**Reviewer's report**

**Title:** Social capital is associated with lower mosquito vector indices: secondary analysis from a cluster randomised controlled trial of community mobilization for dengue prevention in Mexico

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**Reviewer:** Mahesh Karra

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While the research premise of this study is an interesting and understudied one (i.e. to what extent does social capital, as measured by a range of factors, contribute to positive health behavior, as measured by increased household control over the Aedes aegypti mosquito vector?), I found the authors' analysis to be limited and incomplete given their study aims. Given that the authors have access to data from a randomized controlled trial that specifically intervenes on community relationships through social mobilization, it was my hope that they could have exploited the random variation that is generated from the trial to demonstrate how the community mobilizing intervention's causal effect on vector control could be mediated through the social capital channel. While it could be possible that the authors are, in fact, seeking to conduct a mediated analysis, whereby a causal DAG for this analysis would look like: INTERVENTION → SOCIAL CAPITAL → VECTOR CONTROL, it seems that the primary focus of this paper has been to evaluate only the second half of this DAG, i.e. SOCIAL CAPITAL → VECTOR CONTROL, and that too at a far less rigorous associational level. The authors could have better identified the social capital channel by, for example, using the random assignment from the trial as an instrument for social capital. Some specific comments that I have are as follows:

1. The social capital index: while I do not have any outstanding issues with the construction of the index itself (I think that the authors' methods are well-justified in this regard), I would like to know how sensitive the Mantel-Haenszel estimates are to variations in the factor variables that are utilized in the index. It would be important to conduct some sensitivity analyses to demonstrate that the estimates are relatively stable across a convincing range of index constructions.

2. While Mantel-Haenszel methods are often used for binary exposures and outcomes, it seems like the authors have a lot more variation in their social capital scores and in many of the other covariates that are likely to be available in their data set. By dichotomizing variables, the authors throw away a lot of potentially rich variation that would allow for more nuanced inference. More problematically, however, is the issue of confounding and bias in the authors' estimates. By only controlling for one possible confounder through stratification (urban or rural place of residence), the authors fail to adjust for other confounders that could be driving both social capital and vector control outcomes. Given that Mantel-Haenszel methods are limited in their capacity to simultaneously control for multiple confounders, I would recommend that the authors consider using multivariate regression methods (e.g. logistic regression) to adjust for multiple sources of confounding.

3. In combining my previous comment with my main recommendation to the authors to exploit the random assignment to the mobilizing intervention, I would urge the authors to consider a more sophisticated interaction model that would seek to identify the differential effect of the mobilizing intervention by social capital score. Such a model with explicit interaction terms between the intervention and social capital would effectively be able to identify both the independent effects of these variables as well
as their joint effect. Alternatively, it might behoove the authors to run a similar saturated model of
the intervention on their vector control outcomes, but stratified by levels of social capital, which
would allow for the identification of heterogeneity (effect modification) of the intervention
effect.4 The authors acknowledge that the theoretical association between social capital and health
behavior is not well understood; however, they make little effort to move the research discourse of
the link between social capital and health beyond a mere association. While they generally cite the
role of social capital in promoting cohesive and supportive behaviors, they do not address how
these social behaviors would then translate into improved health outcomes. As their paper
currently stands, it seems as if the correlation between social capital and health is but a spurious
one. The authors attempt to forge a link in their discussion, but the current discussion on this link,
as it stands, is relatively weak, and a more rigorous examination of this link is warranted.5 A
minor comment: the authors presented four indices of the presence of Aedes aegypti at the
household (page 8); however, I only saw results presented for the first two indices throughout the
paper.

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