

Comparing the Effectiveness of Compassion-Focused Therapy and Metacognitive Psychotherapy on Body Shame in Women Seeking Cosmetic Surgery

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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:

The manuscript states that the sample size was determined “based on... the methodological consideration that 15 participants per group would provide an adequate sample size.” This justification is insufficient for an intervention study with three groups and three repeated measurements. A formal a priori power analysis should be reported, including the assumed effect size, α level, desired statistical power, number of groups, number of repeated measurements, assumed correlation among repeated observations, and any correction for nonsphericity. If no a priori calculation was conducted, this should be acknowledged transparently, and the authors should avoid implying that the selected group size was demonstrably adequate merely because a methodological source suggested 15 participants per group.

The participant eligibility criteria require further conceptual and procedural clarification. The Methods section excludes women with “a history of psychiatric disorders” and those “using psychiatric medications,” but does not explain how these criteria were assessed. The authors should specify whether psychiatric history was determined by self-report, clinical interview, medical records, or a structured diagnostic instrument. This is especially important because clinically relevant body dysmorphic

disorder, depression, anxiety, eating pathology, and obsessive-compulsive symptoms may be disproportionately represented among individuals seeking cosmetic surgery and may directly influence body shame and treatment response. Excluding psychiatric disorders without systematic screening may introduce both misclassification and selection bias.

The manuscript should provide substantially more information on the cosmetic-surgery characteristics of the sample. The statement that participants were “women seeking cosmetic surgery” treats a potentially heterogeneous population as a single clinical group. The authors should report the types of procedures sought, such as rhinoplasty, breast surgery, liposuction, abdominoplasty, facial procedures, or other operations; whether participants were first-time applicants or had undergone previous cosmetic procedures; and whether surgery had already been scheduled. These characteristics may be associated with different levels and forms of body-image disturbance and could therefore influence baseline body shame and responsiveness to psychological intervention.

The study relies on the Body Image Shame Scale, but the manuscript should clarify the scoring procedure actually used in the analysis. The instrument paragraph states that “the mean score ranges from 0 to 4,” whereas Table 1 reports total and subscale values such as 37.50, 21.06, and 16.43, which clearly represent summed rather than mean scores. The authors should explicitly describe the number of items contributing to each subscale, the possible score range for internalized shame, externalized shame, and the total score, and whether summed or averaged values were analyzed. The scoring description must be consistent with the reported data to permit replication and accurate clinical interpretation.

The psychometric reporting for the primary outcome measure is incomplete for the current sample. The Data Collection Tools section cites prior internal consistency coefficients but does not report reliability estimates from the present participants. Because reliability is sample-dependent, Cronbach’s alpha and preferably McDonald’s omega should be calculated for the total Body Image Shame Scale and both subscales at least at baseline, and ideally at each measurement occasion. Given the relatively small sample, confidence intervals around reliability estimates would also be informative. Reporting only previously published reliability coefficients does not establish adequate measurement precision in the present dataset.

The intervention descriptions are detailed in content but insufficient regarding treatment fidelity. For example, the compassion-focused intervention is described as “eight-session intervention based on Gilbert’s compassion-focused therapy framework,” yet the manuscript does not specify who delivered the sessions, the therapist’s academic qualifications, clinical training, experience with compassion-focused therapy, supervision arrangements, or whether a treatment manual/checklist was used. The same limitation applies to the metacognitive intervention. The authors should report therapist characteristics and describe how adherence to each protocol was assessed, because differential therapist expertise or protocol adherence could partly explain the observed superiority of compassion-focused therapy.

The presentation of the repeated-measures ANOVA in Table 2 is statistically incomplete. The table gives “Sum of Squares, Error Sum of Squares, F, p, and Partial η^2 ” but does not report the numerator and denominator degrees of freedom for any effect. These degrees of freedom are essential for transparent statistical reporting, particularly because a Greenhouse–Geisser correction was reportedly applied to internalized shame. The corrected degrees of freedom and, preferably, the Greenhouse–Geisser epsilon should therefore be reported. The exact model specification and error terms underlying each F statistic should also be verified because the current table does not provide enough information to reproduce the analysis.

