

Preliminary Evidence on Moral Underpinnings of Rape Victim Blaming in Iran: Replication and Extensions of Milesi et al. (2020)

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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 theoretical development in the paragraph beginning “How and why are rape victims blamed?” needs a clearer distinction between Moral Foundations Theory and dyadic morality. The manuscript currently states that “Individualizing foundations align closely with the concept of ‘dyadic morality’” and subsequently uses this correspondence to predict negative associations between Care/Fairness and RVB. However, conceptual overlap does not establish that the theories generate identical psychological mechanisms. The authors should articulate whether dyadic morality is being proposed as a mechanism connecting individualizing foundations to victim-blaming, as a complementary theoretical perspective, or simply as an interpretive framework. This distinction is especially important because the General Discussion later treats the nonsignificant Care/Fairness effects as evidence that Iranian participants may “deviate” from dyadic morality, which is a considerably stronger theoretical conclusion than the analyses directly justify.

The rationale concerning binding foundations should be sharpened by specifying distinct mechanisms for Loyalty, Authority, and Sanctity rather than treating the three collectively as manifestations of the same cultural process. For example,

the manuscript states that Loyalty may reinforce a “stranger danger” heuristic, Authority may reflect traditional gender hierarchies, and Sanctity may involve perceptions of bodily contamination. These are theoretically different pathways and should therefore produce potentially different predictions. The results themselves support such differentiation: Authority and Sanctity emerge more consistently than Loyalty across Studies 1 and 2. I recommend formulating foundation-specific hypotheses before presenting the studies and then evaluating each hypothesis separately rather than repeatedly interpreting the three foundations collectively as “binding morality.” This would considerably improve correspondence between theory and empirical results.

The Persian AMMSA translation requires substantially stronger validation before the manuscript can conclude that the measure has “satisfactory psychometric qualities.” The Methods state that the 30 items “were translated to Persian by one of the researchers and then back translated to English by a professional translator,” followed essentially by coefficient alpha. Back-translation and $\alpha = .89$ establish neither construct validity nor factorial equivalence. The authors should provide information about reconciliation of translation discrepancies, cultural adaptation, pilot testing, item comprehension, dimensionality, and preferably confirmatory factor analysis. Because Study 3 compares Iran with European samples using language-specific versions of AMMSA, evidence of measurement equivalence becomes even more important. Without such analyses, cross-cultural differences could partly reflect scale functioning rather than substantive differences in rape-myth endorsement.

The causal language used in the Study 1 mediation analysis needs revision. The manuscript states that the “negative effect of Fairness on RVB is explained by its negative influence on AMMSA” and that Authority and Sanctity operate through their “positive influence” on AMMSA. Yet moral foundations, AMMSA, and victim-blaming were measured cross-sectionally rather than experimentally or longitudinally. PROCESS can estimate an indirect statistical association, but the temporal and causal sequence $MFT \rightarrow AMMSA \rightarrow RVB$ is not established by these data; reverse or reciprocal explanations remain plausible. The manuscript should consistently use terms such as “indirect association” or “statistical mediation pattern,” avoid “influence” and “effect” where causal inference is implied, and explicitly acknowledge temporal ambiguity when interpreting mediation findings.

Response: Revised and uploaded the new document.

1.2. Reviewer 2

Reviewer:

The paragraph introducing AMMSA makes an important argument but overstates what the instrument accomplishes when the authors write that it provides a way of assessing rape myths “implicitly and thus avoiding social desirability biases.” AMMSA is still a self-report measure involving direct agreement with attitudinal statements; its items may be subtler than traditional rape-myth scales, but it is not an implicit measure in the psychometric sense. The manuscript should therefore distinguish “modern/subtle” measurement from “implicit measurement.” Claims that AMMSA “avoids” social-desirability bias should also be replaced by the more defensible proposition that it was designed to reduce problems associated with overtly worded traditional rape-myth items. This conceptual correction is particularly important because the purported suitability of AMMSA for the Iranian context constitutes one of the stated motivations for the research.

The “Overview of the current studies” needs explicit, testable hypotheses corresponding to each study. At present, Study 1 is described as investigating relationships, Study 2 as a preregistered replication plus an ethnicity moderation analysis, and Study 3 as a cross-cultural comparison, but directional hypotheses are dispersed throughout the manuscript. The authors should state, before Study 1, predictions for Care, Fairness, Loyalty, Authority, Sanctity, and AMMSA; specify whether AMMSA mediation is confirmatory or exploratory; and clearly identify the preregistered Study 2 hypotheses. For Study 3, the predicted country $\times$ foundation interactions and expected directions should similarly be stated before analysis. This restructuring would also allow readers to distinguish preregistered tests from exploratory analyses and reduce the current risk of interpreting post hoc patterns as hypothesis-confirming evidence.

The Study 1 sample-size justification is insufficiently reproducible. The Participants paragraph states that “A sensitivity analysis indicated that a total sample of 216 was sufficient to detect a medium to large effect size ... at 80% power and 0.05 alpha,” but a sensitivity analysis ordinarily reports the minimum detectable effect size given N , α , statistical model, and power rather than determining that a sample is “sufficient” for an unspecified medium-to-large effect. Please report the exact G*Power procedure, regression model tested, number of predictors, assumed or detectable effect-size metric (e.g., f^2), α , desired power, and resulting critical effect size. The same correction should be made for Study 2. This is particularly important because several interaction and mediation analyses are considerably less powered than a basic multiple regression.

The psychometric properties of the Moral Foundations Questionnaire represent a major limitation that requires analytical rather than merely narrative treatment. In Study 1, Care $\alpha = .58$, Fairness $\alpha = .49$, and Loyalty $\alpha = .58$, and Study 2 again reports Care $\alpha = .48$, Fairness $\alpha = .49$, and Loyalty $\alpha = .58$. Reliabilities around .48–.58 imply substantial measurement error and raise serious concerns about attenuation, unstable regression coefficients, and particularly the interpretation of nonsignificant effects for Care and Fairness. The manuscript should not interpret the absence of Care effects as substantive evidence about Iranian moral cognition without addressing this issue. At minimum, report item-level diagnostics and omega coefficients; ideally, evaluate the factorial structure of the Persian MFQ using CFA or an appropriate latent-variable model and conduct sensitivity analyses demonstrating whether conclusions remain similar after accounting for measurement reliability.

Response: Revised and uploaded the new document.

2. Revised

Editor's decision: Accepted.

Editor in Chief's decision: Accepted.