Author’s response to reviews

Title: Self-Reported Psychopathy in the Middle East: A Cross-National Comparison across Egypt, Saudi Arabia, and the United States

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Author’s response to reviews:

Reviewer #1:

I would be grateful if the authors would add a short consideration of the cross-cultural validity of big 5 personality measures. There is some relevant coverage in the discussion, but I think this needs to be covered more broadly and in the introduction, as it is relevant to the interpretation of the results presented.

Response: We agree that a brief discussion of the cross-cultural validity of the FFM would be a helpful addition to the Introduction. Thus, we have added a paragraph to the end of the Introduction (see pg. 7) briefly describing the cross-cultural FFM literature and noting that although the FFM has been found to be reasonably robust across cultures, Openness may be one notable exception with less consistency in its manifestation across cultures.

The approach to analysis is certainly acceptable. However, I did wonder whether the authors considered running confirmatory factor analyses to see whether the configuration of items on factors held across culture. Setting this up as a multi group analysis and testing successively tighter forms of invariance might have been the way I would have approached the analysis. This model could have been extended to include the FFM correlates as well. I wonder if the authors have considered this approach?

As I read the ms, further, it appears that the authors have conducted analyses of this sort (p 19 line 46 and beyond), and say that the factorial structure may differ between cultures and that this may explain some of their findings above which assume that the factor structure is the same.
I feel uncomfortable with the current presentation of these results. In general, I think it is usually unhelpful to refer to analyses in the discussion that are not presented in the results, as the methods and findings are not presented fully enough to evaluate them. I agree that the small sample size etc may weaken these analyses and the finding got other studies must also be considered. However, as I read the ms, I thought this sort of analysis would be appropriate and I think other readers will agree. Therefore, I would prefer these CFAs to be reported more fully in the results section.

Response: We very much appreciate the Reviewer raising this concern as we struggled with where it would be best to put these analyses/results. Consistent with the Reviewer’s suggestion, as well as that of the Editor, we now include these results in the Results section (see pg. 17) and also describe these analyses in the Analyses section (see pg. 11).

Spelling out the abbreviations in the table titles would make them easier to read.

Response: Abbreviations in the table titles have now all been spelled out out.

Reviewer #2

Given the aim of the study, it is necessary to report and discuss: 1) the factor structure of the scale in each sample; 2) a multi-group analysis which investigate the feasibility of several levels of measurement invariance. This information is essential to evaluate the cross-cultural generalizability of examined constructs. I think this can be done with data at hand. Therefore, I suggest to present in more details the analyses reported in the limitation section (which should appear as study results).

Response: As noted in our response to Reviewer #1 above, we have expanded our description of the multi-group CFA analyses and now include these results in the Results section (see pg. 17) and also describe these analyses in the Analyses section (see pg. 11). We thank this Reviewer, as well as Reviewer #1, for this helpful suggestion.

Maybe I missed something, but I am not sure why FFM personality correlates of Psychopathy were not reported for the Saudi Arabian sample.

Response: As described now in more detail in the text, (see pg. 10), Saudi participants were unfortunately not administered the NEO-FFI thus FFM data are not available in this sample.

The hypotheses (p. 7) are poorly developed. Moreover, they refer to the correlations between FFM and PPI-R, without any discussion about how these are expected to vary (or not) across cultures.

Response: We thank the Reviewer for encouraging us to more fully develop our FFM—PPI-R hypotheses. As we now describe more fully on page 8, this is the first study of these questions in Middle Eastern Arabic-speaking samples. Nonetheless, we advance a number of provision a priori hypotheses deduced from the broader cross-cultural literature.
About the extremely low reliability coefficient of the openness scale in the Egyptian sample: Authors might refer to earlier studies which have noticed that openness has not been consistently replicated in non-Western counties (e.g., Cheung et al., 2001; Schmitt et al., 2007).

Response: We very much appreciate the Reviewer encouraging us to discuss potential explanations for the low reliabilities of Openness and bringing these references to our attention. As described above in response to Reviewer #1, we now include an additional paragraph concerning the cross-cultural validity of the FFM and in this paragraph note that Openness appears to potentially evidence less cross-cultural consistency than the other domains (see pg. 7). Further, we now also include reference to these previous findings in the Discussion section when discussing the low reliabilities for the Openness scale (see pg. 22).

The moderating role of gender has been examined without any rationale or hypothesis about expected results, which are presented in an exploratory way.

Response: We very much appreciate the Reviewer raising this concern as we agree that our inclusion of the gender analyses should have included more of a rationale. We have thus added an additional paragraph to the end of the Current Study section (see pg. 8) in which we describe the potential role of gender and, given how little is known regarding the role of gender in Middle-Eastern samples, describe these analyses as more exploratory.

Reviewer #3

1. One of my concerns pertains to the translation-based issues. First, the authors describe conducting a translation and back-translation of the NEO but never indicate how (or even if) any observed discrepancies identified in the back-translation were resolved or corrected. Second, while I am certainly not an expert on the Arabic language, my understanding is that there are important differences in dialects across countries (or so I have been told, by Arabic linguists). This leaves me wondering what dialect was used to develop the Arabic translation of the PPI and the extent to which this version was understandable by both Egyptian and Saudi participants (and even whether significant regional differences exist in the language between these two countries). The authors note that the lack of significant elevations in inconsistency scores supports the understandability of this translation, but that is a very crude index and would likely only identify extreme levels of confusion or near-random responding.

