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Comparing the Effectiveness of Young's Schema Therapy and Intensive Short-Term Dynamic Psychotherapy on Psychological Resilience in Kidney Patients

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1. Round 1

1.1 Reviewer 1

Reviewer:

In the abstract, the sentence “both approaches were equally effective” should be softened because the study was not designed as an equivalence or non-inferiority trial. A nonsignificant difference between schema therapy and ISTDP does not statistically demonstrate equal effectiveness; it only indicates that no statistically significant superiority was detected in this small sample. The authors should replace this wording with “no statistically significant difference was observed between the two active treatments” and avoid causal or equivalence language unless an equivalence margin and adequate power analysis are provided.

In the introduction, the paragraph beginning “Kidney patients, particularly those who require repeated medical follow-up or dialysis…” would benefit from a clearer specification of the target renal population. The manuscript alternates between “kidney patients,” “chronic kidney disease,” and patients requiring dialysis, but the inclusion criteria do not specify CKD stage, dialysis status, transplantation status, disease duration, or renal diagnosis. Since psychological burden and resilience may differ substantially across predialysis CKD, hemodialysis, peritoneal dialysis, and transplant candidates, the introduction should align more precisely with the actual recruited population.

In the introduction, the sentence “Resilience refers to the capacity to maintain or regain psychological balance after stress, adversity, or trauma” is appropriate, but the authors should expand the theoretical rationale for why resilience is the primary outcome rather than depression, anxiety, quality of life, treatment adherence, or illness perception. The introduction establishes that kidney disease produces psychological burden, but it should more clearly justify resilience as a clinically meaningful and intervention-sensitive construct in renal care, especially because the study uses only one psychological outcome.

In the paragraph describing intensive short-term dynamic psychotherapy, the sentence “By increasing emotional tolerance, insight, and adaptive affect regulation, intensive short-term dynamic psychotherapy may enhance the patient's capacity to face stressors associated with chronic illness” requires stronger linkage to renal populations. The current rationale is theoretically plausible but generic. The authors should add empirical evidence from ISTDP or emotion-focused psychodynamic interventions in medical or psychosomatic populations, or explicitly acknowledge that direct evidence in kidney patients is limited and that the current study is exploratory.

In the paragraph describing schema therapy, the sentence “Evidence from health-related and clinical populations suggests that maladaptive schemas can be associated with depression and psychological vulnerability in kidney- and liver-related contexts” is too narrow to support the intervention rationale. The cited evidence appears to involve donors rather than kidney patients undergoing illness management. The authors should strengthen this paragraph by explaining which specific maladaptive schemas are expected to be activated by chronic kidney disease and how schema change would theoretically increase resilience.

In the Interventions section, the sentence “no independent treatment-fidelity rating was conducted” identifies an important limitation, but the manuscript should also state whether manuals, checklists, supervision, session recordings, or therapist adherence logs were used. For psychotherapy trials, fidelity monitoring is central to determining whether the intended treatment was actually delivered. The authors should provide enough procedural detail to allow replication and to reduce concerns about therapist drift or overlap between schema therapy and ISTDP techniques.

In the Measure section, the sentence “Psychological resilience was assessed using the Connor-Davidson Resilience Scale” should include the score range and interpretation more explicitly. Since the CD-RISC has 25 items scored 0–4, total scores range from 0 to 100, and this should be stated. The authors should also clarify whether the Persian version was used, whether permission was obtained if required, and whether the factor structure or prior validation evidence applies to kidney patients rather than only Iranian students.

In the Statistical Analysis section, the sentence “equality of covariance matrices across repeated measures using Mauchly's test of sphericity” is technically inaccurate. Mauchly’s test assesses the sphericity assumption for within-subject factors, not equality of covariance matrices across groups. Equality of covariance matrices is typically assessed using Box’s M test in multivariate contexts. The authors should revise this statement and specify whether Greenhouse–Geisser or Huynh–Feldt corrections would have been applied if sphericity had been violated.

Authors revised the manuscript and uploaded the updated document.

1.2 Reviewer 2

Reviewer:

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In the final paragraph of the introduction, the sentence “The main research question was whether these two therapeutic approaches differ in their effect on psychological resilience across pretest, posttest, and three-month follow-up assessments” should be accompanied by explicit hypotheses. The manuscript would be stronger if it specified whether the authors expected both treatments to outperform control, whether one treatment was hypothesized to be superior, and whether effects were expected to persist at follow-up. This would improve the logical connection between theory, design, analysis, and interpretation.

In the Methods section, the paragraph beginning “The statistical population consisted of kidney patients in Mashhad, Iran, in 2023” requires more detailed recruitment information. The authors should state where patients were recruited from, such as dialysis centers, nephrology clinics, hospitals, or community settings; who screened participants; how eligibility was verified; how many patients were approached; how many declined; and whether any participants withdrew before posttest or follow-up. Without this information, the risk of selection bias and attrition bias cannot be evaluated.

In the Sample Size and Allocation section, the sentence “No formal a priori statistical power analysis was reported” is commendably transparent, but it should be moved from a descriptive statement into a fuller methodological limitation and supplemented with justification. Given the small sample size of 15 per group, the authors should clarify whether this was a pilot or feasibility study and avoid presenting the findings as confirmatory. A post hoc detectable-effect discussion may also help readers understand the precision and limitations of the treatment comparison.

In the Sample Size and Allocation section, the sentence “Group allocation was performed after recruitment to keep the three study arms equal in sizeInclusion criteria were…” contains a typographical and formatting error. The missing space or sentence break between “size” and “Inclusion” should be corrected. More importantly, the phrase “to keep the three study arms equal in size” does not explain the allocation mechanism. Equal group size alone does not indicate whether allocation was random, block-based, matched, or nonrandom.

In the inclusion criteria, the phrase “absence of psychotic-level depression” is conceptually unclear and should be revised. Depression and psychosis are distinct clinical constructs, and “psychotic-level depression” could mean major depressive disorder with psychotic features, severe depression, or psychotic symptoms in general. The authors should specify the diagnostic screening method, the instrument or clinical interview used, who conducted the assessment, and whether other major psychiatric conditions were excluded.

In the Interventions section, the sentence “The schema therapy intervention consisted of eight sessions based on Young's protocol” requires more detail about session duration, delivery format, therapist qualification, therapist training, and whether sessions were individual or group-based. Similarly, the ISTDP intervention consisted of fifteen sessions, but the manuscript does not report session length or therapist competence. These omissions are critical because psychotherapy dose, therapist expertise, and delivery format can substantially influence outcome.

In the Interventions section, the comparison between eight sessions of schema therapy and fifteen sessions of ISTDP raises a serious treatment-dose confound. The authors should explicitly justify why the two interventions had unequal numbers of sessions and discuss how this affects interpretation of comparative effectiveness. If the purpose is to compare therapeutic models, unequal contact time makes it difficult to determine whether outcomes are due to treatment content, therapist attention, expectancy, or intervention intensity.

Authors revised the manuscript and uploaded the updated document.

2. Revised

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Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.

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