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Comparison of the Effectiveness of Mindfulness-Based Cognitive Therapy and Acceptance and Commitment Therapy on Ideas of Reference, Thought–Action Fusion, and Cognitive Avoidance in Women with Breast Cancer

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1. Round 1

1.1. Reviewer 1

Reviewer:

The ACT intervention paragraph similarly lacks sufficient information to permit replication. The authors state that the protocol was based on Dahl et al. and underwent “minor adaptations,” but the exact nature of these modifications is not described. More importantly, several listed session components, such as “thought stopping” and “selective attention for greater relaxation regarding negative automatic thoughts,” do not clearly correspond to contemporary ACT principles and may conflict with ACT’s emphasis on cognitive defusion and willingness rather than suppression or control of cognition. The authors should critically review the intervention content against the six core ACT processes—acceptance, cognitive defusion, contact with the present moment, self-as-context, values, and committed action—and explain how each session operationalized these processes.

The manuscript should report intervention fidelity and therapist effects because these are essential for a comparative psychotherapy trial. Neither the MBCT nor ACT paragraph indicates whether the same therapist administered both

interventions, whether different therapists were used, or whether sessions were audio- or video-recorded and evaluated against treatment checklists. If separate therapists delivered the interventions, therapist-specific characteristics could become confounded with treatment condition. If one therapist delivered both interventions, allegiance effects and cross-contamination become possible. The authors should report therapist assignment, supervision procedures, adherence assessment, competence ratings if available, and steps taken to minimize contamination between therapeutic models.

The role of the control group should be more precisely described. The manuscript currently states that "the control group received no psychological intervention during the study period." This creates the possibility that observed differences may partly reflect nonspecific therapeutic factors such as attention, expectancy, social contact, group support, and therapist interaction rather than treatment-specific mechanisms. The authors should clarify whether the control condition was treatment-as-usual, wait-list control, or no-treatment control and describe the oncology care received by all participants. This issue should also be acknowledged as a limitation, and the discussion should avoid attributing observed effects exclusively to specific MBCT or ACT mechanisms unless the design included an attention-matched comparator.

There is a critical inconsistency between the stated three-group design and Table 1, which reports only "Experimental" and "Control" groups. According to the Methods, the study included MBCT (n = 15), ACT (n = 15), and control (n = 15), yet the descriptive statistics collapse the two active interventions into a single experimental category. This prevents readers from evaluating the central research question, which is explicitly comparative. Table 1 should present separate pretest and posttest means and standard deviations for MBCT, ACT, and control for every dependent variable. The authors should not combine the two intervention arms because doing so eliminates the information necessary to determine whether the therapies differed in effectiveness.

The descriptive values in Table 1 appear inconsistent with the interpretation of treatment benefit and should be carefully checked against the original dataset. For example, "ideas of reference" reportedly increased from 21.74 to 32.44 in the experimental group, while the control group increased from 23.74 to 77.08. More importantly, cognitive avoidance increased from 37.38 to 47.13 in the experimental group but decreased from 37.84 to 35.12 in the control group. If higher Cognitive Avoidance Questionnaire scores represent greater avoidance, these data would imply deterioration rather than improvement in the experimental group. The authors should verify every descriptive value, confirm the direction of scoring for each instrument, and ensure that the textual interpretation is consistent with the observed means.

The value "77.08 (10.23)" reported for the posttest of the control group on the outcome labeled ideas of reference is impossible if this variable was measured using the 15-item PTQ described in the Methods, because the PTQ total score reportedly ranges from 0 to 60. This is a particularly serious internal inconsistency that strongly suggests either a transcription error, use of a different scale, incorrect variable labeling, or incorrect table construction. The authors should return to the original SPSS output and raw scoring sheets and verify the instrument, scoring range, and all descriptive statistics before further statistical interpretation. Until this discrepancy is resolved, the validity of the corresponding inferential results cannot be adequately evaluated.

The manuscript states that the assumptions of normality were examined using the Kolmogorov–Smirnov test and concludes that "the distributions of the dependent-variable scores did not significantly deviate from normality." However, Table 1 provides only one Kolmogorov–Smirnov statistic and p value per outcome, despite having multiple groups and assessment occasions. The authors should specify whether normality was tested separately within each of the three groups at each time point, on model residuals, or on the pooled observations. For ANCOVA/MANCOVA, examination of residual distributions is generally more relevant than testing raw outcome scores alone. Q–Q plots, skewness and kurtosis, and assessment of influential observations would provide a more informative evaluation of distributional assumptions.

