






The Effectiveness of Acceptance and Commitment Therapy on Non-Suicidal Self-Injury and Executive Functions in Patients with Borderline Personality Disorder




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E d i t o r	R e v i e w e r s
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1. Round 1

1.1 Reviewer 1

Reviewer:

In the Introduction, the statement “effective treatment requires interventions that can address both emotional distress and the maladaptive behavioral strategies used to manage it” is conceptually sound but insufficiently integrated with current treatment guidelines for BPD. The authors should provide a more comprehensive comparison of ACT with established interventions such as Dialectical Behavior Therapy (DBT), Mentalization-Based Therapy (MBT), and Schema Therapy, explaining the specific rationale for selecting ACT over these evidence-based approaches.

The paragraph describing NSSI functions presents intrapersonal and interpersonal functions as the central outcomes, yet no theoretical justification is provided regarding why these dimensions were prioritized over NSSI frequency, severity, medical lethality, or recency. The authors should explain why functional dimensions were selected as primary outcomes and discuss their clinical significance relative to more commonly reported NSSI indicators.

The paragraph beginning with “Executive functions are another important domain in BPD” would benefit from greater conceptual precision. Executive functioning is a multidimensional construct involving cognitive flexibility, working memory,

inhibition, planning, and monitoring. However, the manuscript later operationalizes executive functioning exclusively through the BDEFS. The authors should justify why a self-report measure was chosen instead of objective neuropsychological tests and discuss the implications for construct validity.

The hypothesis section states that ACT would improve “time self-management, self-organization/problem-solving, self-restraint/inhibition, self-motivation, and emotional self-regulation.” These hypotheses are highly specific, yet no theoretical mechanism is proposed linking each executive-function domain to ACT processes. The authors should elaborate how acceptance, cognitive defusion, and values-based action are expected to influence these particular executive domains.

In the Participants and Setting section, the sentence “Approximately 120 eligible patients were estimated to be available in the participating centers” raises concerns regarding sampling transparency. Please report the actual number screened, the number meeting eligibility criteria, the number refusing participation, and the number ultimately enrolled. A CONSORT-style participant flow diagram would substantially improve reporting quality.

The Eligibility Criteria section explicitly acknowledges that “the study did not use an independent structured diagnostic interview, such as the SCID-5-PD.” This limitation is potentially serious because diagnostic reliability is fundamental in BPD research. The authors should provide detailed information regarding the qualifications of the clinicians who established diagnoses, diagnostic procedures employed, and inter-rater reliability, if available.

The Measures section lacks psychometric information for both the BDEFS and ISAS within the present sample. Reliability coefficients (Cronbach’s alpha, McDonald’s omega, or composite reliability) should be reported for each scale and subscale. Furthermore, evidence supporting the validity of the Persian versions used in the study should be included.

Table 1 presents the ACT intervention protocol, yet the session descriptions are too brief to permit replication. For example, Session 5 includes “values-consistent crisis coping” and “self-soothing with mindful awareness,” but the specific exercises, metaphors, experiential techniques, and homework assignments are not described. Greater procedural detail is necessary to establish treatment fidelity and replicability.

The paragraph stating “The largest effect sizes were observed in emotional self-regulation and self-motivation” provides a plausible interpretation but lacks integration with contemporary neurocognitive and emotion-regulation models of BPD. The discussion would benefit from linking findings to current theories of frontolimbic dysregulation, inhibitory control deficits, and emotion-processing abnormalities in BPD populations.

Although limitations are acknowledged, an important issue is omitted: all outcome measures were self-report instruments. This introduces the possibility of common-method bias and expectancy effects, especially when participants know they are receiving treatment. The authors should discuss how this limitation may have influenced the observed effect sizes and consider incorporating behavioral or clinician-rated measures in future studies.

The manuscript contains both a “Conclusion” section and a separate “Clinical Implications” section, followed by “Limitations and Future Directions.” Consider reorganizing these sections to improve coherence. Specifically, the Clinical Implications section should include concrete recommendations regarding patient selection, treatment settings, therapist qualifications, risk-management procedures, and integration with existing BPD treatment pathways rather than remaining at a relatively general level.

The reference list requires careful editorial review. Several references appear duplicated in different citation formats (numbered references and subsequently repeated IEEE-style entries). This duplication may confuse readers and does not conform to standard journal formatting requirements. The reference section should be standardized according to the target journal’s guidelines.

Authors revised the manuscript and uploaded the updated document.

1.2 Reviewer 2

Reviewer:

The intervention section states that “some exercises may overlap with skills commonly used in other third-wave interventions.” This statement raises concerns regarding treatment purity. The authors should explicitly identify which components may overlap with DBT or mindfulness-based interventions and explain how adherence to an ACT framework was maintained throughout treatment delivery.

The Procedure section does not report whether any participant attrition occurred between pretest, posttest, and follow-up assessments. The absence of attrition data makes it impossible to determine whether intention-to-treat analyses were necessary. Please report retention rates and explain how missing data were handled statistically.

The Statistical Analysis section reports the use of repeated-measures analysis but does not specify whether assumptions regarding multivariate normality, covariance matrices, or outliers were evaluated. Additionally, given the relatively small sample size ($n = 40$), the robustness of repeated-measures ANOVA should be justified, or alternative approaches such as linear mixed-effects modeling should be discussed.

In the Results section, the statement “the main group effect for intrapersonal NSSI functions was borderline and should be interpreted cautiously ($F = 3.85$, $p = .050$)” deserves further discussion. A p -value of exactly $.050$ occupies a threshold region and should not be interpreted as definitive evidence. Confidence intervals should be reported alongside effect sizes to facilitate interpretation.

Table 2 reports statistically significant interaction effects for NSSI outcomes, but descriptive statistics (means and standard deviations for each group at each time point) are not presented. Without these data, readers cannot evaluate the magnitude and clinical significance of observed changes. A comprehensive descriptive table should be added.

The paragraph reporting pairwise comparisons for NSSI outcomes indicates maintenance of treatment gains, yet no adjustment procedure for multiple comparisons is specified. Please indicate whether Bonferroni, Holm, Sidak, or another correction method was applied and report adjusted p -values.

In Table 3, effect sizes for executive-function outcomes are remarkably large, particularly emotional self-regulation (partial $\eta^2 = .67$) and time self-management (partial $\eta^2 = .62$). Such magnitudes are uncommon in psychotherapy research. The authors should discuss the practical interpretation of these effect sizes and address the possibility of inflation resulting from small sample size or measurement method variance.

The Discussion section attributes reductions in NSSI to decreases in experiential avoidance, yet experiential avoidance itself was not measured. Because the proposed mechanism was not directly assessed, causal interpretations regarding psychological flexibility should be presented more cautiously. Inclusion of mediator measures would strengthen these conclusions.

Authors revised the manuscript and uploaded the updated document.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.