

Review of: "Gender and Organizational Conflict Management: The Mediating Role of Personality"

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Potential competing interests: No potential competing interests to declare.

I read the paper "Gender and Organizational Conflict Management: The Mediating Role of Personality" with great interest and value the Authors' attempt to delve deeper into studies about gender differences that use biological sex as a dummy variable. However, despite the promising claims at the beginning, the paper falls short of delivering what it claims. I have substantial concerns about the theoretical foundations of the study, the presentation of arguments in favour of the design, and the design itself. Below, I will try to explain my concerns in more detail. Some of them, of course, could be addressed by a careful literature review, strengthening the rationale of the entire study and specific associations expected. Those associated with the simplistic design the Authors employed and the lack of theoretical justification of the model cannot be addressed within the scope of the current sample.

The Abstract starts with a sentence "Many authors examined..." which needs to be either reformulated or supplemented with sources. As adding references in the Abstract should be rather avoided, I recommend reformulating the sentence to make it aim-oriented and to explain that past results about gender differences in conflict management are mixed. In this way, the Authors could present better both their rationale and objectives. The design of the study and measures used are not properly described.

In the Introduction, I would expect a more direct theoretical link between gender-conflict management and gender roles. Both arguments are relatively uncontroversial, but the Authors should unambiguously present their line of thought, which, I assume, is something like this: Because there are different gender norms (expectations about behaviour and characteristics), women are believed to prefer different conflict management styles compared to men. In other words, those expectations are often used as an explanation for observed differences in conflict management.

Afterwards, the Authors claim that "It is also suggested that, in general, women can resolve conflicts more effectively". Why? This is one of the examples of superficial work with the extant literature. It is not sufficient to mention a series of sources after such a claim. Instead, the Authors should try to explain how and why this conclusion was achieved, what the mechanisms are that link biological sex and effective conflict management?

In the very next sentence, the Authors write that "However, there is still little known about personality factors as potential mediating mechanisms..." First, why do the Authors – within three sentences – move from gender norms and stereotypes about women's efficiency to personality? Also, perhaps there is little known about the role of personality, but this is not a justification for a study in and of itself. The Authors need to stress the rationale for their study more than by claiming that some research was not done previously. I believe it is particularly important in personality and/or gender research. Both

gender and some aspects of personality show correlations with various outcome variables. However, without a proper theoretical framework, the results are nothing more than potentially spurious correlations. In other words, I would expect to read something like this: it was claimed that men and women tend to prefer various conflict management styles (references). However, the results are at best mixed - some studies show similarities, others differences. Therefore, we decided to delve into possible drivers of the effect and focused on personality because... (for instance, it was shown that personality is associated with conflict management styles). Without a proper theoretical background, the study seems like fishing for significant findings – which are quite probable to find when using biological sex and personality as variables in the design.

When stating their aims, the Authors claim they “[...] aim to fill the gap in the investigation of personal variables underlying gender differences in conflict management styles”. But filling a gap is not the most fortunate formulation of a research aim. As long as there is no sufficient theoretical justification, not every gap is worth filling.

p. 2: The Authors write “highly inconsistent knowledge” – philosophically speaking, knowledge can hardly be inconsistent. Findings could.

p. 3: The Authors write that “In other studies, gender-based differences were not confirmed” – I presume the studies were correlational, so I recommend avoiding causal language.

All three aspects of the mediation relation are poorly developed in the text. It seems rather sketchy, and the work with literature is superficial and highly selective. For instance, in the context of gender differences in personality traits, the authors fail to refer to Hyde’s gender similarity hypothesis and the entire tradition of studies that show the differences to be small, marginal, or even insignificant. Also, the way the authors discuss the findings does not justify expecting personality to be a mediator in this relationship. Since gender differences in both management style and personality are disputed, I would recommend focusing on the link between personality and conflict management, as this seems relatively well-established. The way it is now described in the paper suggests that the links are dubious – a claim I tend to agree with – and, consequently, the entire mediation model lacks justification. The fact that the data support it to some extent should not be considered sufficient. Again, without theoretical grounding, implications of such findings remain unclear.

Generally, the introduction does not explain the study rationale sufficiently and fails to substantiate the design. It requires thorough rewriting to stress why those three variables should be investigated in combination – other than some stereotypical views.

Apart from theoretical foundations, the study lacks adequate control variables such as, for instance, knowledge and acceptance of gender norms that could inform the results better. Also, objectives of the study are described vaguely. It is puzzling why the authors focus on personality since the results about gender differences in this area are mixed. This is an important drawback of this design - since conflict management styles are potentially associated with gender norms/expectations, there should be some measure of gender norms, how those norms are socialised, or at least how a person is likely to self-present themselves in accordance with the expectations. Without controlling for this, the results could be an artefact.

Given all these, also the contribution of the paper is vague: what more do we learn above the info that some dimensions of personality, biological sex, and management styles somehow correlate. But why? How do the authors want to convince the readers that the results are more than spurious correlations? I am not claiming that they are, just that so far the justification for the study is weak. Taken together, both the introductory part and the design are not convincing.

The part "Present study" is needlessly wordy. The associations between specific personality traits and conflict management should be explained in the Introduction. The mere fact that the Authors use FFM is sufficient to understand how they define the traits – they are bound by the model itself.

A minor comment: those are not "trap questions," but rather attention checks or control questions.

The Authors also claim that "No financial reward was provided for participation in the research." I find this claim questionable. The data were collected through "a panel," so I assume there was an agency involved to collect the data. If this was the case, then participants were remunerated by the agency through internal procedures, and the Authors or a party that subcontracted the data collection needed to pay for it.

The discussion starts with a claim that "the study aimed to answer the question about the actual importance of gender in conflict management styles," which is inconsistent with the claim that the aim was to investigate the role of personality. I believe this inconsistency is associated with the lack of model justification. By focusing more on the model foundations, the Authors would immediately see which aspect of the model is focal. Given mixed results about gender differences in personality, the mediation model requires better justification and, obviously, a discussion of the size of the differences when they are found at all.

Paragraphs 2 and 3 in the Discussion are inconsistent: if there is support only for an indirect model, then it is not justified to write about women's preferences. If there is no direct relationship between gender and management style, then it should rather be: instead of biological sex, preferences for a cooperative style were associated with personality traits.

The entire fragment starting with "Finally, what was unpredicted and unexpected in our study was the finding of a positive relationship between Agreeableness and Avoiding" is speculative, and I am not convinced that (i) the association between Agreeableness and Avoiding is puzzling at all and (ii) that it could be explained by country-level Hofstede results - how this could explain variability in your single-country sample?