

Comparison of the Effectiveness of Existential Therapy and Schema Therapy in Reducing Death Anxiety among Women with Multiple Sclerosis

Liah. Aqa Ali Akbari¹, Mahdieh. Rahmanian^{2*}, Amin. Rafieipour³

¹ Department of Clinical Psychology, K.I.C., Islamic Azad University, Kish, Iran

² Professor, Department of Psychology, Payame Noor University, Tehran, Iran

³ Associate Professor of Health Psychology, Faculty of Psychology, Payame Noor University, Tehran, Iran

* Corresponding author email address: m.rahmanian@pnu.ac.ir

E d i t o r	R e v i e w e r s
Ramakrishna Biswal [✉] Assistant Professor, Department of Psychology, National Institute of Technology Rourkela Rourkela, India biswalrk@nitrkl.ac.in	Reviewer 1: Mohsen Kachooei [✉] Assistant Professor of Health Psychology, Department of Psychology, Humanities Faculty, University of Science and Culture, Tehran, Iran. kachooei.m@usc.ac.ir Reviewer 2: Nadereh Saadati [✉] Department of Couple and Family therapy, Alliant International University, California, United States of America. mdaneshpour@alliant.edu

1. Round 1

1.1. Reviewer 1

Reviewer:

The participant description should include clinically relevant MS characteristics that are currently absent. The manuscript identifies the population only as “married women with multiple sclerosis” but does not report disease subtype, disease duration, disability severity, relapse status, or current neurological stability. These variables could substantially influence death anxiety and responsiveness to psychotherapy. At minimum, the authors should report disease duration and MS phenotype, and, if available, Expanded Disability Status Scale scores, recent relapse history, and disease-modifying treatment. Baseline equivalence among the three groups should also be assessed for these variables.

The manuscript states that participants were assessed at “pretest, immediately after completion of the interventions, and again at the planned follow-up assessment,” but the exact follow-up interval should be stated consistently throughout the Methods and Results. The intervention descriptions suggest a two-month follow-up, yet the general study-design paragraph does not specify this. The authors should explicitly state that follow-up occurred two months after posttest, if that was the actual interval, and clarify whether all 45 participants completed the follow-up assessment or whether attrition occurred.

In the Data Collection Tool paragraph, the authors describe the Templer Death Anxiety Scale in detail but do not report the psychometric properties of the instrument in the present sample. The statement that “test-retest reliability of approximately .83 and internal consistency based on the Kuder-Richardson method of approximately .76 were reported” refers to prior validation work and does not establish reliability in this study. The authors should calculate and report an appropriate internal consistency coefficient for the current sample, preferably KR-20 because the items are dichotomously scored, and should clarify which validated Persian version of the scale was administered.

The statistical analysis strategy requires reconsideration because Table 2 presents two separate repeated-measures analyses—existential therapy versus control and schema therapy versus control—rather than a single omnibus model incorporating all three groups. The primary research question concerns the comparative effectiveness of existential therapy, schema therapy, and control; therefore, the most coherent analysis would be a 3×3 mixed repeated-measures ANOVA or an equivalent linear mixed-effects model with group as a three-level between-subject factor and time as a three-level within-subject factor. The authors should either reanalyze the data using a unified model or provide a strong statistical rationale for performing separate pairwise repeated-measures analyses.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The scoring description of the Death Anxiety Scale should be made more precise. The manuscript states that “responses are summed to obtain a total score, with higher scores indicating greater death anxiety,” but it does not specify the theoretical score range or whether any items are reverse-scored. Because the observed baseline means are approximately 9–10, readers need to know that the instrument consists of 15 dichotomous items and the resulting score range is 0–15. The authors should also indicate the scoring direction for true/false responses and cite the specific Persian validation source if applicable.

The schema therapy protocol is described extensively, but the manuscript needs additional information regarding treatment fidelity. The sentence “The schema therapy intervention was developed based primarily on the conceptual framework proposed by Young, Klosko, and Weishaar and was delivered in 10 group sessions” should be followed by details on who delivered the treatment, the therapist’s professional qualifications, prior schema-therapy training, use of a manual, supervision procedures, and whether session adherence was independently monitored. Without these data, it is difficult to determine whether the intervention was implemented consistently and whether observed effects can be attributed to schema therapy rather than therapist-specific factors.

The existential therapy protocol similarly requires clearer specification of therapist competence and fidelity. The manuscript states that the intervention “was developed on the basis of Yalom’s existential psychotherapy framework,” but several techniques listed in the sessions—such as cognitive defusion, mindfulness, emotion-focused interventions, nonviolent communication, and sexual assertiveness training—derive partly from other therapeutic traditions. The authors should explain the theoretical rationale for integrating these techniques, clarify whether the intervention should be characterized as pure existential therapy or an integrative existential protocol, and provide sufficient information to ensure conceptual fidelity to the treatment label.

The intervention comparison may be influenced by nonspecific treatment factors, and this issue should be addressed explicitly. Both active groups received 10 sessions of 75 minutes, which appropriately controls for treatment duration, but the control group apparently received no equivalent attention. The authors should clarify whether the control condition was wait-list, treatment-as-usual, or no-treatment control. Because therapist contact, group support, expectancy, and therapeutic alliance can independently reduce distress, the Discussion should acknowledge that the design does not isolate specific treatment mechanisms from common psychotherapy factors.

In the assumptions paragraph, the statement that “The Kolmogorov-Smirnov test indicated that the distributions of death anxiety scores did not significantly deviate from normality” is insufficiently detailed. The authors should report the actual test statistics and p-values, or preferably complement formal normality testing with inspection of skewness, kurtosis, Q-Q plots,

and residual distributions. With only 15 participants per group, normality tests have low power to detect deviations, so relying solely on nonsignificant Kolmogorov-Smirnov results is not methodologically robust.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor's decision: Accepted.

Editor in Chief's decision: Accepted.