Reviewer's report

Title: A Reversed Gender Pattern? A Meta-Analysis of Gender Differences in the Prevalence of Non-Suicidal Self-Injurious Behaviour Among Chinese Adolescents

Version: 1 Date: 31 Mar 2017

Reviewer: konrad bresin

Reviewer's report:

Overall the authors have done an excellent job addressing my concerns. In particular, I enjoyed the analyses looking at region. I just have a few minor points that I think would be worth addressing.

In the introduction, I think it's worth making an argument for why it's worthwhile to look at gender and NSSI in China, given that there's already an international meta-analysis. It seems like it would be easy to add a sentence noting that it's difficult to apply the results of Bresin & Schoenleber (2014) to China, given the gender differences in suicide in China. I also think that the rural, urban, Hong Kong distinction should be brought up in the introduction not the method.

I think you should have a sentence in the method section explaining why you choose to do the odds ratio with >1 meaning a bias for men. Bresin & Schoenleber (2014) did it the opposite way, so it's worth noting for people to not get confused across papers.

I think that there's a typo in relation to the clinical samples. In the abstract and paper, you report, "(OR = 0.50, 95% CI = [0.42, 0.60], p > 0.1)". Either the CI or the p is wrong, because if 1 isn't in the interval the p-value should be less than .05.

In the discussion in the last paragraph on page 12. I think you could add a sentence tying the rural versus urban results to the gender conflict because to me it seems to support your case. Maybe I'm missing something though.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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Yes

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I am able to assess the statistics

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