Author's response to reviews

Title: What is known about an association between rural medical education and rural practice location: a scoping review

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Version: 2 Date: 15 February 2015

Author's response to reviews: see over
Reviewer's recommendations

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<th>Reviewer 1</th>
<th>Reviewer 2</th>
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<td><strong>Main suggestion for improvement would be for the authors to propose a framework for research which would be more effective in answering the research questions than was found in the papers which were reviewed</strong></td>
<td><strong>Thankyou for the very positive comments</strong>&lt;br&gt;<strong>We have included a new section that outlines a framework for answering the research questions</strong></td>
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<td><strong>To describe or define what the authors mean by rural education and working in rural practice. In the results, through the typology, the authors describe the different types of rural education. They do not, however, define or describe what they mean by “working in rural practice”. This is particularly problematic given that they refer to employment following graduation - but it is unclear whether that mean graduation from medical school or from postgraduate training, or after bonded service. By including work location during residency training, the definition doesn’t generally cohere with studies that look at work location after all post-graduate training is complete (and the study does not seem to distinguish between these outcomes). Please provide definitions for the outcomes used in the study.</strong></td>
<td><strong>In the manuscript we referred to entry-level medical education to denote our focus on graduation from medical school. In the exclusion criteria for the study we state that we excluded postgraduate training. However, we acknowledge that the reader might need greater clarity so we have added the term pre-vocational to the abstract and introduction and stated clearly in the introductory paragraph that the focus of the review was on rural practice immediately following graduation. We have made it clear within the manuscript that a study exploring the impact of postgraduate study is different to our focus.</strong></td>
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<td><strong>There is a relevant review (Grobler, L., B.J. Marias, S.A. Mabunda, P.N. Marindi, H. Reuter and J. Volmink. 2009) and a related one Wilson, N.W., I.D.Couper, E. De Vries, S. Reid, T. Fish, B.J. Marais.2009. Please provide a rationale for a scoping review, given these systematic reviews</strong></td>
<td><strong>The purpose of the 2009 systematic review of Grobler et al. was to assess the effectiveness of interventions aimed at increasing the proportion of health professionals working in rural and other underserved areas. This study has a different purpose to our review as it focused on health professionals more broadly. Additionally, the results of their systematic review indicated no studies that met their inclusion criteria. The value of a scoping review is that all study designs are included, as the purpose is to map relevant literature, rather than exclude studies based on quality. The study by Wilson et al. published in 2009 was again focused on the broad health professional workforce. The purpose was to look critically at interventions to redress inequitable distribution of health care professionals. Their work included retention. Again articles were excluded based on quality criteria. Whilst both of these reviews are helpful to consider the broad workforce, our focus was much more refined and consistent with scoping review methods we wished to map all studies that related to our research question.</strong></td>
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More details are needed to replicate the work. It would be helpful to include the number of studies that met each of the inclusion and exclusion criteria so the reader can have a better understanding of article selection strategy.

I have some concerns about the articles that were included in the study. The authors cite one of my studies as an example of a medical education provided completely in a rural place (CMAJ, 2006 175:357-360). In the article, they state that it was included because the medical school is described as a "provincial medical" school (which I assume they interpreted as rural). In fact, the article describes the sole medical school in a Canadian province and it located in an urban city (albeit a relatively small one). We actually produced another article based on same study (Canadian Journal of Rural Medicine, 2008, 13(1):15-21) that shows up in google scholar and looked at rural work place locations that I would have thought met the study's inclusion/exclusion criteria. While I'm delighted that the authors included my research in their study, the fact that they didn't find the rurally focussed paper and chose the other paper makes me less confident in their search and selection strategies.

Thankyou for your useful comment regarding your own work. Mathews et al (2006) was included as the title and manuscript referred to the term 'provincial'. We debated the inclusion of this article when we initially completed the review. It was included in view of the description of provincial, and the size of the location of Memorial University. In the introduction we noted that we included the term provincial. However, your point is a good one and we have carefully reviewed all other studies to ensure that our inclusion/exclusion is consistent. In response, we have excluded this paper from our review.  As the Canadian Journal of Rural Medicine article is from the same study, it does not fit the inclusion criteria for the reasons outlined above.

