Author’s response to reviews

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

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The benefits of co-location in GP practices: the perspectives of GPs and patients in 34 countries.

Dear Editor,

Thank you for sending us the reviewer’s additional comments. Below, we report the revisions we have made according to these comments (the main changes are marked by text in green in the manuscript).

Reviewer reports:

Maike Tietschert (Reviewer 1): 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.

We have updated the analysis of our study by reintroducing the same covariates in all models that involve patient experience. We have specified this in the statistical analysis paragraph in the Methods section. Essentially, the results did not change in comparison to the previous ones (Table 5, 6 and the second in Appendix), but they are now more easily comparable, as suggested by the reviewer.

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.

We did not modify the article by introducing the analysis of the effect of primary care strength also for GP outcomes. Indeed, we checked if there was an effect of primary care strength upon these outcomes, but the results of these stratified models did not differ from those of initial models without stratification (results presented in the letter to reviewers attached).

Therefore, re-writing the article would not bring more information to our findings, since the interaction between the level of strength of primary care structure and GP co-location is present only for patient experience.

At the beginning of the Discussion (lines 10-16), we had provided a summary of our findings, before discussing the results in detail. For this reason, we had mentioned primary care strength starting this section in order to explain the results concerning the relationships between GP co-location and patients’ experiences.
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.

We took the opportunity to improve the discussion of the results by underlying the comparison between the findings concerning patient outcomes and those concerning GP experiences. Although not reframing completely the structure of the section (which was designed to help the readers to follow the answers to the research questions), we agree with the reviewer to add value to our manuscript by discussing more the differences between the two perspectives analysed.

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.

As suggested, we have improved the manuscript by consulting a native English speaker for proofreading services. We attach a certificate.