Author's response to reviews

Title: The Effects of Residency Training on Patient Outcomes: a Systematic Review

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Author's response to reviews: see over
Dear Claire Tree-Booker, Editor,

Thank you for the opportunity to revise our manuscript entitled “The Effects of Residency Training on Patient Outcomes: a Systematic Review” for publication in BMC Medicine. We are grateful to The Journal and the reviewers for the insightful remarks. We are pleased with the resulting improvements, and we hope you will be too. We herein share our point-by-point revisions in response to the reviews.

Reviewer #1

Discretionary revision
I think therefore the paper could be improved further if the authors would be a bit more explicit about the different questions they sought to answer or at least have answered with their categorisation of papers. Perhaps a reorganisation of the paragraphs according to these questions would not only make the messages clearer but would also allow the authors to comment or annotate their findings per ‘question’ to help the reader better to understand the large number of discretely different outcomes of the individual studies.

We thank the reviewer for the suggestion. We discussed the organization and categorization issues with different medical and research colleagues to improve clarification and readability. Because our search strategy was explicitly designed to find patient outcomes of residency training – while residents were in training and as a result of training, we have re-organized the presentation of our results. The categorization of our results now mirrors the natural course of residency training: the start of training, individual progress, development through residency years, and in comparison to faculty.

Minor essential revision
One final minor point is the statement “This is surprising given that quality and performance initiative espouse life-long learning…” needs either referencing to empirical literature (which I doubt there is) or may best be rephrased into “.given that there is shared belief that quality…”

We agree and have rephrased the statement accordingly.

Reviewer #2

Discretionary Revisions
Methods /Data sources & searches/ end para 2: ‘compete search strategies are available on request’. For an on-line journal, perhaps these could be made available on-line.

Minor Essential Revisions
Background/mid para 2 typo: GME typically focuses on educational outcomes and DOES not include patient outcomes.
We thank the reviewer for the careful reading. We have now corrected the typo.

**Methods/Study selection/end para 2:** The authors’ meaning by ‘non-targeted participants’ (i.e. medical students or nursing home residents) is unclear.

We have now rephrased ‘non-targeted participants’.

**Results/Categorization of studies and subsequent Results sections:** The paper is inconsistent in its hierarchy of description for groups of papers. The sections on ‘10 studies’ and ‘17 studies’ both refer to the second ‘subcategory’. The sections on ‘19 studies’ and ‘7 studies’ both refer to the third ‘subcategory’. This could be corrected by referring to the 79 and 17 groups as ‘categories’ and the 43, 10, 19, and 7 groups as ‘subcategories’ and ensuring that the appropriate descriptor ‘first’, ‘second’ etc is used. A minor point, but important to follow the organization of results. Figure 1 is very clear.

We agree and have taken up this point together with the suggestions by reviewer#1 in order to present a more consistent hierarchy of our results.

**Discussion/Explanation of results/para 1:** ‘This makes it much more difficult to assess the educational effect of residents’ care on patient outcomes’. I suspect this intends to say ‘This makes it much more difficult to assess the effect of residents’ education on patient outcomes’? It is not a logical statement as it stands.

We have revised the sentence accordingly.

**Major Compulsory Revisions**

**Methods/Data sources & searches/para 2:** the authors definition of GME does not include training for family practice. Was such GME excluded? I suspect not, as there is mention of such training later in the paper. This needs clarification.

We thank the reviewer for this observation. We have now clarified our use and definition to include ‘general practitioner or family physician’.

**Results/17 studies evaluated the effect of specific training situations/final sentence:** how was the assertion that studies on ‘specific training situations’ yielded equal or improved outcomes etc.’ reached?

We have rephrased the last sentence of this paragraph to clarify our conclusion. The text now states: ‘In conclusion, the seventeen studies in the category of ‘specific training interventions’ showed improved patient outcomes in eleven studies and a drop in patient satisfaction in one study.’

The paper’s overall conclusion is that ‘patient care is safe and of equal quality when delivered by residents’. Yet the results as presented do not support this degree of absolute statement. Across the reported results, a significant minority of papers showed that residents, or subgroups of them, delivered worse care. The authors report this to be the case for 8 out of 43 studies; 6 out of 10 studies; 6 out of 19 studies; 6 out of 7 studies; 1 out of 17 studies (these studies relating to comparisons of training situations); one study of post residency outcomes showed that programmes could be ranked. This data collectively suggests that there is considerable evidence that outcomes may be worse following treatment by some categories of residents in comparison with faculty/other residents/themselves at a later point in training. Given that new doctors have to learn, it would seem more accurate to acknowledge this strand of data and discuss the practical significance of the differences observed and their acceptability to
patients and training organisations. If the authors feel that the above is not a fair interpretation of their results, there is a need robustly to justify that position from the data as presented in the paper. Otherwise, this requires a significant review of the discussion, conclusions and abstract.

We appreciate the reviewer’s interpretation of our results. We have put great effort into the acknowledgement of the differences in outcomes found in different studies of our paper. We have now provided a nuanced and balanced interpretation of the different results, leading to the substantial improvements in our manuscript.

Discussion/second sentence: This is the first mention that the review was aimed to gather evidence that ‘better training’ will enable residents to become ‘better doctors’. This statement fits neither with the previously stated aim of the review nor the literature selection strategy. I would suggest consistency here with the previously stated aim.

We have deleted the statement to avoid confusion about the aim of our review.

Discussion/ Explanation of result/ para 1: ‘From a cost perspective…needed to ensure (a) future health care workforce and ‘Exploring differences between teaching hospitals and non-teaching hospitals’. These points are discussing separate issues from those addressed by the review and do not therefore place the results in context (implications for research and practice are discussed in a later section)

Thank you for the observation. We have moved the text to the implications for research and practice.

Discussion/ Explanation of result/ para 2: ‘We found little evidence for the July effect…’ Please see comments above. A minority of studies did identify this effect. This needs greater acknowledgement and discussion.

Throughout the paper, we now place a stronger emphasis on this minority of studies in order to acknowledge and balance the varied results.

The title was, for me, a UK reader, misleading. I expected the paper to relate to outcomes following initial medical training via the Graduate entry route (4 year graduate entry courses) rather than following initial postgraduate medical education. I note that MeSH refer to ‘Graduate medical education’ as do the authors, rather than ‘postgraduate medical education’. The abstract refers to ‘residency training’. Perhaps this term might be used in the title. The authors could either use ‘residency training’ throughout the paper in place of ‘GME’, or substitute ‘GME’ early in the paper, with explanation.

In order to prevent any confusion we have used ‘residency training’ throughout the paper as well as in the title.

All aforementioned revisions were implemented in the manuscript. All authors have now approved the revisions.

We are looking forward to your thoughtful consideration and hopefully favorable decision to publish the revised and formatted manuscript.

Yours sincerely,
Renée van der Leeuw, MD
On behalf of all authors