The authors should distinguish more carefully between main effects and intervention effects. The sentence “These interaction effects demonstrate that the temporal pattern of change in body shame differed significantly across the compassion-focused therapy, metacognitive psychotherapy, and control conditions” is appropriate, but elsewhere the manuscript interprets significant main effects of group and time as evidence of intervention effectiveness. In a longitudinal intervention design, the group $\times$ time interaction is the principal test of differential treatment response, whereas a group main effect may reflect baseline differences and a time main effect may reflect temporal change unrelated to treatment. The discussion should therefore emphasize the interaction and subsequent simple/pairwise effects rather than treating all three omnibus effects as equivalent evidence of efficacy.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The manuscript should clarify whether the two experimental interventions were delivered by the same therapist or by different therapists. This issue is critical because the finding that “compassion-focused therapy produced significantly greater reductions than metacognitive psychotherapy” could reflect therapist effects rather than treatment-specific mechanisms if different clinicians delivered the interventions. If different therapists were used, their qualifications and experience should be compared, and therapist effects should be discussed as a potential confounder. If the same therapist delivered both interventions, the authors should explain how allegiance effects and contamination between protocols were minimized.

The control condition is insufficiently characterized. The manuscript states that “the control group received no psychological intervention during the study period,” but it is unclear whether these participants received usual clinical care, remained on a waiting list, proceeded with cosmetic consultations or surgery, or had any contact with study personnel between assessments. These details are essential because a passive control does not control for therapeutic attention, expectancy, group support, or repeated contact. The authors should clearly define the control condition and moderate causal interpretations accordingly, especially when comparing the relative efficacy of two active interventions against no treatment.

The manuscript should report intervention attendance, adherence, and attrition separately for each group. The exclusion criterion of “absence from more than two treatment sessions” implies that participants could attend as few as six of eight sessions and remain in the analysis, but the actual number of sessions attended is not reported. Mean attendance, range, number completing all sessions, homework adherence, and reasons for missed sessions should be presented. These data are necessary for judging intervention feasibility and dose exposure and may help explain differential effects between compassion-focused therapy and metacognitive psychotherapy.

The six-month follow-up procedure requires clarification, particularly in relation to cosmetic surgery occurring during the interval. The manuscript states that “the follow-up assessment was conducted six months after the final treatment session,” but does not indicate whether participants underwent their intended cosmetic surgery between posttest and follow-up. This is a major potential confounder because surgery itself could substantially alter body satisfaction, body shame, social feedback, and self-evaluation. The authors should report how many participants in each group underwent cosmetic procedures during follow-up, the types and timing of those procedures, and whether this variable was controlled analytically or considered in sensitivity analyses.

The statistical analysis should be reconsidered in light of the small and apparently unequal group sizes. The manuscript reports conventional repeated-measures ANOVA and extensive assumption testing; however, if the actual group sizes were 16, 15, and 17, the analysis should explicitly account for unbalanced groups. More importantly, repeated-measures ANOVA generally requires complete observations and can perform poorly when follow-up data are missing. The authors should state whether all participants had complete data at all three time points and how missing data, if any, were managed. A linear mixed-effects model would provide a more flexible approach to repeated observations, unequal group sizes, and missingness and should at minimum be considered in a sensitivity analysis.

The manuscript's treatment of statistical assumptions needs greater precision. The Findings section states that “none of the Shapiro–Wilk values... were statistically significant, indicating normal distributions.” A nonsignificant Shapiro–Wilk test does not establish normality, particularly with small samples in which the test has limited power. The authors should instead describe the results as showing no strong statistical evidence against normality and supplement these tests with Q–Q plots, skewness/kurtosis, or examination of model residuals. Assumptions for repeated-measures ANOVA pertain especially to residual distributions and covariance structure rather than simply the raw outcome distributions within each cell.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor's decision: Accepted.



Editor in Chief's decision: Accepted.