





The Relationship Between Sexist Parenting Style and Cognitive Emotion Regulation Styles in Men With Perpetration of Domestic Violence Against Women

Fatemeh. Aliakbari¹, Davood. Taghvaei^{1*}, Zabih Pirani¹



¹ Department of Educational Psychology, Ar.C., Islamic Azad University, Arak, Iran

* Corresponding author email address: davoodtaghvaei@yahoo.com

Editor

Monika Szczygieł¹
Department of Psychology,
Jagiellonian University, Krakow,
Poland
monika.szczygiel@uj.edu.pl

Reviewers

Reviewer 1: Mohammad Hassan Ghanifar¹
Assistant Professor, Department of Psychology, Birjand Branch, Islamic Azad University, Birjand, Iran. Email: ghanifar@iaubir.ac.ir
Reviewer 2: Abotaleb Saadati Shamir¹
Assistant Professor, Department of Educational Sciences, University of Science and Research, Tehran, Iran. Email: psychology@iau.ac.ir

1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, paragraph three, you note "emerging research indicates that cognitive emotion regulation strategies... serve as both risk and protective factors," yet the study only models maladaptive strategies as risk factors. Consider discussing protective factors or clarifying focus.

The reporting of SEM results in Table 4 should include confidence intervals for path coefficients to provide readers with precision of estimates.

In the Methods, paragraph one, you mention suspending physical distribution due to COVID-19 but do not discuss potential impact of this shift to online data collection on sampling bias. Addressing this would strengthen methodological transparency.

In the Results, you report correlation coefficients and structural path coefficients but do not clarify the potential directionality or model specification (e.g., control of measurement error). Adding a sentence clarifying model assumptions would improve interpretability.

The Introduction discusses "sexist parenting style" but does not clearly define this construct in operational terms early in the text. Consider providing a precise definition at first mention.

The title of Table 4 could be revised to explicitly state "Standardized Path Coefficients" to avoid ambiguity regarding raw versus standardized coefficients.

In the Discussion, you conclude that maladaptive cognitive emotion regulation is positively related to violence, yet Table 4 shows a negative beta coefficient. This needs clarification: specify whether lower scores on adaptive regulation imply maladaptation or adjust the interpretation.

Response: Revised and uploaded the manuscript.

1.2. Reviewer 2

Reviewer:

In the Introduction, the sentence "Domestic violence against women is a complex and multidimensional phenomenon..." could benefit from clearer delineation between existing literature and the study's specific research gap. Consider explicitly stating the gap this study addresses.

In the Methods section, paragraph one, you mention "intrinsic and extrinsic religious orientation," but this variable is not later operationalized or reported in the Results. Please clarify its role or remove reference to it.

In the Methods, the description of the Domestic Violence Questionnaire lacks details on factor structure and prior validation metrics. Consider adding these psychometric properties to bolster instrument validity discussion.

The description of the Cognitive Emotion Regulation Questionnaire (CERQ) in the Measures section should include subscales used, as CERQ measures multiple dimensions. This helps clarify what aspects of emotion regulation were analyzed.

In the Results section, Table 1, the reported mean and SD values would benefit from including ranges (min/max) to give readers a clearer picture of variable distributions.

The description of Table 2 states "higher use of certain cognitive emotion regulation strategies may be associated with increased violent behavior," yet Table 4 path coefficient shows a negative relationship. This apparent inconsistency should be clarified.

In Table 3, while reporting fit indices, please include degrees of freedom and p-value for the Chi-square test itself to provide complete model fit information.

The Discussion cites prior studies (e.g., Bermea et al., 2021) on marginalized populations, yet does not tie this specifically to your findings on men in Iran. Consider more tightly integrating cross-cultural findings with current results.

In the Conclusion, you mention "biopsychosocial framework" but do not discuss biological variables. Consider reframing or expanding this claim to better align with study scope.

In the Limitations section, you note generalizability issues due to sampling. It would strengthen this section to also mention cultural specificity of measures and potential measurement bias.

In the Discussion, paragraph four, you state "Keivanlu and Mikaili reported that combining emotion regulation therapy with compassion-focused therapy significantly improved mental well-being..." This references female survivors; explicitly connect how this implies relevance to perpetrators for coherence.

Response: Revised and uploaded the manuscript.

2. Revised

Editor's decision after revisions: Accepted.

Editor in Chief's decision: Accepted.

