Author's response to reviews

Title: Under the same roof: co-location of practitioners within primary care is associated with the comprehensiveness of care for patients with chronic conditions.

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Author's response to reviews: see over
Thank you to the Editor and to all the reviewers for assessing the paper and for your helpful feedback. **Amended text is in bold in the revised manuscript.**

**Reviewer:** Jacqueline Cumming

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**Major compulsory revisions**

Overall, this is an important and very topical piece of research. However, I have two major concerns, in particular with the reporting on the NZ case study:

1. Relates to the response rate. This is very, very low in the NZ case and I would like the authors to provide either evidence that those responsive is representative of the general practice population, or more convincing evidence that this is not a problem in interpreting the results. Any information on representativeness is better than the amount presented in the article at present (ie none).

Thank you for your comments on the response rate. There are four issues we will address: first, the NZ and Ontario responses rates may be similar; second, GP response rates are known to be problematic; third, we have done some comparisons between our NZ sample and other NZ national data; and fourth, we consider whether response rates may affect our specific analyses and conclusions.

Firstly, the response rate. The rate of response in the NZ sample was 12.8%, despite multiple mail outs, reminder postcards and incentives. That said, it might not be dissimilar to the Ontario rate of response. The two jurisdictions had slightly different approaches to sampling. Unlike the NZ investigators, the Ontario team was not able to access a register of General Practitioners, and so a variety of means were used to create a list of possible respondents. These people were then sent an invitation to participate, mailing the survey pack to those who indicated willingness. As such, the rate of response looks comparatively higher in this group than in the NZ sample. To
clarify this point, we have amended the text in the manuscript. The ‘Sample’ paragraph page 6 now reads:

We aimed to achieve 220 participants from both NZ and Ontario, in accordance with the protocol of the broader QUALICOPC project [29]. Networks from the Ontario College of Family Physicians and the Centre for Effective Practice were used to contact potential participants in Ontario; those who expressed a willingness to participate were sent survey packs (n=229) from which we received 184 completed questionnaires. A different approach was taken for the NZ sample, all practices were identified from telephone books, and registers held by the Royal New Zealand College of General Practitioners and the University of Auckland were sent survey packs (n=1373). From this mail-out and subsequent follow-up mail-outs, we received 168 responses, giving a combined total sample of 352 practices.

- While all survey studies struggle with achieving high response rates; those involving General Practitioners appear to be among the worst affected. Additionally, GP surveys in NZ and Canada report lower response rates than those in European countries (Cook JV, Dickinson HO, Eccles MP: Response rates in postal surveys of healthcare professionals between 1996 and 2005: an observational study. BMC Health Serv Res 2009, 9:160.) General Practitioners are a difficult group to access, reflecting their busy schedules, high administrative load, and possibly some survey ‘fatigue’ – we note that the ongoing Australian benchmark study of general practice activity has repeatedly described response rates of less than 30%.

- The NZ investigators explored the representativeness of their sample against some published national estimates. We were limited in the factors we could compare; noting also that we are interested in the characteristics of the practice/centre, and not the respondent practitioner themselves. Compared to the national average, the participating NZ general practices in our study were slightly less likely to be located rurally (15% compared to 20%). We discuss the possible impact of rurality below (point 3, Reviewer 2).

- In any survey, a rate of response less than 100% has the potential to introduce a selection bias. What is critical is whether there are factors associated with participation, that impact
on the associations of interest. We have discussed this possibility in our limitations, and cannot exclude the potential for bias from this source.

- That said, our analyses are based on the entire sample from both jurisdictions. We do not attempt any direct inter-country comparisons, nor make any claims of broader generalizability. We draw inferences only on the patterns and associations seen in our sample, conclusions which are supported by the current evidence.

In conclusion, we agree that the low response rates in both jurisdictions are a significant limitation on the external validity of our findings. However, our findings have reasonable precision, are based on adequate sample sizes from two countries, are consistent with both current theory around patient care teams and quality of care for patients with chronic conditions, and congruent with the published literature. As such, these findings are informative and helpful in guiding future research in this area. These points have been added to paragraph 2 of the Discussion, page 12 (see below).