Response: We appreciate the Reviewer raising these questions as translational issues are always important to consider and describe. We have expanded upon our description of the translations of the instruments and have now included a new section, “Translations” (see pg. 10). Further, we have moved what might have been a confusing footnote regarding our approach to resolving back-translation issues to the main text of the document (see pg. 10).

2. Is there a reason why the NEO was not administered to the Saudi sample? A brief explanation would be helpful.

Response: As described in our response to Reviewer #1 above, and as now described in more detail in the text, (see pg. 10), Saudi participants were unfortunately not administered the NEO-
FFI as a result of time constraints when these data were collected and thus FFM data are not available in this sample.

3. I would suggest the authors provide more descriptive data, including means/SDs for the PPI and some indication of the percentage in each sample that falls above the cut-off threshold for identifying elevated levels of psychopathy. I realize that the PPI was not intended to "diagnose" psychopathy but interpretation of these data does hinge on knowing the general levels of psychopathic traits in these samples.

Response: We have added means and standard deviations for the PPI-R scales to Tables 2-4. Consistent with recommendations in the PPI-R manual (Lilienfeld & Widows, 2005), we have not, however, indicated percentages falling above a cut-off, though. Indeed, as described in the manual, there are not appropriate cut-offs for PPI-R scales/factors given the inherent dimensional nature of the scales specifically and, more generally, the large literature that has failed to extract taxa from psychopathy scales. Further, given the large literature on the structure of psychopathy, we do not agree that this is needed to interpret our findings.

4. The description of correlational analyses is very long-winded. I cannot offer a simple solution for this problem, as there are quite a few analyses, but I would suggest the authors rely more on the tables and do whatever seems feasible to avoid such a long, repetitious account of the findings in the text.

Response: We have attempted to tighten this section up as much as possible relying more on the tables, but are open to any suggestions that the Editor might have to tighten this more if needed.

5. I was surprised to see "post-hoc" analyses described in the Discussion (p. 19). I think these analyses are important and deserve a fuller treatment, as well as placement in the Results section. In fact, it would seem that these analyses are critical to interpreting the "primary" analyses. Perhaps the authors intend to make factorial invariance the focus of a separate paper but it seems a critical precursor to the analyses reported here.

Response: As noted in our responses to Reviewers #1 and 2 above, we have expanded our description of the multi-group CFA analyses and now include these results in the Results section (see pg. 17) and also describe these analyses in the Analyses section (see pg. 11).

6. The Discussion section also raises a number of concerns regarding the accuracy of the conclusions rendered. For example, in the first paragraph the authors conclude that the construct of "psychopathy is meaningful in cultures that are markedly different from those in the Western world." I don't see the basis for a global declaration of this sort. While there are some aspects of the construct that seem similar, the focus of these analyses is quite narrow -correlations within PPI scales and between PPI and FFM scales. These analyses do not address the essence of the construct, or the breadth of external correlates one might want to see before reaching such a broad conclusion. Moreover, the analyses reported raise serious doubts as to whether the construct of psychopathy is truly similar across cultures. For example, the findings regarding coldheartedness, which comprises a critical element of the psychopathy construct, is highly problematic in the Middle-Eastern samples.
Response: We very much appreciate the Reviewer bringing these astute concerns to our attention. Upon reading the manuscript again with these concerns in mind, we agree that we overstated some of the implications for our findings. Further, we agree that a bit more discussion of the potential concerns that may follow from our results with regard to the cross-cultural generalizability of psychopathy is warranted (particularly with regard to Coldheartedness). We have thus carefully revised the document throughout to temper any conclusions these data may lead us to omitting/revising any global statements. Further, we now include more discussion of the potential concerns with regard to cross-cultural generalizability of psychopathy, particularly for Coldheartedness (see, for example, pgs. 17-18).

Whether that reflects a problem with the Arabic version of the PPI or a difference in the construct is unclear, but its importance seems glossed over by the authors. Likewise, I am intrigued by the fact that gender played such an important moderating role in the Arabic samples, yet does not in US samples. Again, this might reflect a critical difference in the nature of the psychopathy construct yet its importance is downplayed in the Discussion. Note that I do not mean to suggest that the Discussion ignores these interesting findings, but that the findings should lead the authors to question whether there are important differences in the construct of psychopathy across cultures - a question that does not seem to be given serious consideration.

Response: Again, we very much agree with this suggestion and have revised the Discussion to more fully describe these potential concerns noting that it is not clear from our results whether these potential issues are a reflection of the Arabic version of the PPI-R or a difference in the construct across cultures (see pg. 20). We have also expanded this discussion with regard to our gender findings (see pgs. 20-21).

7. Another aspect of the Discussion that seems overstated include the conclusion that "psychopathy is a multifaceted construct" (p. 16). While I'm sure this statement is true, the analyses in this study do not really address this issue.

Response: We have tempered this statement and have also included a bit more description with regard to the way in which our findings, taken together with previous findings, support such a statement (see pg. 18).

8. Finally, while generally quite well written, there are a number of minor typographical errors in the manuscript including deviations from APA style, unbalanced parentheses, and missing words/odd phrasing (e.g., p. 6, lines 27-33; p. 8, line 33). The placement (and meaning) of the footnote is also confusing - it seems, if I understand the intent of this footnote correctly, to be misplaced in the text.

Response: We have carefully proof-read the manuscript and have corrected any/all typographical errors throughout. Further, as noted in our response to an earlier comment by this Reviewer, we have moved what might have been a confusing footnote to the main text of the document (see pg. 10).