

Effectiveness of Short-Term Psychodynamic Psychotherapy on Defense Styles, Alexithymia, and Parent–Child Relationship Quality in Adolescents with Emotional and Behavioral Problems

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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 discussing defense mechanisms, the manuscript states that “mature defenses such as sublimation, humor, anticipation, and suppression generally permit affect to be managed without major distortion of reality.” The authors should clarify the developmental appropriateness of applying the adult-derived mature/neurotic/immature DSQ-40 taxonomy to adolescents aged 13–17. Because defensive organization changes developmentally, some behaviors classified as relatively immature in adults may have different normative significance during adolescence. The Discussion should also acknowledge this issue when interpreting changes in defense scores as evidence of improved psychological maturity.

The paragraph on alexithymia correctly identifies “difficulty identifying feelings, difficulty describing feelings, and externally oriented thinking,” but the Introduction should more directly justify why alexithymia is expected to change over only 16 sessions. The authors should explain the proposed mechanism by which repeated affect clarification, verbal symbolization, and exploration of defenses within STPP could modify a relatively stable trait-like construct. This would make the later interpretation of the large Group × Time effect for alexithymia more theoretically persuasive.

In the Introduction, the statement that “short-term psychodynamic psychotherapy is particularly suited to this multidimensional formulation” is central to the study but currently somewhat assertive. The authors should distinguish STPP from broader psychodynamic, psychoanalytic, and experiential dynamic therapies and specify which model or manual most closely informed the 16-session protocol. As currently written, the intervention integrates affect focus, defense clarification, transference work, reflective functioning, and relational formulation, but the exact treatment lineage remains unclear. This limits replicability and makes it difficult to determine which evidence base is most directly relevant.

The statement that “the therapist was a clinical psychologist trained in psychodynamic psychotherapy and experienced in adolescent treatment” indicates that treatment may have been delivered by a single therapist. If so, therapist and treatment-condition effects are completely confounded. This should be stated explicitly in the Methods and emphasized more strongly in the limitations. A single highly skilled therapist may produce outcomes that cannot be attributed solely to the STPP model, and future replication should involve multiple therapists with assessment of therapist-level effects.

The control condition is described as a “wait list for 16 weeks” with access to “routine nonpsychotherapy clinical monitoring as needed for safety.” Please define what routine monitoring involved, how often it occurred, which professionals provided it, and whether the frequency differed across control participants. Because nonspecific clinical contact can influence outcomes, particularly parent–child relationship ratings and emotional distress, the amount of contact received by the control group should be quantified and reported.

In the Measures section, the manuscript reports current-sample Cronbach’s alpha coefficients of .73, .70, and .82 for mature, neurotic, and immature defense styles. However, the DSQ-40 style scores aggregate heterogeneous defense mechanisms and are based on relatively few items per defense. The authors should discuss whether coefficient alpha is the most appropriate reliability index for these higher-order style composites and, if possible, report McDonald’s omega or another reliability estimate. At minimum, the relatively modest reliability of the neurotic defense score should be acknowledged when interpreting its smaller treatment effect.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the Methods section, the sentence “A priori power estimation for a two-group repeated-measures interaction with three measurements, $\alpha = .05$, power = .80, a moderate effect ($f = .25$), and an assumed within-person correlation of .50 indicated that approximately 34 participants were required” requires substantially more detail. Please report the software used for power estimation, the assumed nonsphericity correction, number of groups, number of repeated measurements, and whether the effect size referred specifically to the Group $\times$ Time interaction. Given the final sample of only 20 participants per group, transparency regarding the power model is essential for evaluating whether the trial was adequately powered for five primary outcomes.

The eligibility criterion stating that participants had a “borderline/clinical elevation on the Total Problems, Internalizing, or Externalizing broadband scale, together with confirmation of clinically significant impairment during a psychologist-led diagnostic interview” is insufficiently operationalized. The authors should provide the actual YSR T-score threshold used to define borderline or clinical elevation and identify the diagnostic interview procedure. If a structured or semi-structured diagnostic instrument was not used, the manuscript should state this explicitly and describe the domains assessed, interviewer qualifications, and criteria for determining clinically significant impairment.

The exclusion criterion “acute suicide risk requiring a higher level of care” is clinically appropriate but needs operational definition. Please specify how suicide risk was assessed, whether a standardized suicidality instrument was used, what threshold triggered exclusion or emergency referral, and what safety procedure was followed if suicidal ideation emerged during treatment. This is especially important in a trial involving adolescents with emotional-behavioral problems and should be described under both ethical procedures and intervention safety management.

In the randomization paragraph, the manuscript reports that “group assignments were placed in sequentially numbered, opaque envelopes prepared by a research assistant not involved in assessment or treatment.” The authors should clarify who generated the allocation sequence, who enrolled participants, and who opened the envelopes. In addition, please indicate whether envelopes were sealed and tamper-evident. These details are necessary to establish whether allocation concealment was adequately maintained and to reduce concerns regarding selection bias.

The intervention paragraph states that “with consent, a subset of sessions was audio-recorded and reviewed against a brief adherence checklist.” This procedure requires greater methodological specificity. Please report how many sessions were recorded, how the subset was selected, who rated adherence, whether raters were independent of the treating therapist, the number and scoring format of adherence items, and whether inter-rater reliability was assessed. Without quantitative fidelity data, it is difficult to determine whether the intervention was delivered consistently across participants.

Response: Revised and uploaded the manuscript.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.