) = 8.11$, $p < .012$, but not for Condition, $F(1, 17) = 1.34$, ns, nor the Condition X Time interaction, $F(1, 17) = .11$, ns ($d = .15^{E}$).

Reliable Change. As with the post-treatment data, subjects who had 'improved' and subjects who were 'unimproved', as measured by the M-PTSD, were computed relative to pre-treatment. This displays a profile supporting the statistical analyses. There was no difference between the EMDR and the REDDR group, and now only three (33.33%) in each condition were 'generally improved' compared to six (66.67%) who had either not changed or had deteriorated.

Subsidary Analyses.

The credibility ratings for the two treatment conditions were compared using an independent groups (EMDR and REDDR) t-test. The Credibility of Therapy questionnaire (COT) was administered to all participants just after the treatment rationale was given, and just before the first treatment session. There was found to be no significant difference in the credibility of the two treatment rationales (EMDR $\bar{O}$=26.9 and REDDR $\bar{O}$=31.0) $t(30) = -1.482$, ns. Furthermore, Pearson product-moment correlations displayed no significant relationships between the treatment credibility and change scores for all outcome measures at post-treatment or the 6 month follow-up ($\alpha = 0.05$).

To compare the physiological indices, the difference in scores, for systolic and diastolic blood pressure and pulse rate, were computed at rest and when imagining the trauma for the pre-treatment data. These differences were computed again for the data collected at the end of session 2. Three analyses of variance (using a Group (2) by Time (2) design) were applied to

systolic and diastolic blood pressure and heart rate.

There emerged no Condition X Time interaction effect (α = .05) for either systolic or diastolic blood pressure, or for pulse rate. The effect size for this interaction was computed and displayed no sizeable effect for systolic (d = .07[E]) and diastolic (d = .18[R]) blood pressure and pulse rate (d = .01[E]). Again there was no significant effect for Condition on any of these dependent variables, although there was a significant effect for Time. The increase in diastolic blood pressure from rest to imagining the trauma at post-treatment was significantly less compared to the elevation at pre-treatment, across both groups, $F(1, 24) = 4.28$, $p < .05$. This effect also approached significance for pulse rate, $F(1, 23) = 4.18$, $p = .052$, but not for systolic blood pressure, $F(1, 24) = 1.29$, ns.

Discussion

This research examined the efficacy of EMDR with a sample of war veterans suffering from PTSD symptomatology and the necessity of eye movements in the EMDR process. The results suggest that EMDR is not as effective as initially claimed (Shapiro, 1989b), at least with a war veteran sample. The findings also imply that eye movements are unlikely to be the modus operandi of the little change that was apparent.

From pre- to post-treatment, no statistically significant differences were found between the three conditions on any of the outcome measures. While all three conditions improved over time on measures of trait anxiety, depression, subjective ratings of disturbance and PTSD pathology, there was no statistically significant difference in the magnitude of this improvement between the three conditions. Although improvements for the EMDR condition appeared slightly larger than those of the comparison groups for some of the measures, this was not statistically significant. Certainly the treatment effects were not consistent with the large effect sizes reported in past research favourable to this method of intervention (i.e., a reported effect

size for EMDR of 1.82; Wilson et al., 1995). With this level of effect size, one would expect the number of subjects per cell in this study to be more than great enough to demonstrate the superior efficacy of EMDR should it exist.

The control condition in this study (SPS) displayed statistically similar short-term symptomatology to the treatment conditions. However, at post-treatment there was a reliably significant difference between the treatment and no treatment conditions on our sole outcome measure of PTSD symptomatology. Thirteen out of the twenty four participants who received a treatment intervention reliably improved on the Mississippi scale for combat-related PTSD. This compares to only one out of ten participants in the Standard Psychiatric Support condition displaying a reliable improvement. However, this was conducted for only one measure, and this finding should not be over-generalized.

