

# Effectiveness of Rational Emotive Behavior Therapy on Oppositional Defiant Disorder and Sexual Deviance in Working Children

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### Editor

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## 1. Round 1

### 1.1. Reviewer 1

Reviewer:

In the Introduction, the paragraph beginning with “Childhood is considered one of the most critical developmental stages” provides a broad theoretical background; however, the manuscript lacks a precise epidemiological context regarding the prevalence of oppositional defiant disorder and sexually maladaptive behaviors among working children in Iran. Including national or regional prevalence data would substantially strengthen the rationale for the study and clarify the public health significance of the investigated variables.

The paragraph stating “Another serious concern among working children involves sexually deviant thoughts and behaviors” raises important clinical concerns, yet the construct of “sexual deviance” is not sufficiently operationalized for a child population aged 9–11 years. The authors should clearly distinguish between clinically inappropriate sexual behaviors, trauma-related sexualized behaviors, developmental sexual curiosity, and culturally defined deviance. The current terminology risks conceptual ambiguity and may inadvertently pathologize developmentally normative behaviors.

In the Introduction, the statement “Behavioral disturbances such as oppositional defiant disorder and sexual deviance are often rooted in dysfunctional cognitive structures” oversimplifies the etiology of these behaviors by emphasizing cognitive

mechanisms while underrepresenting trauma exposure, attachment disruption, environmental deprivation, and neurodevelopmental vulnerabilities. A more integrative biopsychosocial framework would improve the theoretical sophistication of the manuscript.

The paragraph beginning with “One of the most influential cognitive-behavioral approaches designed to modify irrational beliefs” presents the REBT framework adequately; however, the manuscript does not sufficiently justify why REBT was selected over other evidence-based approaches for traumatized or vulnerable children, such as trauma-focused CBT, play therapy, or attachment-based interventions. The authors should explain the specific theoretical and practical advantages of REBT for working children exposed to chronic adversity.

In the Methods section, the sentence “The participants were initially identified by the school psychologist as exhibiting symptoms related to oppositional defiant disorder, sexual deviance, or both behavioral conditions” requires much greater precision regarding diagnostic procedures. It remains unclear whether standardized clinical interviews, DSM-based diagnostic criteria, behavioral observations, or merely informal judgments were used for screening participants. This ambiguity weakens the internal validity of the study.

The inclusion criterion “demonstrating symptoms of oppositional defiant disorder or sexual deviance according to clinical judgment” is insufficiently standardized and potentially subjective. The authors should report whether inter-rater reliability procedures were implemented and whether symptom severity thresholds were established before participant selection. Otherwise, there is a significant risk of inconsistent participant classification.

In the Measures subsection, the description of the Oppositional Defiant Disorder Questionnaire states that it was developed based on DSM-IV criteria, while the Introduction relies on DSM-5 conceptualizations. The manuscript should explain whether the DSM-IV-based instrument remains fully compatible with DSM-5 diagnostic criteria or discuss possible discrepancies between the two diagnostic systems.

In Table 2, the reported effect sizes ( $\eta^2 = 0.90$  for both dependent variables) are extraordinarily large for psychosocial intervention research and may indicate inflation due to small sample size, measurement overlap, regression to the mean, or lack of adequate experimental control. The authors should discuss these unusually large effects more critically and consider reporting confidence intervals or adjusted effect size estimates.

The Discussion section repeatedly interprets findings in strongly causal terms, such as “REBT significantly reduced symptoms” and “REBT can serve as an effective psychological intervention,” despite the quasi-experimental design and convenience sampling strategy. The authors should adopt more cautious wording acknowledging that causal inference is limited without randomized population sampling and long-term follow-up data.

In the paragraph beginning with “The effectiveness of REBT in reducing sexual deviance behaviors may be explained through several psychological mechanisms,” the proposed explanations remain highly speculative because no mediating variables such as emotional regulation, irrational beliefs, shame, or impulse control were directly measured in the study. The manuscript should explicitly acknowledge that these mechanisms are hypothetical interpretations rather than empirically demonstrated processes.

Authors revised the manuscript and uploaded the document.

## 1.2. Reviewer 2

Reviewer:

The description of the Child Sexual Deviance Questionnaire is methodologically problematic because the reported subscales primarily assess “sources of sexual information” rather than sexually deviant behaviors themselves. The authors should clarify the conceptual validity of using exposure sources as proxies for sexual deviance. At present, the measure appears to conflate environmental exposure with behavioral pathology.

The Intervention subsection provides a general overview of the REBT sessions but lacks sufficient procedural detail for replication. The manuscript should include a structured session-by-session protocol describing therapeutic objectives, specific

cognitive restructuring techniques, homework assignments, behavioral exercises, and methods used to address sexual behavior concerns specifically.

The statement “All sessions were administered by a trained therapist certified in cognitive-behavioral therapy approaches” is too vague for publication in a scientific journal. The authors should specify the therapist’s educational background, years of clinical experience, REBT-specific training, supervision procedures, and whether treatment fidelity or adherence checks were conducted during the intervention period.

In the Data Analysis section, the authors report conducting ANCOVA after testing assumptions; however, no information is provided regarding statistical power analysis or sample size justification. Given the extremely small sample size ( $n=30$ ), the manuscript should explain how statistical power was estimated and whether the study was adequately powered to detect meaningful intervention effects.

In Table 1, the control group mean for oppositional defiant disorder remained exactly “6.14” at both pretest and posttest, with identical standard deviations “0.86,” which appears statistically improbable in behavioral research involving children. The authors should verify whether these values reflect a typographical error, data entry problem, or computational artifact.

The paragraph reporting ANCOVA assumptions contains a serious statistical inconsistency. The manuscript states that “the interaction effect between group membership and pretest scores was not statistically significant for oppositional defiant disorder ( $F = 23.77$ ,  $p = 0.630$ ).” An  $F$  value as large as 23.77 would not ordinarily correspond to a non-significant  $p$ -value of 0.630. Similar inconsistency exists for sexual deviance ( $F = 21.67$ ,  $p = 0.631$ ). These statistics should be recalculated and corrected immediately.

Authors revised the manuscript and uploaded the document.

## 2. Revised

Editor’s decision: Accepted.

Editor in Chief’s decision: Accepted.