Author's response to reviews

Title: The supply of General Practitioners across local areas: accounting for spatial heterogeneity

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Author's response to reviews: see over
Response to referee comments

In this revised version of the manuscript, all the comments from the referees have been addressed. The structure of the paper has been reorganised. The paper now includes a discussion about the limitations of the methodology and is substantially shorter in length.

Referee 1: Maria Pia Fantini

Major compulsory revisions

1. The first section should be rearranged. In particular, I suggest moving the second paragraph (Ln. 90-100) to the bottom of the Introduction (Ln. 129-130). Also, I would remove some sentences which are not suitable for this section of the paper (Ln. 133-139; Ln 141-144).

The first section of the manuscript has been rearranged according to the reviewer’s suggestion.

2. I think that the journal does not allow to write two introductory sections (i.e., Introduction and Background). Please move the first paragraph of the Background (Ln. 148-159) after the first paragraph of the Introduction. As for the second and third paragraph of the Background, you may move them to the Conclusions and (in a very shortened version) to the beginning of the Methods, respectively. Alternatively, you may delete them.

The Background section has been deleted with the first paragraph being moved to the introduction, and to aid in condensing the paper the second paragraph has been deleted while the third paragraphs has been shortened and added to the Methods section, an option suggested by the reviewer.

3. Please entitle the first section of the Methods (Ln. 176-255) as “Statistical analyses”.

The subheading “Statistical analysis” has been added to the methods section.

4. For the sake of consistency, please replace “small areas” with “postal areas” (Ln. 226).

As the reviewer points out, the language is inconsistent, “small areas” has been replaced with “postal areas.” The entire text has been edited to ensure consistency throughout the paper.

5. Please specify that the number of GPs per 1,000 persons is 2008 data (Ln. 258)

The specification that the data are measured in 2008 has been added “GP supply is measured as the number of GPs per 1,000 persons in 2008.” has been added to the Data section.

6. Please state at the beginning of the Results that the number of postal areas you have analysed is 2032 (Ln. 322-328).

A sentence indicating that the number of postal areas analysed is 2032 has been added to the Results section.

7. The Results section actually ends on P. 14, Ln. 354. Everything after that should be considered as part of the Discussion. As a whole, the authors must try their best to condense their thoughts from P. 14 to 21, because the paper is very long, and many concepts seem to be reiterated.

The Discussion section now begins where the reviewer has suggested. The Discussion and Conclusion sections have been condensed; repetition has been removed. The paper is now substantially shorter.
8. The authors state that “the variable capturing the proportion of indigenous persons is significant in the non-spatial Tobit model and the 10-neighbour band, but not significant in the 5-neighbour band” (Ln. 434-436). This appears inconsistent with the figures presented in Table 3 – the authors seem to have inverted 10 and 5
Agree. The 5 and 10 neighbour bands were inverted in the text. This has been corrected.

9. Another inconsistency between text and tables. The authors state that the coefficient of mortality rate “is negative in the spatial models” (Ln. 444); however, it seems to be positive (Table 3).
Agree. Mortality rate is positive in all models; the reference to negative mortality rates has been removed.

10. The authors should address some limitations of the adopted methodology, which, in my opinion, has room for improvement. For example, the choice of a fixed number of neighbours for each postal area is debatable: coastal areas may have less, because in some cases the farthest areas will result to be “too upcountry and far”; rural, remote and very large areas also may have less, because villages that are more than 1,000 kilometres apart cannot be considered “neighbours”. I think that a possible solution is to consider neighbours of coastal and remote areas the 3 closest, and neighbours of all the other postal areas the 5 closest. Or, as an alternative, 5 and 10 respectively.

