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Technical comment on Horton, E K.J., Schermerhorn, N., & Hanel, P H P. (2025). The impact of toxic masculinity on restrictive emotionality and mental health seeking support

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ABSTRACT

Horton, Schermerhorn, and Hanel (2025) purport to demonstrate the impact of “toxic masculinity” on men’s mental health help-seeking. While we applaud efforts to understand mens health, this comment argues that conceptual flaws, psychometric redundancy, and statistical errors fundamentally compromise the two studies presented in this paper. We highlight the reliance on outdated “hydraulic models”, the circularity of predictor-criterion relationships (i.e., tautology), and the misapplication of mediation analyses where no primary association exists. Furthermore, we critique the unjustified causal language in a cross-sectional design and the potential for Type I errors given the analysis’s exploratory nature. We conclude that the paper relies on sensationalist terminology rather than rigorous theory-testing.

Horton, Schermerhorn, and Hanel (2025) investigate the relationship between ‘toxic masculinity,’ ‘restrictive emotionality,’ and help-seeking. We argue that conceptual flaws, psychometric redundancy, and statistical errors fundamentally compromise their conclusions.

1. Conceptual weaknesses and theoretical assumptions

The study relies implicitly on “hydraulic” models of emotion—the idea that “holding in” feelings is inherently damaging. While the authors do not explicitly cite Freud, this conceptualization mirrors outdated ‘catharsis’ perspectives, which lack empirical support. The authors adopt a constructivist view of men’s psychology, assuming gender norms are entirely “early learned” behaviors. This perspective ignores the evolutionary or biological origins of why these norms emerge, why men are uniquely susceptible to them, and why they persist. By failing to consider that emotional control may have adaptive functions (e.g., reducing vulnerability or maintaining status in male hierarchies), the authors frame masculinity as predatory or dangerous even in small amounts. Indeed, men who suppress their emotions more are less likely to commit domestic violence, suggesting that defining emotional control as some form of toxicity may be overly simplistic (Logoz et al., 2023).

Furthermore, the definition of “toxic masculinity” as an “exaggeration” of norms is untested. The authors imply a nonlinear threshold or hysteresis point where masculinity becomes “toxic,” yet they use linear, continuous measures. If “toxicity” is defined by its adverse effect on others, self-report attitude scales are an inappropriate proxy. Indeed, the paper reflects a recurrent vilification of “the masculine” as observed in the APA over the last decade (Defant, 2025).

2. Psychometric issues and tautology

The study suffers from severe overlaps between the predictors and the criterion. The ‘Toxic Masculinity’ subscales (e.g., Power over Women) naturally covary with the mediator ‘Restrictive Emotionality’

($r = 0.59$), suggesting they measure the same underlying construct rather than distinct variables. This overlap likely stems from the authors’ constructivist framework, in which predictors and mediators are conceptually intertwined rather than distinct variables; their correlations confirm rather than truly test, let alone falsify, hypotheses.

Moreover, the relationship between the predictor and the outcome is semantically circular. The ‘Self-Reliance’ subscale is the operational inverse of the outcome ‘Help-Seeking’; finding a negative correlation between preferring to solve problems alone and seeking help is tautological. The failure to use subscales appropriately or control for this overlap creates spuriously “strong links” that are artifacts of measurement.

3. Statistical irregularities and data interpretation

The statistical handling of the data is inconsistent and potentially biased toward confirmation.

3.1. Mediation errors

The authors construct mediation models despite finding no basic association between the independent variable (i.e., Toxic Masculinity) and the dependent variable (i.e., Help-Seeking). While we acknowledge that modern mediation approaches allow for indirect effects in the absence of a total effect, the lack of a primary association renders the paper’s title—claiming an “Impact”—misleading. Moreover, if relationships are revealed after a third variable is entered, it suggests suppression, not mediation, which is even harder to predict, interpret, and replicate. Without a direct effect to explain, claiming indirect effects via restrictive emotionality is statistically dubious. It adds no new information beyond the known link between emotionality and help-seeking.

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3.2. Type I error inflation

In Study 1, the authors calculated 165 correlations. While the authors report these for descriptive purposes, interpreting specific relationships from such a matrix is fraught with Type I error risks. Despite their (in our opinion) weak correction for Type 1 error in Study 1, the authors proceed to present Study 2 with a similar sample size and number of tests, without such a correction. This suggests a post hoc rationale and an inconsistent approach to corrections.

3.3. Fishing for results

The finding that toxic masculinity predicts seeking help from religious figures appears to be the result of item-level fishery. The authors defend this approach by labeling their analyses as “explanatory,” yet the methodology is, by definition, “exploratory”. Mislabeling post hoc data mining as explanatory blurs the critical distinction between hypothesis generation and hypothesis testing. Although the authors used a conservative alpha (0.001) for these exploratory items, the interpretation remains convoluted. Alternative parsimonious explanations, for example, that men prefer advice from dominant, trusted figures (e.g., priests) over strangers (e.g., therapists), were ignored in favor of linking religiousness to toxicity to confirm their untested, politically motivated musings. Such issues have previously been identified by social psychologists (Jussim, Crawford, Stevens, & Anglin, 2016).

3.4. Moderation gaps

The paper discusses differential effects in men and women but fails to adequately test for moderation. Study 1 included both men and women. While the authors acknowledge the study is underpowered, a priori power analyses should have been conducted to detect effects in both sexes independently before running even the most liberal of moderation tests, such as Fisher’s z (Baron & Kenny, 1986). Study 2, conversely, included only men, rendering any test of sex moderation impossible. Consequently, claims regarding differential effects by sex are either statistically fragile (Study 1) or structurally untested (Study 2).

4. Conclusion

While we applaud efforts to improve men’s health, Horton et al. (2025) rely on biased confirmatory methods and sensationalist

terminology. We urge future research to prioritize rigorous theory-testing and distinct construct measurement over political ideology.

CRediT authorship contribution statement

Peter Jonason: Conceptualization, Writing – review & editing.
Dritjon Gruda: Conceptualization, Writing – original draft.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

No data was used for the research described in the article.

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