

The Effectiveness of Dialectical Behavior Therapy on Health Anxiety, Perfectionism, and Identity Crisis among 15- to 18-Year-Old Students in Selective and Gifted Schools

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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 paragraph on the Short Health Anxiety Inventory, the manuscript describes three factors—"the main health-anxiety component, general health concerns, and perceived negative consequences of illness"—whereas the Results section uses the labels "general health concern," "illness likelihood," and "negative consequences." Please standardize the terminology throughout the manuscript and ensure that the subscale structure corresponds to the validated version actually administered. The number of items contributing to each subscale and the possible score range for each should also be reported.

The ethical procedures require substantial strengthening. The Methods section currently states that "informed consent was obtained from the participating students and their legal guardians" and that confidentiality was maintained, but no institutional ethics approval number or approving committee is reported. Because all participants were minors, the manuscript should clearly distinguish parental/legal guardian consent from adolescent assent and provide the name of the approving ethics committee, approval code, and date. The authors should also clarify whether participation or refusal had any consequences for students within their schools.

The intervention paragraph is reasonably detailed, but treatment fidelity cannot currently be assessed. The manuscript states that the intervention consisted of “eight 60-minute sessions delivered twice weekly,” yet it does not identify the therapist's qualifications, DBT training, professional experience, supervision, or whether a structured treatment manual/checklist was used. Please specify who delivered the intervention, whether the same facilitator conducted all sessions, how adherence to the protocol was monitored, and whether session attendance and homework completion were recorded.

The statement in the Results that “the Bonferroni-adjusted comparisons confirmed significantly lower total health anxiety, total perfectionism, and identity crisis scores in the experimental group” should be revised because the comparison presented in Table 3 appears to collapse across time. If the authors wish to conclude that the experimental group had lower scores “after treatment,” they need simple-effects analyses at posttest and follow-up separately. This is particularly important for identity crisis because the baseline mean was actually higher in the experimental group (57.00) than in the control group (55.00), whereas the direction reversed substantially at follow-up.

The descriptive data reveal several potentially important patterns that deserve closer examination. In the control group, identity crisis increased from 55.00 at pretest to 61.90 at follow-up, whereas it decreased from 57.00 to 48.55 in the experimental group. Likewise, some control-group perfectionism components fluctuated substantially across assessments. The Discussion currently attributes the interaction primarily to therapeutic improvement, but part of the interaction may also reflect deterioration or natural fluctuation in the control group. Please distinguish treatment-related improvement from control-group change and avoid implying that the interaction is generated exclusively by reductions in the intervention group.

The Discussion's explanation for the nonsignificant parental-pressure outcome—“this dimension reflects an external interpersonal context that may not be directly altered by an individually or group-delivered student intervention”—is plausible but currently speculative. This interpretation should be framed more cautiously and linked directly to the nature of the measured construct. Because parental behavior was not assessed independently, it is unclear whether parents actually maintained the same pressure or whether students' perception of pressure remained unchanged. Consider discussing alternative explanations, including limited statistical power, measurement sensitivity, and the relatively short intervention and follow-up periods.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

The control condition requires clearer description. The manuscript states that “the control group received no intervention during the intervention period.” Please specify whether participants received treatment as usual, school counseling, routine educational services, or no psychological contact whatsoever. This distinction is methodologically important because an untreated control cannot separate the specific effects of DBT skills from nonspecific factors such as therapist attention, peer interaction, expectancy, and repeated assessment. The authors should explicitly acknowledge this issue when interpreting intervention effects.

The statistical-analysis paragraph should specify the exact model used for repeated-measures ANOVA. The manuscript refers broadly to “repeated-measures analysis of variance,” but the design includes one between-subject factor (group: experimental vs. control) and one within-subject factor (time: pretest, posttest, follow-up). Please identify this explicitly as a 2×3 mixed repeated-measures ANOVA and state which effect constituted the primary test of treatment efficacy. In this design, the Group $\times$ Time interaction is generally more informative regarding differential treatment effects than the main effect of time.

The assumption-testing paragraph requires correction concerning normality. The manuscript states that normality was accepted because Shapiro–Wilk significance values were “above the prespecified criterion of .01,” including a follow-up identity-crisis result of $p=.033$. The conventional criterion for statistical assumption testing is generally .05 unless a different alpha level was prospectively justified. Using .01 solely to avoid rejecting normality should not be done post hoc. More importantly, normality assumptions for repeated-measures models concern residuals rather than simply raw outcome

distributions. Please justify the selected criterion and, ideally, supplement significance tests with skewness, kurtosis, Q–Q plots, or residual diagnostics.

The treatment of sphericity should be reported more completely. The manuscript correctly notes that Mauchly's test was significant for negative consequences, total health anxiety, organization, and high standards for others and states that Greenhouse–Geisser correction was used. However, Table 2 reports only F , p , and partial η^2 without corrected degrees of freedom or epsilon values. Please report the Greenhouse–Geisser ϵ and corrected numerator and denominator degrees of freedom for every outcome violating sphericity so that readers can verify the reported inferential statistics.

Table 2 should distinguish clearly between the main effect of time and the Group $\times$ Time interaction when interpreting treatment effectiveness. For example, total health anxiety showed a significant time effect, $F=6.618$, $p=.004$, but the principal evidence for intervention efficacy is the significant interaction, $F=5.418$, $p=.010$, partial $\eta^2=.125$. Similarly, identity crisis showed a nonsignificant time effect but a significant interaction. The Discussion should consistently attribute efficacy to differential trajectories between groups rather than to within-subject temporal effects alone.

The interpretation of partial eta-squared should be more precise. The Results section states that striving for excellence showed “the largest interaction effect” at approximately 26.9%, and similar percentage interpretations are used elsewhere. Partial $\eta^2=.269$ indicates that 26.9% of the variance attributable to the effect plus its associated error is associated with the interaction within that model; it should not be interpreted simply as “DBT explained 26.9% of the variable.” Please revise all effect-size interpretations accordingly and, preferably, describe them using accepted small, medium, or large benchmarks cautiously rather than presenting them as direct percentages of outcome variance explained by treatment.

Table 3 is labeled “Bonferroni-Adjusted Pairwise Comparisons Between the Experimental and Control Groups,” but the presented means appear to be averages across the three measurement occasions rather than post-intervention or follow-up comparisons. For example, the experimental total perfectionism mean of 179.767 corresponds approximately to the average of the pretest, posttest, and follow-up means. This comparison is difficult to interpret as evidence of intervention efficacy because it incorporates baseline values. Please clarify exactly what estimated marginal means were compared. More informative follow-up analyses would compare groups separately at posttest and follow-up, or compare pretest–posttest and pretest–follow-up changes while controlling for multiple comparisons.

Response: Revised and uploaded the manuscript.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.