The statistical model is not adequately aligned with the research design. The manuscript refers to "multivariate analysis of covariance," but Table 2 reports only one "Group" degree of freedom, which is consistent with a two-group comparison rather than the three-group structure described in the Methods. With three groups, the group effect would ordinarily have 2 degrees of freedom. This discrepancy must be resolved. The authors should clearly specify the exact statistical model, including the three-level between-subjects factor, the posttest dependent variables, corresponding pretest covariates, and the method used for subsequent pairwise comparisons. The primary inferential analysis should directly test differences among MBCT, ACT, and control rather than collapsing the intervention conditions.

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The manuscript requires a stronger account of clinical and oncology-related covariates. The inclusion criteria restrict participants to "stage III and grade 2 breast cancer" and current chemotherapy, but no information is provided regarding time since diagnosis, type of surgery, chemotherapy regimen, number of completed treatment cycles, radiotherapy status, hormonal treatment, menopausal status, recurrence history, pain, fatigue, psychiatric medication, or previous psychological treatment. These factors can meaningfully affect anxiety, cognitive functioning, fatigue, emotional regulation, and treatment attendance. At minimum, the authors should provide relevant clinical characteristics by group and examine whether randomization achieved reasonable baseline balance. Any clinically important imbalance should be considered in sensitivity analyses or discussed as a potential confounder.

The ethical reporting is currently insufficient for a study involving women undergoing active cancer treatment. The Methods paragraph states only that "the necessary administrative and institutional permissions had been obtained" and that participants provided informed consent. The manuscript should report the name of the approving ethics committee, ethics approval code, approval date in the Gregorian calendar, and whether the study was prospectively registered, if applicable. The authors should also describe confidentiality procedures, voluntary participation, the right to withdraw without affecting oncology care, management of psychological distress identified during sessions, and whether control participants were offered an intervention after study completion. Given the exclusion criterion involving severe depression and psychotic symptoms, the authors should explain the referral procedure used when individuals requiring immediate psychological or psychiatric care were identified.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

The numerical values in Table 2 contain substantial internal inconsistencies and should be reconstructed directly from verified statistical output. For instance, the text reports "F = 333.19" for ideas of reference, while the table gives F = 334.21. More fundamentally, an F value above 300 accompanied by p = .049 is statistically implausible for the reported degrees of freedom. Comparable concerns apply to other rows in which extremely large F statistics are paired with p values such as .006 or .007. The reported sums of squares, mean squares, F statistics, p values, eta-squared values, and statistical power should therefore be recalculated or retranscribed from SPSS. The authors should verify that decimal separators were converted correctly during translation and should preferably report exact p values to three decimal places, using $p < .001$ only when appropriate.

The manuscript reports Wilks' lambda as "Wilks' $\Lambda = .41$, $F = 6.75$, $p < .01$," but essential multivariate information is missing. The authors should report the hypothesis and error degrees of freedom associated with the F approximation, along with an appropriate multivariate effect size such as partial η^2. In addition, because multiple dependent variables were included, the manuscript should state whether other multivariate statistics—Pillai's trace, Hotelling's trace, or Roy's largest root—were examined. Pillai's trace is often preferred when assumptions may be imperfect. The authors should also explain how multivariate assumptions, including equality of covariance matrices and multicollinearity among dependent variables, were evaluated.

The post-hoc comparisons need to be based on actual statistical estimates rather than qualitative inference. The current Table 3 states that MBCT and ACT each differed significantly from control but acknowledges that "direct difference cannot be estimated from the supplied statistical output." Because the primary objective is to "compare the effectiveness" of MBCT and ACT, a formal MBCT-versus-ACT comparison is indispensable. The authors should conduct adjusted pairwise comparisons following the omnibus model and report adjusted mean differences, standard errors, 95% confidence intervals, multiplicity-adjusted p values, and preferably effect sizes. If such analysis cannot be performed from the available dataset, the title and conclusions should be revised so that the manuscript does not claim a direct comparison between the two interventions.

The Discussion paragraph stating that "women receiving the interventions demonstrated lower levels of maladaptive self-referential thinking, thought–action fusion, and cognitive avoidance at posttest" is not fully supported by the descriptive statistics currently presented. As noted above, the experimental-group cognitive avoidance mean increases from pretest to

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posttest in Table 1, and the purported “ideas of reference” values raise scoring-range concerns. The Discussion should be rewritten only after the descriptive and inferential analyses have been corrected. Each interpretation should explicitly correspond to verified group-specific results, and causal language should be used cautiously given the sampling strategy, control condition, and uncertainty regarding the exact design classification.

Authors revised the manuscript and uploaded the document.

2. Revised

Editor’s decision: Accepted.
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