Author's response to reviews

Title: Compliance with clinical guidelines for whiplash improved with a targeted implementation strategy. A prospective cohort study.

Authors:

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Author's response to reviews: see over
Dear Armee Valencia

Thank-you for the opportunity to resubmit the above manuscript to BMC Health Services Research. We have made substantial revisions to the manuscript based on your and each of the reviewers comments and feel that as a result the manuscript is considerably improved. Please find a point-by-point response to the concerns raised by the reviewers and yourself. The corresponding changes to the manuscript appear in red text for ease of identification. We look forward to your further consideration of this manuscript.

Kind Regards

Trudy Rebbeck
Corresponding author
Reviewer 1

1.1 Based on the hypothesis I would expect that the target group would only be non-compliant professionals. However, it seems that due to the low response rate of potential participants via records of the MAA, additional recruitment strategies were initiated resulting in a sample of both compliant and non-compliant professionals. Can the authors clarify this?

Author response
We have now clarified in the Methods (page 7, paragraph 1 and page 8) that our intent was to ensure that a proportion of the participants were non-compliant at baseline, as our previous experience was that participants recruited through advertising were largely compliant at baseline. Hence prior to the beginning of the study we set up a recruitment strategy to ensure this.

1.2 Non-compliance is defined in the introduction by lack of knowledge and adequate practice. However, non-compliance was measured via record of clinical practice and not based on lack of knowledge.

Author response
The reviewer is correct in these statements. We have clarified in the introduction that compliance has been measured by both professional knowledge and practice (page 4, paragraph 2). We have substantially revised the methods to elaborate more on how the implementation strategy was developed in response to this and other comments. Amongst the additional information is a sentence clarifying that the authors and the working party developing the strategy decided what their definition of compliance would be for this study. Hence in now reads:

‘The group defined compliance with guidelines for this study using a standard record of clinical practice. This was believed by the authors to be the measure that would most accurately reflect actual clinical practice. The record of clinical practice is a standard form used to report assessment findings and treatment choices to compulsory third party insurers in Australia. Non-compliance with clinical guidelines was considered as failure to adhere to at least three of the four key messages outlined above.’

1.3 Was the analysis of record of practice for identifying non-compliant professionals via MAA used as baseline measurement, or were separate records sent to the researchers by participants themselves? If the latter: were the two records (used for recruitment and sent by participants) concordant for non-compliance? And how was baseline measurement conducted for participants as recruited via advertisement?

Author response
Separate records were provided to the researchers by the participants themselves. This is now clarified in the methods (page 9, paragraph 1) which now reads:

‘Professional practice/ compliance: Professional practice and compliance with clinical guidelines was evaluated by responses to a recent record of clinical practice. Each participant submitted a recent (within the past month) record of clinical practice for a patient with whiplash assessed by them. Responses recorded on the recent clinical record were then assessed independently by two authors (TR and LM) and categorized as compliant or non-compliant with guideline recommendations against each of the four criteria (Table 1). A participant was classified at baseline as overall compliant with clinical guidelines if their answers were consistent with at least three of the criteria. Differences were resolved by consensus between the 2 authors. This method ensured baseline compliance was assessed consistently.

Hence baseline compliance was measured by the same method for all participants regardless of recruitment method. As stated in points 1.1 and 1.2 above, we provided substantially more information in the methods to clarify that the insurers perusal of clinical records was a recruitment strategy rather than an assessment of compliance. This is in part is because several different insurance personnel reviewed past clinical records in order to provide this information to the MAA. As the clinical record is the property of the insurer, and the authors were blinded to how participants were recruited, we had no way of controlling for the consistency of how records were assessed by insurers.

1.4 The authors describe in the discussion that their hypothesis was that greatest improvement was expected for low baseline knowledge and being physiotherapist. In addition to the abovementioned issue that knowledge and practice were measured separately, the authors did not include professional background in their hypothesis as described in the introduction.

Author response
Thank-you for alerting us to this oversight. We have now clarified (Introduction: page 4, paragraph 1) that professional background should also be a factor to consider as an effect modifier.

1.5 The authors argue that the identification of eligible professionals for education can be done via insurers. However, only non-compliant practitioners can be identified by the insurer via the record of practice which does not necessarily indicate low knowledge.

Author response
The reviewer is correct. We have now clarified this is in the discussion, with substantially revised paragraphs. See paragraphs 2 and 3, pages 14 and 15.
1.6 The hypothesis that non-compliance would be main predictor for the impact of the education is not confirmed in the univariate and multivariate regression models. Please elaborate on this issue in the discussion.

1.7 Please elaborate on the recruitment and non-compliance issues in the discussion.

Author response: 1.6 & 1.7
We thank-you for alerting this to us, and discussed this at length as to why this may have occurred. As a result, we added a paragraph in the discussion to elaborate on why baseline compliance was not a predictor of outcome in this paper. We have also clarified the effect of the recruitment strategy this paragraph (paragraph 2 page 15). The reviewer is correct in that this was not discussed previously.

1.8 Why were only 3 key messages used in the confidence questionnaire (knowledge related to use valid measures of outcome is not used)?

Author response
This was a pragmatic decision. To clarify this, the following sentence has been added to the methods (page 10, first paragraph):

‘The confidence measure had been found to be related to compliance amongst general practitioners, and was being used in a concurrent study [19]). We were interested to see if this would also be a factor with other health professionals.’

1.9 Was the detailed analysis of the prognostic factors based on open ended documentation of the records of practice or were the features pre-classified. In addition, were the VAS and NDI used in the further analysis?