One of the strongest confounders in the education/work place question is rural background (i.e. whether physicians/trainees came from a rural community). While the authors allude to the relationship between these variables (and discuss it the penultimate paragraph of the discussion), the analysis does not take into account studies that controlled for background and those that did not. Without controlling for rural background, it is difficult to agree with the general conclusion that "indications are positive" or that "there is some evidence to suggest a gradient effect" between rural education and rural practice.

We agree with your point about rural background being one of the strongest confounders. Consistent with scoping review methods, our purpose was to map the literature to identify any association between rural medical education and practice location immediately following graduation. We identified that there were a multitude of variables that impact on practice location. As stated, we did discuss the influential nature of rural background but also presented a counterfactual position. The tables that report the articles selected for our review include commentary on studies where rural background of students was reported. We believe that the reporting typology that we developed was the most useful to report a relatively large group of articles. As stated in our review, the reporting of many studies was
poor and there was variable information provided. We made the decision early in our review that attempting to divide studies according to ones that controlled for rural background and those that did not was difficult because of missing information. We have added a section on this issue to our new limitations section. We believe that our general conclusion that ‘indications are positive’ or that ‘there is some evidence to suggest a gradient effect’ between rural education and rural practice is supported by the articles that are reported in our tables. Many of the articles that did control for rural background found the strongest predictor for rural practise following graduation was locality of the medical school and rural exposure during medical education. We have added this point to the manuscript.

The typology seems to distinguish between interventions (first four categories) and research method (last category). The reason for this distinction is not readily evident since survey research could be used to assess the different types of medical education interventions. Please provide rationale for creating method and intervention specific categories.

Thank you for this very useful comment. It was not our intention to provide a research method category and the use of the term survey was incorrect in the title. We have retitled the last category to reflect the articles that are included.

The discussion and conclusions are well balanced but not adequately supported by the data. While the authors discuss many methodological issues (including definition of rural, confounding with rural background, small sample size, poor comparability between studies etc.) of the studies included in the review, these discussions seem to be separate from the discussion of the results of the individual studies (see comments above).

In writing up the findings of our review, we followed the method for scoping reviews described by Arksey and O’Malley. Their approach is described in the methods section. Consistent with this approach, the major study findings are charted in the tables with the discussion, a collation and summary of the literature. As the other reviewers commented on the quality of the overall article, and we have followed a well-established method we believe that our approach is appropriate.

While I agree with their final conclusion that cost-effectiveness analyses of medical education interventions are needed, there’s nothing in the study itself to support this claim (for example, they didn’t look for economic studies). Economic evaluations (or other related terms) were not included in their search terms.

In our discussion section we note that we found no studies considering the cost effectiveness of different models of producing doctors for rural places. We have included a section in the findings indicating that we specifically read each reference to assess whether they reported on cost effectiveness. In the limitations section, we have added the point that cost effectiveness was not the primary purpose of our review and whilst we read all articles carefully for reporting of costs effectiveness none were found. In our new recommendations for further research section we identify economic analysis as an important future area of study.

For title, I suggest “The impact of rural medical education on rural practice location: a scoping review”. While the abstract alludes to the typology, it is not presented the different categories. To me the conclusion doesn’t really

The title has been changed to “What is known about an association between rural medical education and rural practice location: a scoping review” reflecting the purpose of the study.
In terms of writing, it would be helpful to describe the terms used in Australia to describe the various stages of physician training so that non-Australian readers can identify equivalents in their own system (and better contextualize the results).

Reviewer 3

I did wonder if the authors have chosen the right research question, and feel they should discuss this in some more detail. I have a couple of comments on the title, ‘Rural medical education and the impact on rural practice location’. Firstly, is the choice of terminology – ‘impact’. My concern relates to the causation argument and the effect of confounding factors and prior intent, as the authors do indeed acknowledge. Some papers do claim (or imply) causation; others describe ‘associations’ and I note that the word ‘produce’ is used in the 2nd para of the Discussion and Conclusions. It seems to me that ‘impact’ is somewhere in between – but it is still hard to claim impact without further data, perhaps via triangulation from interviews etc. I would therefore suggest the authors discuss their perspective on this point and how it relates to the findings in the literature (or, alternatively, synthesize the literature and provide some commentary on this point).

