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.

Authors:
Manila Bonciani (m.bonciani@sssup.it)
Willemijn Schäfer (W.Schafer@nivel.nl)
Sara Barsanti (s.barsanti@sssup.it)
Stephanie Heinemann (sheinem3@gwdg.de)
Peter Groenewegen (P.Groenewegen@nivel.nl)

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

Dear Editor,

Thanks for the Reviewers’ comments that give us the opportunity to clarify some aspects of our study and to improve the quality of the manuscript. Below, we are reporting the revisions we did according to the reviewer’s comments (text in green in the manuscript).

Reviewer reports:

Maike Tietschert (Reviewer 1): 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 on 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.
We sincerely thank the reviewer for the positive remarks on our study.

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.

It was not our intention to use co-location and teamwork interchangeably, since we are aware that the former does not necessarily coincides with the latter. We introduced this point in the Background and we discussed it later, considering the results emerging from the study. We reframed the sentences mentioned by the reviewers and we tried to underline again in the Discussion the difference between the two terms.

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 results in less continuity, as the patient is seen by more providers and hence more coordination is required.

We tried to explain better why we expect more continuity of care when a GP is co-located with other GPs and/or other professionals. In our framework, information will be more easily available when patients consult their GP and other professionals in the same practice, compared to when they consult other professionals outside the practice of their GP. Although it is true that the increased complexity of the organisation requires more coordination, the proximity of the professionals due to co-location helps them to exchange information more easily, also informally.

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.

The reviewer is right in pointing out that we used different variables/measures at the GP and the patient level. In order to avoid misunderstanding on this point, we corrected the manuscript by referring always to the dependent variables at the GP level as “GPs’ outcomes” and to the dependent variables at the patient level as “patients’ experiences”.

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.

We introduced in the Method section additional information concerning the QUALICOPC study, which is the source of data of our study. In particular, we explained that:

- Nationally representative samples were used in the majority of countries;
- One GP per practice was sampled;
- Target response was 220 (75 in the four smallest countries);
- Age and gender of participating GPs was compared to national statistics and representativeness on these two variables was generally good;
- Strength of primary care systems at national level was in tertiles.

We tried to balance the need of readers to understand better the methodology and that to focus on the key information relevant for this specific study. In this sense, we did not provide tables with age and sex of GPs and of the patients, which are available in other papers and would require two additional big tables to the detriment of parsimony and readability.

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.

We introduced in the Method section additional information clarifying the dependent variables at the GP and the patient level. In particular, we specified the content of the scales and their process of construction.
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.

We explained better in the manuscript that the scales were built from several items and constructed in a multilevel latent class analysis, resulting in variables that can be treated as continuous variables. We did not insert tables with the distribution of dependent variables at the patient level because they were already presented elsewhere (https://www.nivel.nl/sites/default/files/bestanden/Appendices-Assesing-the-potential-for-improvement-of-PC-in-34-countries-WHO-Bulletin-2015.pdf). The dependent variables concerning GPs’ outcomes were not already published and therefore we added the tables in Appendix.

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

We modified it with “the results related to these analyses” in order to avoid misunderstandings.

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.

We mentioned in the Method section which co-variates at the patient level were included in the models (sex, age, education, ethnicity, household income, self-reported health status, chronic conditions, main reason for the visit, having their ‘own’ doctor). We took into account all the variables that are relevant in the literature for the dependent variables analysed. In the model analysing the relationship between GP co-location and accessibility we removed the covariate
chronic conditions that was not significantly associated with the outcome variable, while in the model analysing the relationship between GP co-location and comprehensiveness of care we removed the covariates education, household income and main reason for the visit for the same reason. This way we contained the complexity of the models without losing information on the relationships between the independent and dependent variables. In fact, the covariates did not affect other aspects of the models, their coefficients in themselves were not very interesting and they were not a part of our hypothesis. Considering these aspects and the fact that the models also have been stratified in the last part of the analysis, we have chosen to remove some co-variates. Included co-variates have been all reported in the table footnotes. We did not report the co-variates in the regression output, because providing the coefficient for all covariates for all models performed would have required a large number of tables, which are of only very limited interest to the readers, with an effect of distracting them from the focus of our study.

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

We used Stata 14.

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.

We did not design our study with a specific hypothesis about the strength of primary care. However, when observing i. the results of our analysis that were opposite to our second hypothesis, ii. the differences in the dependent variables at the patient level between countries and iii. the divergence between the GPs’ perspective and that of patients, we chose for a straightforward analysis in order to understand more deeply the relationship between GP co-location and patients’ experiences. Then we performed the models with the dependent variables at the patient level by stratifying them per category of the strength of primary care. We considered these additional results very relevant, as the reviewer mentioned, and we decided to report them, although the study had already a broad approach that kept together the provider and user perspectives. At the same time, we tried to ensure parsimony and to avoid a potential feeling of confusion of readers by concentrating the analysis without performing the stratified models with the dependent variables at the GP level. Moreover, it should also be considered that our study has an additional complexity since it focuses on the interaction of two co-location variables (monoprosessional and multiprofessional GP co-location). Using a further cross-level interactions, as suggested by the reviewer, would have required a three-way, cross-level interaction. This kind of analysis would have been probably more difficult to interpret, with the possible consequence of weakening the study findings. Therefore, our choice was to leave this for future research in order to investigate the different relationships between GP co-location and GPs’ outcomes and patients’ experiences based on the differences of primary care by countries, also taking into account the organisational strategies to implement GP co-location, as we mentioned in the Conclusions.

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.

We provided more elements for the discussion of the findings, but within the initial framework of this study, for reasons explained previously.

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.

As to the study limitations, we added the potential bias mentioned by the reviewer.

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.

Ling Lin, Ph.D. (Reviewer 2): The authors provided thorough research and discussion regarding the co-location in general practitioners' practices. The authors concluded that mostly co-location provided positive outcomes for the GPs and increased collaboration among different providers. However, they also showed that GP co-location could be related to less positive patients' experience and they were able to identify a few areas with the most significant less-positive experience on the patient's side. A follow-up project would be useful to conduct a more direct or prospective study to test these theories and thus lead to actionable recommendations to the health care community to address the less positive experience from the patients.

We thank the second reviewer for the positive comments and for the suggestion for further studies.