Reviewer’s report

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

Version: 1 Date: 07 Mar 2017

Reviewer: Maike Tietschert

Reviewer's report:

General Comments:

This study addresses an important topic, that of GP co-location, which is highly relevant to many healthcare systems. The inclusion of 34 countries in this study is impressive and in doing so, this study can have far-reaching impact. I was delighted to see that the authors assess the influence of co-location on GP as well as patient experiences. I have a few questions/suggestions for the author's consideration mainly regarding the methodology section that could be clarified or augmented to enhance the value of this contribution. I will provide feedback per section.

Introduction:

p. 2 Line 3-10: The authors use co-location and team work interchangeably. However, while co-location may increase team work, it does not need to do so per se. The authors also highlight this point in their discussion. I would suggest reframing this paragraph, in particular the last sentence in line 9-10.

p. 2 Line 32-33: The authors hypothesize that patients of co-located GPs perceive a better quality of care in terms of continuity of care. Providing an argument for why the authors expect this relationship would be helpful. More providers in one organization that are responsible could ultimately also result in less continuity, as the patient is seen by more providers and hence more coordination is required.

p. 2 Line 43-44: The way this hypothesis is phrased evokes the expectation that the same experiences are measured at the GP and the patient level, while this is not the case.

Methods:

General feedback: The paper requires more information on the sample process, size and demographics at the different levels. Where multiple GPs included from the same organization or
only one? Does the number of GPs that were included equal the number of practices included? How many countries had a weak, medium or strong primary care system?

Note: I understand that detail on these numbers have been published elsewhere. However, some basic information will help the reader to better understand the methodology.

Dependent variable: Line 47-50: The authors write "We used scales, measuring accessibility, comprehensiveness and continuity of care with their GPs". Although the authors mention that these scales have been published elsewhere, briefly describing these scales in one or two sentences would be helpful in order to increase understandability of the method section.

Statistical analysis:

- P. 4 line 54: The authors used multilevel linear regression analysis. However, in the first table of the appendix, scales are described to be 1 to 3 or 0 to 1. I may be wrong here, since scales are not described in detail but it seems that these variables use an ordered categorical scale. If dependent variables are categorical, this would require logistic regression analysis. I was also wondering why there is no table presenting the distribution of dependent variables at the patient level by country, while there is one at GP level. In any case providing more information on the scales would be helpful.

- P. 5 line 1: What do the authors mean with "relative" results?

- P. 5 line 23: The authors mention that they removed patient covariates which were not significantly associated with the outcome variables from the model. It would be helpful to receive more information about which covariates were excluded. I also believe that the authors do not describe what the effect of control variables were in the remaining manuscript and no information on covariates is provided in the tables other than a footnote which lists the covariates that were included. It would be more convincing if the authors describe why they have excluded covariates. I assume that this step was taken to decrease complexity of the model. Providing information on improved model fit would be helpful in supporting this decision. Given the fact, that the authors use different covariates in the different models, it would also be helpful to report the covariates in the regression output.

- P. 5 line 42: Which version of Stata was used?
Results:

- P. 7, line 30-37: I was delighted to see that the authors make a distinction between systems with strong, medium and weak levels of primary care systems. However, I was wondering why the authors did not do so from the onset of the analysis. The average scores of different forms of co-location in weak, medium and strong countries show a large degree of variation and hence an effect of primary care strength on patient and provider experiences seems likely. Not distinguishing between these levels may even out the effect of co-location on provider and patient outcomes, where the negative effect in weak systems is so strong that the overall effect on patient experiences in also slightly negative. Not distinguishing between these levels may also affect co-variates that were previously excluded. For example, extra opening hours may not be so relevant in a highly sophisticated primary care system, because after hour care may be organized at a central level. In weaker systems, however, efforts by individual organizations or practitioners may have a much greater influence in the absence of formalized structures. Given the strong variation, not differentiating between these levels may result in a loss of important information and results that are hardly interpretable. Hence, I would suggest that the authors differentiate the three levels of primary care system strength in their primary analysis, either by presenting separate analysis or by including a cross-level interaction. Information should be provided on the sample size/power for each level of primary care system strength. I would also suggest differentiation between these levels for patient and GP outcomes. If the authors had explicit reasons not to perform separate analysis for weak, medium and strong primary care systems in the GP level analysis, it would be helpful if these reasons were clearly stated.

Discussion:

P. 8, line 35-48: As I already mentioned, I do not think the analysis without considering the differences in primary care level strength are very informative. Instead of describing these results, I would focus on the second part of this paragraph from line 48 onwards. I would rather spend more words in the method section on describing sample size and scales. The discussion could also benefit from a juxtaposition of the GP and patient findings. For example, the authors write in line 6, p. 8 "In fact, more comprehensive primary care gives patients access to healthcare services at the primary care level for which they would usually have to go to other providers". Although this explanation seems plausible, the author's analysis of their data show that co-location is negatively association with access as perceived by the patient. These contradictions given rise to questions of why findings are different and authors should elaborate on the differences that they found regarding the GP and patient experience. Could GPs for example be more biased towards co-location. GPs may consciously decide to group together with different providers and hence may be positively inclined toward co-location. Patients on the
other hand may or may not actively chose for their GP to group with other professionals. The author's reflection on this potential bias in the limitation section would be helpful.

If the authors address these issues, I think this manuscript can make great contributions to better understanding the influence and potential of co-location and given the large number of countries that were included make widely applicable contributions.

**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.

No

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No

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I am able to assess the statistics

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