Modelling a smaller number of neighbours for rural areas and a larger number for urban areas is a novel suggestion to help overcome the limitation of heterogeneity in the size of the postal areas. One of the limitations of adopting a spatial approach is that in order to add in the spatial dimension a symmetric spatial weights matrix needs to be defined. Matrices are constructed using geographic proximity and can take on many different forms. Distance-based neighbours are neighbours which fall within a pre-specified distance from one another. Distances are based on coordinates (latitudes and longitudes) and distance is in the Euclidian form. For the statistical analysis a symmetric matrix is needed, therefore distance-based matrices are limited to the selection of a pre-specified number of neighbours rather than a pre-specified distance. It is for this reason that we could not use a ‘catchment area’ approach, nor a flexible number of neighbours approach (as suggested by the referee). Creating a program where symmetric matrices are not needed may be feasible (and would indeed be very useful), but this is beyond the scope of this paper. Further, a model where the number of neighbours were specified by the number that the postal area actually had would generally be considered endogenous to the model thus creating an endogeneity problem. We have added a discussion of this issue to a methodological limitation discussion in the Discussion.

In addition, (although not included in the paper) we ran a (non-spatial) model including only major cities to assess if the results remain similar when considering only urban areas. The results are consistent with the results that include rural areas except that the variable “percentage of the population that is indigenous” is no longer statistically significant, which is consistent with our hypothesis in the paper that this variable is spatially correlated to an omitted variable.
We have also highlighted other limitations throughout the paper as they arise. In particular, we now highlight the assumption that mortality rate is an exogenous variable, and the fact that we do not have morbidity data nor data on the price of services.

Minor essential revisions

11. I have found some punctuation typos (e.g., Ln. 150, 179, 260). Please check all the paper.

The punctuation errors pointed out by the reviewer have been corrected. The manuscript has also been edited to correct other punctuation errors.

Discretionary revisions

12. Figure 1 clearly shows that there is considerable variation in GP supply across postal areas, but no significant spatial clustering. However, it may be of some interest to present, as additional files, also the detailed maps of the five Australia’s largest cities, where postal areas are very small.

A reference to the visual assessment of no spatial clustering has been added to the results section. Figure 1 has been updated so that it is larger. In addition, two figures “metropolitan Sydney” and “metropolitan Melbourne” have been created and added to the document as Figure 2 and 3 respectively.

Referee 2: Andrea Donatini

Minor essential revisions

1. End of line 479: I feel there’s an extra semi-colon.

This punctuation error has been corrected.

2. Generally a review of punctuation is suggested: for example I have the impression that in lines 521-524 commas were not used appropriately, thus making it difficult to read the article.

The punctuation errors pointed out by the reviewer have been corrected. The manuscript has been edited to correct other punctuation errors.

Discretionary revisions

1. page 1: the shift in topics between the first paragraph (lines 81-88) and second (lines 90-100) appears to me to be a bit drastic: the first paragraph, correctly, describes the issue of GP location when, all of sudden, the authors switch to a detailed and technical description of the methodological issues involved in geographic referencing. A short description of geographical referencing might benefit.

The first section of the manuscript has been rearranged; the discussion of the geographical referencing has been moved to the end of the introduction. In addition, we have now briefly defined geographical referencing.

2. The article deals mainly with the australian system: the australian primary care system does not find a similar counterpart in most European countries. This implies that part of the results of the article might be useful only in the australian context and might not be extended to other countries. One possible suggestion would be to validate the same approach using data from a Beveridge or Bismarck health system.

Although the funding of primary care systems may differ greatly across countries, the organisation of primary care systems, particularly in terms of arrangements being contingent upon geographic boundaries is analogous to
many countries. Reference to this has been added to the paper [P5 Ln 110-112]. Large regional variations in the supply of GPs can be seen across many countries which has led to the introduction of policies and regulation aimed at discouraging GPs from locating in well-serviced areas [P1 Ln 55-58] suggesting that redistribution policies that are geographically bounded are an international phenomena. The result that strength of the association between geographically measured factors and GP supply depends on the level at which the variables are measured should be relevant to any country that organises its primary care system using geographical boundaries.