The results of the present study indicate that by 6 months follow-up, treatment effects had diminished to the point that changes from pre-treatment were no longer statistically significant for trait anxiety, depression and PTSD symptomatology for either treatment. Furthermore, there was no longer a trend for EMDR to display slightly greater improvement. This was also supported by the decline in effect size of the interaction from post-treatment to 6 month follow-up. With respect to reliable change, only 3 participants in the EMDR and 3 in the REDDR conditions displayed any improvement with respect to a direct measure of PTSD pathology, with 6 in both groups displaying no improvement. Again, the authors warn against over-generalization of this outcome measure to other areas of functioning.

Participants in both the EMDR and REDDR groups displayed a significant decrease in SUD levels, in response to trauma imagery at the end of the second treatment session in comparison to pre-treatment levels. However, this effect diminished for both groups over time. Although this improvement was still significant for both groups at 6-month follow-up, it had

attenuated to the extent that remembering the trauma still caused much distress 6-months after the treatment. It should also be noted that while there was statistical improvement at post-treatment for all standardized measures and for some at 6-month follow-up, the mean scores are still high and within the clinical range.

These results are in contradiction to Wilson et al. (1995) and Shapiro (1989b), who reported a continued trend towards recovery in symptomatology for 3-month and 6-month follow-up, respectively. One possible explanation for the differences in long term efficacy of EMDR found in the present study compared to Shapiro (1989b) and Wilson et al. (1995) concerns the method used for follow-up assessment. It is possible that the use of postal data in the current study reduced the demand characteristics to report improvement and that the effects of therapist allegiance (Gaffan, Tsaousis, & Kemp-Wheeler, 1995) were accordingly reduced.

The finding that outcome measures for both treatments displayed statistically reliable improvement for a similar number of patients from pre- to post-treatment, suggests that eye movements are not essential to the efficacy of EMDR and that other components in the procedure are probably responsible for any therapeutic effects. This aspect of the research concurs with Foley & Spates (1995), who reported equivalent results for EMDR and a similar process without eye movements, for public-speaking anxiety with a non-clinical sample.

Shapiro (1989b) claimed that the eye movements were necessary for the effects of EMDR to become apparent. However, in 1993 she did agree that "research is needed to determine what aspects of the eye movements (e.g. rhythmical, bilateral, external orientation, attention-focusing) and of the complete procedure are responsible for its effects" (Shapiro, 1993; p. 421). Shapiro (1996) now claims that "external stimuli" (p. 209) are important during the process. The current authors argue that such a timely change in perspective does not fully

incorporate the results of this study when taken in context with other research (Sanderson & Carpenter, 1992; Renfrey & Spates, 1994; Foley & Spates, 1995).

The EMDR technique makes use of imaginal exposure for prolonged periods. It is now widely accepted that prolonged imaginal exposure has beneficial effects on outcome in the treatment of PTSD (Foa & Meadows, 1997). The eye movements or dilations may act as a distraction for small periods of time, yet allow and encourage exposure for longer periods of time. In this way, the participants may have become habituated to the memory. In addition to this, if an increase in anxiety occurs during this exposure the therapists are required to continue with the EMDR (imaginal exposure) until there is a plateau in affect, thus prohibiting the escape behaviours associated with anxiety disorders.

Other properties of the EMDR procedure that are not specific to this intervention include: the generation of a treatment expectancy with a credible rationale, reciprocal inhibition and cognitive restructuring (e.g. Dyck, 1994; Boudewyns & Hyer, in press; Renfrey & Spates, 1994).

It is believed that the slight improvement of the SPS group is due to a number of factors. Firstly, the participants in the SPS group were mostly receiving treatment, although not EMDR, from sources outside of the research (as were most of the participants in the other two conditions). Although this may appear to be a flaw in the design, it was believed, a priori, that the acquisition of veterans who were not receiving any form of intervention, and would be willing to take part in the research, would be so low as to make the study impractical. It was considered ethically unsound to bar these subjects from their ongoing treatments during the research. Furthermore, given that they had been receiving such interventions for several years with minimal improvement, it was considered unlikely that sudden clinical gains would occur over the brief monitoring period of the study. It was also believed that permitting ongoing clinical contacts provides an ecologically valid strategy for a study involving combat veterans,

and that this methodology provides us with a true "control norm" with which to compare. Another reason for the mild improvement of the control condition is the possible reactive effect of completing the questionnaire battery. During administration of these questionnaires the participants were forced to engage in a certain amount of imaginal exposure. For example, when obtaining the SUD levels they were asked to imagine a representative picture of their trauma. It is possible that this imaginal exposure was enough to prohibit a significant effect between groups, but not to produce clinically significant change for this condition.