We agree with your comments and thank you for your insight. We have removed the term ‘impact’ and changed the title to now read: “What is known about any association between rural medical education and rural practice location: a scoping review”. We agree with the reviewer’s comments and have added a section to our discussion and limitations noting this point.

Secondly, I think the authors need to consider (and provide some discussion/justification) the issue of whether the effect of a rural placement can be separated from the other variables mentioned. The rural ‘pipeline’ is well described, and I suspect that it is a combination of many factors that contribute to graduate outcomes. For example, the program I am involved in (which provides some of the data cited) would argue that it is a combination of recruitment, selection, curricular time, placements, role models, regional location, contextual teaching and postgraduate pathways, among other things, that contribute to where graduates practise (along with other factors out of our control). In other words, any effect is due to much more than the rural placements alone, or, put another way, a program without any of these factors in place that introduces rural placements of one sort or another may find their outcomes are quite different.

This problem is similar to that described in the BEME guide to staff development (No 9), where the authors note: ‘Overall satisfaction with...’

We agree with the reviewer comments and they link well with the helpful views of reviewer two on the same point. We have introduced an expanded limitations/recommendation section, where the difficulties of separating variables are discussed. The need to develop well-designed, rigorous studies that account for different variables and extrapolate the impact of each on rural practice outcomes is identified in our new recommendations for research section.

As above. We agree and have made stronger commentary on this issue.
faculty development programs was high. Participants consistently found programs acceptable, useful and relevant to their objectives’ but later describe that high satisfaction is not surprising given the programs were devised for a specific purpose or need i.e. in both cases a program designed to address a particular issue in a particular context may not work elsewhere. I note the ‘counterfactual’ argument put in the discussion and agree that the observation that rural schools are providing opportunities that might not otherwise be taken up has considerable merit. I still cannot escape the conclusion that this is one factor among many, and that ‘rural medical education’ cannot be easily separated from the many other factors discussed.

Table 3 appears to be missing in the MS I reviewed, and the other Tables seem to have been duplicated

‘PRACTISE’ is usually spelt this when used as a verb – this may need correction in a number of places, or referral to the journal’s style manual.

The authors mention some weaknesses and the need for ‘future high quality research’. They could perhaps strengthen the conclusion by making some specific recommendations on how these might be overcome. Issues like small sample size are significant: many smaller trials have demonstrated proportions in the desired direction – but the absolute numbers are what will make a difference to workforce, which begs the question whether there is capacity to train the numbers needed. While definitions of rurality vary across jurisdictions, much of this is about ‘non-metropolitan’ training (outside of RA-1 in Australia) – often called ‘regional’. Across jurisdictions there is likely to be at least some agreement across the metropolitan – non-metropolitan divide, or the metropolitan – regional – rural – remote spectrum. Finally, the lack of comparative data may be an issue in many countries without access to something like the MSOD database, and the point about self-reporting is well made, and perhaps should be augmented by suggestions like inviting external scrutiny/audit of data, or authors and/or collaborations from multiple institutions.

Thank your for your helpful suggestions, points which we indeed have discussed. We have included expanded recommendations for further research section that has covered these points and others.

The paragraph under the heading ‘New rural medical schools’ could be read as suggesting that the Flinders NTMP is in Queensland, which is not the case.

Finally, the lack of page numbers in the MS made the reviewing process a little
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<td>Please include an acknowledgement section at the end of the manuscript before the reference list. Please acknowledge anyone who contributed towards the study by making substantial contributions to conception, design, acquisition of data, or analysis and interpretation of data, or who was involved in drafting the manuscript or revising it critically for important intellectual content, but who does not meet the criteria for authorship. Please also include the source(s) of funding for all authors. Authors should obtain permission to acknowledge from all those mentioned in the Acknowledgements.</td>
<td>We have included a section indicating that no funding was obtained for this review and that all contributors are authors.</td>
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<td>Please remove the Figure legends in the figure files. They should be included after the References list.</td>
<td>We are not sure what was meant here as the figure legend was not part of the figure file but we have included it at the end of the reference list.</td>
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