This study has limitations, one of which is its lack of representativeness. Participation was voluntary, and recruitment was not pursued once more than 220 eligible practices had indicated a willingness to participate (in accordance with the protocol of the wider multi-country study) [29], even though a number of practices did not return surveys that they had agreed to complete. This approach ensured adequate power for the analyses, but may affect the external validity of the estimates. There are insufficient supporting data to investigate the representativeness of the samples at this time, so we cannot estimate the impact of selection bias. That said, it is difficult to formulate a mechanism whereby the relationship between the number of co-located disciplines and the facility to provide specialized chronic care differs according to a characteristic potentially associated with participation. Overall, it is possible the response rate of our study limits the generalizability of the results. However, our findings have reasonable precision, are based on adequate sample sizes from two countries, are consistent with both current theory around patient care teams and quality of care for patients with chronic conditions, and congruent with the published literature [9, 14, 15]. As such, these findings are informative and helpful in guiding future research in this area.
2. I am also not totally convinced that there are not other factors which might be in part responsible for the findings. In particular, in the NZ case it isn't clear what impact being in a PHO might be making to the findings; it may well be that it is the PHOs that are in part responsible for the increased availability of services. The analyses could identify which PHO each practice is in and see if this makes a contribution, or provide an explanation of why the PHO factor may not be important.

*We appreciate this comment. We have amended the text of the paper to highlight the potential role of this factor (paragraph 1 page 13), given below:*

*Organizational variables may also have an impact; there is evidence from Ontario that funding model is associated with the comprehensiveness of care.* [30] *While we do not have information about the PHO status of the NZ participants, it is also possible that being a member in a larger PHO with greater staff and financial resources may facilitate the capacity of the practice to provide more specialized services. Research involving these types of structural variables would help to explore how some practices were able to achieve their level of service provision within the current environment.*

*Reviewer 1: Grant Russell*

Thank you for the opportunity to review this article, Under The Same Roof: Co-Location Of Practitioners Within Primary Care Is Associated With The Comprehensiveness Of Care For Patients With Chronic Conditions. I have a series of recommendations on the article which I shall arrange under the requested subheadings. The article is succinct, clear and relevant to the issues that clinicians and policy makers are struggling with in the delivery of quality primary care. Statistics seem appropriate for a non statistician, and the findings well overviewed in the discussion. I think that the limitations section was particularly well written notwithstanding the reservations I have detailed below.

I have several comments that require attention:

1) Although the core question is well articulated, I do have some concerns that the definition of comprehensiveness used (line 122 - greater range of services, specialized programs, and specific
clinics tailored to their conditions) is different from broader constructs of primary care comprehensiveness. Canadian experts defined comprehensiveness as <<the provision, either directly or indirectly, of a full range of services to meet patients’ health care needs. This includes health promotion, prevention, diagnosis and treatment of common conditions, referral to other clinicians, management of chronic conditions, rehabilitation, palliative care and, in some models, social services>>.(1).

Thank you for this comment regarding the definition of comprehensiveness in primary care. We agree that the outcomes we have assessed encompass only some of the facets of comprehensiveness, and that our use of this term may unintentionally mislead the reader. We have included the definition of comprehensiveness you have provided, and have amended paragraph 2 page 5 to read:

Quality in primary care encompasses many dimensions, including comprehensiveness, defined by Canadian experts as “the provision, either directly or indirectly, of a full range of services to meet patients’ health care needs. This includes health promotion, prevention, diagnosis and treatment of common conditions, referral to other clinicians, management of chronic conditions, rehabilitation, palliative care and, in some models, social services”. While we were not able to explore all facets of comprehensiveness, we hypothesized that primary care practices with co-located non-physician members may offer broader services and specialized care for patients with chronic conditions: such as more equipment, dedicated programs, and specific clinics tailored to their conditions.

2) The methods are well described however there are 2 important issues that potentially have an impact on the interpretation of the data.
• Sampling methods were not particularly systematic, and it was hard to see how the practices responding compared to size, model and location of practices within each nation.

Thank you for raising this point. We agree that sampling may not be systematic, and this reflects the protocol and methods of the broader multi-national QUALICOPC study. We have discussed the issue of representativeness in our response to the reviewer above, point 1.
Questionnaires were sent to practices, but it seems (as suggested on line 169) that roster size was not established for the practice. We have an idea for the number of different professions at each site, but not the number of individual FTE GPs. There are some family health teams in Ontario with many dozens of providers at a number of satellite sites within the same organization. It was unclear whether such practices could be skewing the findings. Lack of data from Ontario on model is also concerning given the likely influence of model on a number of quality domains.

Thank you for these comments. We agree that roster size may be an important potential confounder and have included this variable as a 3-category factor (country specific tertiles) in the models. (This is noted in paragraph 2 page 7). Unfortunately, due to the international nature of the broader QUALICOPC study, we do not have specific information on the models of care in either NZ (i.e. PHO) or Ontario. However, we have amended paragraph 3 page 12 to reflect the potential role of organizational factors on the analysis (please see point 2 from the first reviewer).

