Reviewer’s report

Title: The benefits of co-location in primary care practices: the perspectives of general practitioners and patients in 34 countries.

Version: 2 Date: 09 Jun 2017

Reviewer: Maike Tietschert

Reviewer's report:

I think the authors have done important work to improve their manuscript. However, there are three comments, that I have made in my previous review, which require further improvement in addition to an editorial suggestion.

1. P.5 line 23: The authors mention that they removed patient covariates which were not significantly associated with the outcome variables from the model. Although, I value the authors efforts to reduce complexity I think removing non-significant covariates, in this case I think it would helpful to the reader to compare results of the exact same models. In any case, presenting model fit results would be more convincing of the authors choice.

2. P. 7. Line 30-37: Strength of primary care: I do understand that the authors did not design their study with a specific hypothesis about the strength of primary care. However, given that the authors have explored the effect of primary care strength and have found a significant interaction between primary care strength and the effect of co-location leading to different conclusions, results of the models that do not consider primary care strength do not add any value. If results were the same whether or not strength is considered, presenting table 6 as sensitivity analysis would be justifiable. However, after seeing the results of the models that do consider primary care strength it does not make much sense to me discuss the effect of co-location in models that do not consider primary care strength. In fact, in the discussion section, the authors themselves start discussing the results which considered primary care strength (First page of the discussion line 12-15). The fact that the authors studied both and are not clear about which results are interpreted in the discussion makes it very challenging to the reader to follow the authors' line of reasoning. Also, given that the authors examined the effect of primary care strength for the patient experience models and found significant influence, I think requires to do the same for the GP experiences. I think this issue has to be solved to make this paper publishable.

3. As mentioned in my previous comment, I think the discussion requires a juxtaposition of the findings on patient and physician experience. I do appreciate the authors have added to the limitation that GPs that are co-located may have a positive bias, however, I think more could be said about how results differ between the provider and patient models. The fact that
authors studied both patient and provider experience is a strong point of this manuscript which is currently underutilized. I suggest structuring the discussion as follows: Summary and interpretation of patient outcomes (using models that consider primary care strength). Summary and interpretation of GP outcomes (using models that consider primary care strength). Difference and similarities between patient and physician outcomes.

4. Editorial: Upon reading the manuscript again, I think readability of the paper could be improved and I suggest using a proof reading service to improve sentence and paragraph structures.

Thanks for the opportunity to review this work and I wish the authors the best as they continue to pursue these important questions.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**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?**
If not, please explain in your comments to the authors.

Yes

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If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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