Author response
We assume the reviewer is referring to the results (page 13 and Table 4). To clarify this we have added the following to this section:

‘Therefore, in order to further examine whether practitioners did improve their knowledge on the identification of adverse prognostic factors, we further analyzed an open ended question on the knowledge questionnaire, which asks “What features in a whiplash patient signal to you that a patient may not recover”. Health professionals would nominate factors they thought related to non-recovery and the researchers would then classify the responses into the categories nominated in the guidelines’

In regards to the VAS and NDI, yes they were used in this further analysis but evaluated by construct rather than instrument measure. For example the VAS is an instrument to measure the construct ‘initial pain intensity’, whilst the NDI is an instrument to measure the construct ‘initial disability’. As our guideline
recommendations do not restrict measurement of these constructs to a single instrument (eg initial disability could also be measured by other disability instruments such as the Functional Rating Index), we classified the prognostic factors by construct. To clarify this we have added that the VAS and NDI are examples of instruments to measure this construct in the footnote of Table 4.

1.10 Define ‘these’ health professional groups in the introduction at the bottom of page 4.

Author response
This is done

1.11 The ‘recently’ published RCT from the authors of the study (introduction, page 5) was in fact seven years ago.

Author response
You are correct, and the word recent has been deleted.

1.12 What are ‘education venues’ (methods, page 6)?

Author response
This has been clarified: conference rooms

1.13 Change p values ‘greater’ than 0.25 into ‘smaller’ than 0.25 (methods, page 10)

Author response
This is done
Reviewer 2

2.1 One of the aims of this study is to develop a target implementation. However nothing is said about development in the results. As it seems the implementation was constructed as workshops before the study started.

Author response
The reviewer is correct with this comment. As a result we have elaborated on the development of the implementation strategy over 2 paragraphs on pages 5 and 6 of the methods, as the development of the implementation strategy was part of the wider programme of research. We have clarified in the aims however, that we aimed to ‘describe and evaluate’ the strategy in this paper. Hence we have deleted the word ‘develop’ from the aim to make it clearer. It now reads:

‘The primary aim of this study was therefore, to describe and evaluate the effect of a more targeted implementation strategy on the knowledge, practice and beliefs of allied health professionals managing patients with whiplash. In addition this study aimed to identify factors that predicted learning in relation to clinical guidelines for whiplash’

2.2 Additionally the authors’ definition of the word target in the present study is unclear. If it meant that the implementation was directed to those professionals who were non-compliant this was not the case in the study since it also included compliant professionals. The other aim – to describe the factors that predicted improvement is well described in the result section.

Author response
Thank-you for pointing out this lack of clarity. We have substantially revised the method section in relation to this comment and those of reviewer 1. The changes appear in red text in the revised manuscript (pages 5 -7).

2.3 The paper mostly seems to focus on the effects of the implementation and this ought to be included in the aim. This is also stated in the method section where it is described as the main outcome of the study.

Author response
Agreed, as per point 1 we have revised the aim of the study to reflect the methods and results and main outcome of the study.

2.4 In the method section it is also stated that the aim was to include professionals whose practices were non-compliant with the guidelines, but half of the study
population were compliant with the guidelines. If both groups are kept there is a need to revise the text in the method section according to this.

**Author response**
In relation to this and the comments from Reviewer 1, we have substantially revised the text in the methods (page 7) to be clear about the recruitment strategy. As a result we trust it is now clear that that our recruitment strategy was chosen to ensure that non-compliant professionals were **included** as participants. In our previous study, nearly all participants were compliant at baseline, hence not leaving any room for effect. We were therefore not aiming for all participants to be non-compliant.

**2.5 The first tables separate the groups but the analyses are made of the whole group. If the author’s keep both groups it would be of interest to see if the factors for improvement differed in the two groups.**

**Author response**
Our chosen method to determine the effect of compliance on learning was to conduct a regression analysis, so that we could explore the effect of baseline compliance on leaning as well as other potential effect modifiers. Given that our measure of compliance was not found a predictor of leaning, then it would mean the factors would not be different between the groups. We have added a paragraph (page 14 and 15) in the discussion to explain why our compliance measure may not have been predictive.

**2.6 Page 1, third paragraph. Amongst “these health professional group” needs to be explained. Probably it refers to physiotherapists, chiropractors and osteopaths, or does it refer to other allied health professionals too?**

**Author response**
This has been changed to be clearer

**Associate Editor’s comments**

**3.1 The study is of interest and contributes to the knowledge on suitable implementation strategies. The authors should address the issues raised by the reviewers. The aim of the study should be extended with the effect of the implementation. Information is needed on the development of the implementation strategy (what were the targets for change?) as well as the role of the (non-) compliants in the study. The discussion can be elaborated a bit further on how well the implementation strategy worked in the light of its development.**
Author response
Thank-you for your comments. As indicated in our responses to the reviewers we have substantially elaborated on the development of the implementation strategy and the recruitment strategy to ensure non-complaints were included. We have elaborated on this in the discussion also.

3.2 Please make the following formatting changes during revision of your manuscript. Ensuring that the manuscript meets the journals manuscript structure will help to speed the production process if your manuscript is accepted for publication.

*Copyediting:
After reading through your manuscript, we feel that the quality of written English needs to be improved before the manuscript can be considered further.

We would be grateful if you could address the comments in a revised manuscript and provide a cover letter giving a point-by-point response to the concerns.

Author response: We have formatted the manuscript to be consistent with the journals structure and addressed the quality of the written English where indicated.

Thank-you once again for the opportunity of resubmitting this paper.

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