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POWER THERAPIES: EVIDENCE VS MIRACULOUS CLAIMS

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Abstract. Poole, de Jongh and Spector ask for empirical research rather than emotive arguments when evaluating EMDR. When one applies this standard, Poole et al.'s remaining points are devoid of substance. EMDR, like other Power Therapies, is a "miracle" cure that has failed.

Keywords: EMDR, PTSD, placebo, sham therapies.

In response to our recent commentary on "Power Therapies" (Rosen, Lohr, McNally, & Herbert, 1998), Poole, de Jongh and Spector (1999) make several points. They argue that EMDR should not be linked with other power therapies such as TFT, TIR, and EFT. They accuse us of failing to address the literature and fault us for citing review papers written by one of our "group". They conclude that studies on the role of eye movements are equivocal. They suggest that numerous controlled studies have demonstrated "greater rapidity of treatment effects" for EMDR relative to other exposure methods. They observe that EMDR has been listed by a committee of the American Psychological Association as "probably efficacious" for the treatment of civilian PTSD

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and they challenge our characterization of the method as a "cure that has failed". Finally, they urge that empirical research rather than unbalanced reviews and emotive arguments should prevail. We agree with this last point; empirical research should prevail. In the context of this common ground, let us consider why other points made by Poole et al. are devoid of substance.

Figley (1997) rightly linked EMDR with TFT, TIR, and EFT. Advocates of all these methods claim their techniques work faster than traditional cognitive-behavioural methods and produce astounding results. With reference to EMDR, consider the dust jacket of the founder's most recent book which describes the method as an "extraordinary" and "profound" procedure that appears to be a "miracle" and "the most revolutionary, important method to emerge in psychotherapy in decades" (Shapiro & Forrest, 1997). Clearly, the extraordinary claims made on behalf of EMDR place it within the province of other "Power Therapies".

Studies discussed in Lohr, Tolin and Lilienfeld's (1998) comprehensive review demonstrate that eye movements are unnecessary and superfluous to whatever effects EMDR may have. Readers can consult that review, read the cited studies, and determine for themselves how the data fall. In our view, this issue is not equivocal; it is settled. Simply put, eye movements do not add to treatment outcome beyond the effects produced by established procedures such as exposure. Therefore, what is effective in EMDR is not new, and what is new is not effective. Attempts on the part of Stickgold, van der Kolk, and others to "explain" the effects of eye movements are nothing more than an explanation in search of an effect.

The notion that EMDR has "greater rapidity of treatment effects" relative to other exposure methods is another unsubstantiated claim made on behalf of EMDR. This claim rests on comparisons across studies which, unfortunately, tell us very little (Cahill & Frueh, 1997). Thus, we do not know if patients and treatments across studies are truly comparable, and we do not know if particular experimenter or therapist effects operated in one study but not in another. Only between-group comparisons within the same study can address this issue adequately. When researchers conduct such comparisons, they find that EMDR is actually less effective than traditional exposure methods (Muris, Merckelbach, Holdrinet, & Sijsenaar, 1998; Devilly & Spence, in press).

EMDR enthusiasts suggest that studies failing to support EMDR have not employed the method faithfully, whereas studies demonstrating positive effects have good "treatment fidelity". This argument may sound reasonable at first; but, in fact, EMDR enthusiasts have misunderstood and therefore misused the treatment fidelity concept (Rosen, in press). A full discussion of this important issue is beyond the scope of this paper, but a few brief points warrant comment. One error made by EMDR enthusiasts is that they confuse measures of treatment outcome with indices of treatment fidelity. As a consequence, their arguments become a circular process of reasoning that offers no insight. EMDR enthusiasts also have continually changed the procedures and levels of training that define faithful adherence to the method, and they have done this without supporting data. Thus, Shapiro has proclaimed (without evidence) that "Level II" training is required for a therapist to learn EMDR, yet once upon a time "Level I" training was adequate, and before that simple written descriptions were deemed sufficient. This has created a seemingly endless catch-up game for scientists. By the time a Level I trained researcher has published data, Level II training is required and the

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earlier study is said to lack fidelity. One can easily comprehend how this "bait and switch" strategy has created a slippery slope on which refuted hypotheses constantly change, and the data never win.

Poole et al. may excuse Shapiro's "Accelerated Informative Processing" explanation of EMDR on the basis that it is simply a "model", but this does not remove it from the realm of neurobabble. Nor does the table they provide showing numbers of subjects and treatment sessions across diverse studies have any useful meaning beyond an apparent numbers game. Even the claim of greater client acceptability and satisfaction has failed a recent test (Devilly & Spence, in press).

Finally, Poole et al. observe that EMDR has been assessed by a Committee of the American Psychological Association as "probably efficacious" in the treatment of civilian PTSD. Many view this development as a sad reflection of the Committee's criteria rather than a ringing endorsement of EMDR. The reader should appreciate that "probably efficacious" only refers to "better than no treatment". Thus, any treatment that adds an inert ingredient (such as eye movements) to traditional components of cognitive and behavioural therapies will produce effects. We then must ask if every variant of this sort should be listed as "probably efficacious". For example, if phobic patients received *in vivo* exposure with a strong cognitive component and inert magnetic head bands, should the Review Committee provide a specific endorsement for ElectroMagnetic Desensitization and Reprocessing?

Conclusion

After considering the "evidence" as Poole et al. urge us to do, we are left with the same conclusion presented in our original paper (Rosen et al., 1998). EMDR and other Power Therapies have followed the historical pattern of miracle cures that fail the tests of time and science.

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