This study also demonstrated that treatment credibility was not associated with outcome. It was also found that the methodology utilized in the gathering of physiological data was sensitive to emotional arousal, where imagining a representative picture of the trauma produced an increase in blood pressure and heart rate. For both treatment groups it was found that there was a decrease in physiological reactivity between rest and imagining the trauma at post-treatment, compared to at pre-treatment.

This study attempted to use a specific trauma group (combat veterans) to lessen the introduction of confounding variables related to trauma type. However, this population has a propensity to move house and state, and thus the drop-out rate was high. The most common reason for attrition between intake and the post-treatment assessment was the veteran moving back to a country region and not returning the postal outcome measures. Of those in the treatment conditions, only one participant dropped-out between the first and second treatment sessions. Indeed it is believed that one major reason for the attrition from our study between post-treatment and follow-up was the use of an uninterfered, postal, reply protocol. However, considering that the follow-up data were collected by mail, a drop-out rate of less than 40% by 6 months is considered to be a good response rate. Furthermore, while Vietnam veterans are notoriously resistant to change, it was believed to be an appropriate group with which to test

EMDR. The original study by Shapiro (1989b) displayed change in all subjects after one session and this research also utilized Vietnam veterans within the subject pool. This point should also be strongly considered when discussing the length of treatment offered in this project. EMDR has been hailed as a cost-effective and powerful technique by its proponents, and it was indeed this claim that originally caught the attention of the field. This research offered two treatment sessions, double the number originally (Shapiro, 1989b) and hitherto (Shapiro, 1994) claimed as necessary for a large effect. Furthermore, the authors believe that all efforts were made to reduce the effects of secondary gains, by informing the participants of the confidentiality of the research.

A possible criticism of the research is with respect to treatment fidelity. The same therapist delivered both treatments and only one Veteran agreed to be video-taped. Therefore, plans for independent ratings of the procedures had to be dismissed. It is believed, though, that this did increase the veterans' confidence in the confidentiality of the research and reduced the secondary gains for appearing to maintain symptomatology. However, it should be noted that the therapist in this study was initially more practiced and confident with the EMDR procedure and the authors concede that without independent ratings some critics may argue that this confidence and level of expertise worked to the detriment of the REDDR condition. Given the results of the study still others may argue for philosophical inadherence by the therapist to the EMDR procedure. It should be stressed that the primary therapist was initially very enthusiastic about the possibility of a 'cure' for PTSD. This weakness was unavoidable and, if possible, future research should utilize the original design which allowed for raters - independent of the research, and without personal investment in the EMDR debate - to complete fidelity checks, which would negate the charge of therapist bias. Furthermore, the treating therapist also conducted the initial assessments and the authors also accept that, while this is the norm in

usual clinical settings, a therapist blind to the participants presenting level of pathology would make for more technically sound research.

Given the results of the present study, it seems important that future treatment-outcome studies use appropriate control conditions that mimic the non-specific factors of therapy, while testing the hypothesized active components. This applies not just to EMDR but also to other therapeutic interventions, both old and new. Although past studies on EMDR have reported their results not to be trauma or diagnostic specific (Marquis, 1991), future studies into EMDR should also use a subject pool different from combat veterans, with standardized outcome measures comparable to those used previously. This will ensure that the results of this study were not sample dependent.

In summary, the results of the present study did not provide support for the efficacy of the EMDR procedure with combat related PTSDsymptomatology, as reported by Shapiro (1989b). Our results displayed modest short-term benefits on all outcome measures, most of which were no longer evident by 6-month follow-up. The current study also provides support for the hypothesis that eye movements are unlikely to be the mechanism of change in the EMDR procedure, and that other, non-specific factors, such as imaginal exposure, are likely to be important.

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Table 1.

