

Modeling Emotional Eating through Self-Compassion, Body Dissatisfaction, Emotion Regulation, and Psychological Distress

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the first paragraph of the Introduction, the manuscript states that “emotional eating may occur across clinical, subclinical, and community populations and may contribute to cycles of guilt, shame, body dissatisfaction, loss of control, weight fluctuation, and disordered eating symptoms.” This is a strong opening claim, but it would benefit from clearer differentiation between emotional eating, binge eating, and general disordered eating. The current wording risks conceptually blending related but distinct eating constructs, and the authors should define emotional eating more precisely as affect-driven intake rather than loss-of-control eating unless the latter was directly measured.

The Introduction paragraph beginning “A central psychological process in emotional eating is psychological distress” provides a useful rationale, but the authors should clarify why psychological distress is positioned as a mediator rather than only as a predictor. The manuscript would be strengthened by explaining theoretically why depression, anxiety, and stress are treated as proximal emotional mechanisms linking self-compassion, body dissatisfaction, and emotion regulation difficulties to emotional eating.

In the paragraph beginning “Body dissatisfaction is another major risk factor,” the discussion of body dissatisfaction is persuasive, yet the manuscript should better justify why body dissatisfaction is modeled as an exogenous predictor rather than an outcome of psychological distress or low self-compassion. Since the literature often treats body dissatisfaction as both antecedent and consequence of distress, the authors should explicitly defend the directionality of this path in the hypothesized model.

The Introduction paragraph on social media and sociocultural context is relevant, but the study does not appear to measure social media use, appearance comparison, weight stigma, or sociocultural pressure. The authors should either reduce this section or explicitly frame it as contextual background rather than a variable-level rationale, because an extended discussion of unmeasured constructs may create a mismatch between the theoretical introduction and the empirical model.

In the Methods and Materials section, the sentence “This study employed a cross-sectional correlational design using structural equation modeling (SEM)” is accurate, but the manuscript should add a stronger statement acknowledging that SEM with cross-sectional data cannot establish causal ordering. Since the model uses directional arrows and mediation language, the authors should clarify that the tested pathways are theoretically specified associations rather than evidence of temporal or causal effects.

In the Findings section, the demographic paragraph reports gender, age, education, employment, and BMI, but it does not report ethnicity, province distribution, marital status, or income. Because the sample is described as Canadian and the study concerns body image and emotional eating, additional demographic variables could be highly relevant. At minimum, the authors should acknowledge whether these variables were collected and, if not, identify this omission as a limitation.

In Table 1, the authors report Cronbach’s alpha, composite reliability, and AVE for each construct, which is valuable; however, the table combines descriptive statistics, reliability, convergent validity, and correlations in a single dense presentation. The authors should ensure that the manuscript clearly explains how CR and AVE were calculated and whether they were derived from the confirmatory factor analysis loadings rather than from raw scale scores.

Authors revised and uploaded the document.

1.2. Reviewer 2

Reviewer:

In the Study Design and Participants paragraph, the authors report that “A total of 742 adults participated in the study,” but they should provide a sample size justification. For a SEM study, it is important to report either an a priori power analysis, a minimum cases-to-parameters rationale, or a Monte Carlo-based justification showing that the sample size was adequate for the complexity of the measurement and structural model.

In the participant recruitment paragraph, the manuscript states that participants were recruited “through online advertisements, university mailing lists, community organizations, and social media platforms using a voluntary convenience sampling strategy.” This recruitment strategy is acceptable, but the authors should describe how recruitment sources were distributed and whether any subgroup was overrepresented. Without this information, it is difficult to determine whether the sample reflects Canadian adults broadly or mainly university-connected and digitally active participants.

In the eligibility criteria paragraph, the authors exclude individuals with “severe cognitive impairment or psychotic disorders,” but it is unclear how these criteria were verified. The authors should specify whether exclusion criteria were based on self-report screening questions, clinical diagnosis, participant disclosure, or survey logic. This clarification is important because online studies cannot generally verify clinical exclusion criteria unless a structured screening process is used.

In the Data Collection Tools section, the Emotional Eating Scale is described as measuring “the tendency to eat in response to a range of emotional states rather than physiological hunger.” This description is appropriate, but the authors should indicate whether the total score or subscale scores were used in the SEM. Because the figure presents emotional eating indicators such as anxiety eating, anger eating, frustration eating, sadness eating, and depression eating, the methods should explicitly explain how these observed indicators were derived from the original scale.

In the description of the Self-Compassion Scale, the authors write that “Negatively worded items are reverse scored before calculating the total score.” Given ongoing psychometric debate about the use of total self-compassion scores versus positive and negative subfactors, the authors should justify treating self-compassion as a single latent construct. This is particularly important because the figure includes reverse-coded dimensions such as self-judgment, isolation, and over-identification, which may not always load equivalently with positive self-compassion components.

In the Data Collection Tools paragraph for the Body Shape Questionnaire-34, the manuscript states that it evaluates concerns “over the previous four weeks.” The authors should explain how the BSQ-34 was represented in the measurement model, because the figure displays indicators such as body shape concern, weight concern, body image distress, appearance evaluation, and self-classified weight. If these are parcels or theoretically grouped subscales, the parceling procedure must be described in detail.

In the Data Analysis paragraph, the authors state that “missing data represented less than 1% of all responses and were handled using expectation-maximization estimation.” This is useful, but the manuscript should report whether the missingness mechanism was evaluated, for example through Little’s MCAR test or comparison of participants with and without missing data. Even when missingness is low, readers need to know whether expectation-maximization was statistically justified.

Authors revised and uploaded the document.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.