With respect to the number of individual providers, the question pertaining to the number of disciplines was phrased as “How many of the following disciplines are working in your practice/centre?” Although it is possible that some GPs misinterpreted this question and also included staff not co-located, the distribution of this variable in the data does not suggest this occurred frequently. For example, while there were some practices reporting up to 9 co-located non-physician disciplines, the number of these was very few (only 12 practices reported more than 7 disciplines) and this variable had a normal distribution (mean = 3.98, median =4). Additionally, the distribution of this factor in the NZ and Ontario samples were very similar, despite different organizational and geographical factors.

3) as mentioned, the limitations well articulated, however several limitations are not mentioned. These represent my main concerns with the article:

• At least two recent articles evaluated comprehensiveness in Ontario primary care (2, 3) Neither were cited, but both demonstrate the importance of group (rather than panel) size on comprehensiveness, and measure rurality, and a number of other explanatory variables.
Thank you for these references. We concur that group size seems to have a role in the comprehensiveness of primary care. In our study however, we were focused not on the number of family physicians (given that roster size is related to FTE, we have made the assumption that the roster size variable included in the model at least in part indirectly considers this factor), but on the number of different disciplines co-located at the practice/centre. With respect to rurality, we explored the role of this variable in our analyses. We found that the practice location (as defined by the QUALICOPC survey into urban, suburbs, small town, mixed urban-rural and rural) had little impact on the effect estimates for the associations of interest and was not a significant confounder. While there is a theoretical argument for including this variable in the analyses irrespective of its influence on the estimates, we suggest that it is possible that there may be differences in how the two jurisdictions interpret these labels of practice location, such that the QUALICOPC variable we have available may be a source of differential measurement error. Additionally, the inclusion of this extra factor had a negative effect the precision of our estimates, and on balance we concluded that a more parsimonious model was preferred. We have edited the manuscript to include these points, paragraph 1 page 13 now reads:

However, it is possible that rurality may have a role – for example, rural practices may carry a wider range of equipment, reflecting their relative isolation from specialist and referral centers. That said, we found that the practice location (as defined by the QUALICOPC survey into urban, suburbs, small town, mixed urban-rural and rural) had little impact on the effect estimates for the associations of interest and was not a significant confounder. While there is a theoretical argument for including this variable in the analyses irrespective of its influence on the estimates, there may be differences in how the two jurisdictions interpret these labels of practice location, such that the QUALICOPC variable we have available may be a source of differential measurement error, and on balance a more parsimonious model was preferred.

• The outcome variables were well described (Provision of Disease management programs, Special sessions/clinics, level of nurse service provision and equipment use), but not necessarily representative of a consensus definition of comprehensiveness.
Thank you, we have noted this point and have amended the text throughout the manuscript to reflect this (as discussed under point one).

- With the limited perspective of comprehensiveness, there is a danger of over interpretation of the data. The reader (or at least this one!) is not convinced that the measure of comprehensive is sensitive enough or that critical explanatory variables have been assessed. For example, that it may be a core characteristic of the work of several professional groups (in particular dieticians and diabetes educators) to hold and run special clinics or disease management services as part of their normal work. Using a binary variable (service present/not present) a positive score would come from an occasional course delivered to a handful of patients. It is difficult to assume more without more detail, especially with the lack of data on practice size and location.

Thank you for raising this point. We have been discussing the importance of specific roles within the patient care team, and agree that this is an area that would benefit from further exploration. As noted in the text, a limitation of our study is that the findings cannot infer benefit to patients from these specialized programs/clinics or equipment, only that the practice has provides this level of service.

a) Major Compulsory Revisions The authors need to
- Either measure, or acknowledge the lack of data on rurality and practice size (as measured by physician FTE) in adjusting for the outcome measures.

Thank you. As discussed, we include a term for roster size in our analyses and have acknowledged the role of rurality as a potential confounder in the limitations section.

- Acknowledge broader definitions of comprehensiveness.

Thank you. Text amended, as noted above.

- Relate the finding to other investigations of comprehensiveness, particularly in Canada and NZ.

Thank you, the discussion has been amended and an additional reference included.
• Acknowledge the potential of confounding by multisite group practices – by definition these aren’t collocated, but would not be determined by the question: “Which of the following disciplines are working in your practice/centre?”

  Please see the response to this point above.

Minor Essential Revisions
Nil -

Discretionary Revisions?
A table (online) of the frequency of the different provider groups would be helpful.

  Happy to provide this, table now attached as Appendix Two.

The authors may like to consider whether, in the title, they should delete the definite article before “comprehensiveness”:

  ‘The’ has now been deleted.