Pre- and Post-treatment, and 6 Month Follow-up Means (and S.D.) for the Three Conditions

Measure	Data Set	Condition	Pre-treatment	Post-treatment	Follow-up
Mississippi Scale for PTSD	a	EMDR (N=12)	120.42 (26.48)	110.42 (27.72)	---
		REDDR (N=12)	123.17 (18.93)	118.58 (22.64)	---
		SPS (N=10)	110.90 (22.54)	111.20 (24.77)	---
	b	EMDR (N=9)	118.33 (26.89)	---	115 (33.26)
		REDDR (N=9)	127.44 (19.41)	---	122.44 (21.05)
Beck Depression Inventory	a	EMDR (N=13)	28.77 (12.03)	21.23 (15.47)	---
		REDDR (N=12)	28.25 (11.21)	26.50 (13.70)	---
		SPS (N=10)	26.33 (10.20)	24.50 (11.74)	---
	b	EMDR (N=10)	27.80 (13.64)	---	25.50 (16.49)
		REDDR (N=9)	26.00 (12.18)	---	23.11 (13.01)
Spielberger Trait Anxiety	a	EMDR (N=13)	58.62 (11.64)	49.08 (17.81)	---
		REDDR (N=12)	59.92 (10.22)	59.17 (14.77)	---
		SPS (N=10)	57.36 (8.52)	55.60 (10.27)	---
	b	EMDR (N=10)	58.50 (10.99)	---	53.30 (19.32)
		REDDR (N=9)	60.78 (11.52)	---	60.11 (11.39)
Personal Problem Definition	a	EMDR (N=13)	30.85 (4.18)	23.12 (10.73)	---
		REDDR (N=11)	30.82 (4.62)	25.46 (9.97)	---
		SPS (N=9)	29.67 (6.06)	26.44 (4.50)	---
	b	EMDR (N=10)	33.20 (4.08)	---	24.70 (11.50)
		REDDR (N=9)	30.00 (4.90)	---	25.33 (8.78)
		EMDR (N=13)	7.54 (1.61)	5.23 (2.65)	---

Subjective Units of Disturbance	a	REDDR (N=12)	8.17 (1.40)	6.17 (3.04)	---
		SPS (N=10)	8.01 (1.77)	6.60 (3.13)	---
	b	EMDR (N=10)	7.60 (1.58)	---	5.50 (3.31)
		REDDR (N=9)	8.33 (1.22)	---	6.67 (2.40)

Note. Data Set = The data which were used for analyses: a = results from pre- to post-treatment; b = results from pre-treatment to 6 month follow-up.

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Footnotes

[1] A 2 x 3 MANOVA ("Time" X "Condition" [EMDR, REDDR, SPS]), with "Time" as a repeated measure, applied to the dependent variables STAI-Y2, BDI, M-PTSD, SUD and PPD, displayed a similar profile.

[2] The standard error of measurement, in the derivation of the reliable change index, was calculated by using the standard deviation of the pre-treatment within groups variance (sd = 20.56), as all subjects are considered to be from a clinical sample with regard to PTSD. The test-retest reliability of the Mississippi scale for PTSD is 0.97, and thus the standard error of the difference was computed to be 5.04.

[3] The direction of the effect size is denoted by ^{E} for EMDR and ^{R} for REDDR.

[4] Some of the questionnaires were returned later than 6 months and a Mann-Whitney U test displayed no significant difference between the two conditions on the lateness of arrival for the 6 month follow-up, U = 11, ns.

[5] A 3 x 2 MANOVA ("Time" [pre-, post-intervention, 6 month follow-up] X "Condition" [EMDR, REDDR]), with "Time" as a repeated measure, applied to the dependent variables STAI-Y2, BDI, M-PTSD, SUD and PPD, produced similar findings. There was no effect for condition, Wilks' $\Lambda(5, 9) = .51$, ns, nor time, Wilks' $\Lambda(10, 4) = .147$, ns, and no interaction between condition and time, Wilks' $\Lambda(10, 4) = .23$, ns. However, it should be noted that the power of this test, with such a small N, is very low (.19 for the interaction)