Reviewer’s report

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

Version: 1 Date: 12 Sep 2015

Reviewer: Barry Rosenfeld

Reviewer's report:

Thank you for the opportunity to review this interesting manuscript. The topic of cross-cultural validity, whether regarding mental disorders or assessment techniques, is an important one and the authors should be commended for taking on this issue. There are a number of strengths to this paper as well, including the large sample size and the methodological approach of analyzing associations between psychopathy and FFM personality characteristics. My primary concerns noted below pertain to the presentation and interpretation of the study findings, and should be issues that can be readily addressed in a revision.

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.

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

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 (as well as how these samples compare to other, previously published or "normative" samples).

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.
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.

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. 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.

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.

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.

These comments notwithstanding, the authors should be commended for an interesting and thoughtful manuscript that addresses a critical issue.
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Yes

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