Reviewer's report

Title: Acceptance of sexual minorities and the mediating role of social capital on health and well-being among members of same-sex and opposite-sex couples: a cross-European study.

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Reviewer: John de Wit

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As the authors note, health disparities between same-sex attracted and opposite-sex attracted citizens are increasingly attracting attention from public health and social science researchers, as well as from policy makers. This study draws upon a Europe-wide survey to examine if national differences in acceptance of same-sex attracted individuals is related to the health and well-being of same-sex attracted people, relative to opposite sex attracted people, and whether any such association between acceptance and wellbeing is mediated by social capital. While interesting in its aim, I have several major conceptual and analytical concerns that temper my enthusiasm for the paper in its present form. I have limited my comments to what are termed major compulsory revisions.

I find the description of the conceptual approach of the study in the introduction somewhat confusing. It’s not entirely clear what the role is of the minority stress model, as adapted by Meyer for sexual minorities. The main issue seems to be that social acceptance is related to health and wellbeing and Meyer proposes an explanation for that by specifying mediating mechanisms. These do not feature in the data collection and analyses. Most importantly, the relation between the minority stress explanation and the social capital explanation as authors see this remains unclear. Are these considered complements or alternatives?

Furthermore, the description of the notion of social capital is not entirely clear; the definition of the concept as a whole as well as that of the structural and cognitive components requires some work to ensure the reader understands the position of the authors. Under limitations I would also expect a discussion of the validity of the measures of social capital included in the study, in particular in view of the diverse uses of the concept and its rather fuzzy conceptualisation. There is also an issue with the location of the ‘source’ of social capital. In the explanation linking social capital to outcomes, social capital seems entirely located in the dominant social group, which could feasibly result in a deficit for same-sex attracted citizens. However, social capital may also be derived from social relations and networks in a more specific, same-sex attracted community, as has been long argued and investigated. (Authors note a more specific locating of social capital in the discussion, but don’t go all the way in their reasoning.) To the extent that same-sex-attracted people have strong ties within their community, the deficit in social capital hypothesis as presented is incomplete.
Note that the reasoning linking social capital and outcomes can be simplified.

The authors suggest that, in light of wide differences between countries, the Europe-wide social attitudes survey is particularly suited for the exploration of the consequences of structural discrimination on health. To that end, they convert individual-level assessments of social capital and LGB acceptance into country-level mean scores that then are included as higher-level covariates, alongside with individual-level covariates, including social capital. Importantly, however, critical details regarding the data analysis approach are currently missing and information provided is confusing (Are path analysis undertaken by means of regression analysis, as suggested on page 6? Currently structural equation modelling would be expected for path analysis, and that these were undertaken is suggested by the goodness of fit indicators provided). Also, LGB acceptance was measured with an item that seems to tap into participants’ personal acceptance and it is questionably to what extent this item is a valid indicator of social acceptance; there might be smaller between country-level differences in acceptance rates as reported by same-sex attracted participants (i.e., self-acceptance) than by opposite-sex attracted participants. Also, I understand that a country-level mean score of individual scores was included to assess country-level effects, which seems a rather crude and unusual way of undertaking multi-level analyses, for which dedicated analyses approaches are available. This may however be a reflection of missing information. It’s also not fully clear how mediation was assessed statistically and those details seem not to be presented. In particular, how did authors estimate the indirect effect? Table four is suggestive but not conclusive regarding the approach taken and further details are needed.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**
I declare that I have no competing interests