Author's response to reviews

Title: Prognosis of Patients with Whiplash-associated disorders consulting Physiotherapy: Development of a predictive model for recovery

Authors:

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Author's response to reviews: see over
Dear Editor-in-Chief,

Thank you for the revision on our manuscript “Prognosis of Patients with Whiplash-associated disorders consulting Physiotherapy: Development of a predictive model for recovery” by Tony Bohman, Pierre Côté, Eleanor Boyle, J. David Cassidy, Linda J. Carroll and Eva Skillgate for publication in BMC Musculoskeletal Disorders.

The reviewer’s comments have been very helpful to revise and improve the manuscript. We have considered their comments and hopefully made necessary changes and clarifications. All changes are highlighted with red font in the revised manuscript.

Reviewer: Michael Schneider

Major Compulsory Revisions:
NONE. Overall, this is an extremely well done study of the highest quality.

Minor Essential Revisions:
Comment 1: These data were gathered over the two year period 1998-1999, which is 13 years ago. The authors should give some explanation as to why they are just now publishing the results.

Response: The data used in this analysis comes from a very large population-based cohort study of individuals injured in traffic collisions. The study was originally designed to assess the effectiveness of rehabilitation for whiplash injuries. However, the richness of the data offers the ability to answer many new and relevant research questions. In fact, the data from the SGI cohort has been used in several important publications with the first paper published in 2006. Some of these publications has been included as references (page 6, Methods; paragraph 1, line 3).

Comment 2: The primary outcome measure was a single question upon which recovery was defined. Was this the sole basis of the telephone interviews at 6 weeks, 3 and 6 months? Was any other information gathered during these follow-up phone interviews?

Response: We used time to recovery as our outcome. However, the follow-up interviews included several questions. The follow-up interviews provided information on pain location and intensity, disability, health-related quality of life, exercise, activity limitation, health care provision, depressive symptoms and work status. This information has been added to the manuscript (page 9, Outcome; line 4-7).

Comment 3: There is brief mention of a baseline questionnaire that is part of the SGI form that must be completed by patients to report their injury. However, we do not have any specifics about how many or what type of items are on that questionnaire...unless Table 1 reflects those items. This is unclear.

Response: We have revised the manuscript and this information is presented on page 7. (Data collection; line 3-6).

Comment 4: Also, the authors state they selected these potential prognostic factors based on the literature and clinical experience. Were there any other potential factors that were not available from the SGI form that could still be important potential prognostic factors?
Examples such as: Body mass index, physical activity level, type of physiotherapy care received, etc. Are there any other limitations to gathering data from a form over which the research team has no ability to modify?

**Response:** This is an important issue. Unfortunately, we were limited by the use of secondary data. We have already mentioned the lack of information regarding the specific clinical interventions provided to participants. We have added a discussion of the lack of information on life-style factors (page 16, paragraph 1). We did not include BMI because it has been shown not to be a predictor of recovery in whiplash patients.[1] Overall we believe that we have information on the majority of possible prognostic factors for patients with WAD. The majority of the items are also valid and reliable.

**Comment 5:** The CONSORT study flow diagram shows that a total of 7524 patients reported neck/shoulder pain as a result of their collision, yet only 901 (12%) consulted physiotherapists. Where did the vast majority (88%) of these other patients go for treatment?

**Response:** The majority of patients with WAD did not consult a PT. Most of those not included in the study population visited a medical doctor (about 60%). A smaller proportion visited doctors of chiropractic (about 20%) and some visited a combination of these professionals and/or massage therapists. We did not look at the distribution of other healthcare providers consulted by the study participants because it was not our objective. In Saskatchewan, patients injured in traffic collisions primarily consult medical doctors, physiotherapists and chiropractors.

**Comment 6:** Are the predictive factors of recovery in the 12% of WAD who seek PT services different from those who seek other medical care? This low number of patients treated by PT and these questions above are worthy of some discussion.

**Response:** This is a very good question and it deserves to be investigated. However, it is beyond the scope of this paper. To address this issue, we have included a statement in the background (page 6, second paragraph, line 3 to 6) and also discussed the issue at page 15, paragraph 4. We simply do not know why only 12% of Saskatchewan residents with WAD choose to consult PT, but we are confident that the reported proportion is valid since it is comparable to what was reported by Côté et.al.[2]

**Discretionary Revisions**

**Comment 1:** The authors mention that this study can help physiotherapists to improve the treatment of patient and help manage their expectations. However, there is no discussion about how therapists would specifically modify their treatment approach/methods to improve patient outcomes based upon these results. For example, are the authors recommending that therapists use the principles of cognitive behavioral therapy to attenuate catastrophizing or fear avoidance behaviors in patients with a good prognosis? Should therapists take a more aggressive approach to physical exercise and conditioning in these patients? Some discussion about the practical application of these results for practising therapists would be useful.

**Response:** This is an interesting issue, but it is beyond the scope of our paper. As this is the first (derivation) step of constructing a prediction rule we believe that it is too early to give
any recommendations on how this model could guide the treatment for a therapist. The model has to be validated further. Nevertheless, based on this comment of yours, we have changed the wording on page 6, paragraph 2, line 3 from “improve the treatment” to “improve the care” to reduce the focus on treatment.

Reviewer: Howard Vernon

Major Compulsory Revisions:

Comment 1: The authors have not provided a sufficient and convincing justification for why the findings of this study are relevant only to physical therapists (see below). There is no problem with obtaining data from a single practitioner group and identifying that process in the Methods. However, it is, in my opinion, no justifiable to then limit the implications of the findings to only one practitioner group. Please address the points itemized in Minor Essential Revisions.

Response: All patients included in our analysis consulted a PT and a medical doctor, therefore we agree that our results apply to patients who consulted a PT and a medical doctor. We agree that this was not clear in our manuscript and have revised our manuscript accordingly. In the background section of the manuscript, we now explain why a prediction tool should be used in a population similar to the one used to develop the tool (page 6, second paragraph, line 3 to 6). Furthermore, we discuss that the model may also be used by medical doctors (page 15, paragraph 4). However, we do not agree that our results can be generalized to other health care specialties because each specialty treats different subgroup of the population.

When it comes to patients consulting other health care providers as e.g. doctors of chiropractic and massage therapists we cannot exclude that these patients may have different characteristics than patients included in our study and therefore the predictive factors may differ. Beattie and Nelson conclude that one “should ensure that patients and clinicians for whom the prediction rule is to be used share the similar traits to those used for validation”. [3] Furthermore, studying neck and low back pain patients Côté et.al. found differences in characteristics between patients consulting chiropractors and medical doctors. [2] We think these finding supports our beliefs that we should be careful to transfer our result to other practitioners than PT and MD. The above mentioned references are included in the revised manuscript (background and discussion). [2, 3]

Minor Essential Revisions:

Comment 1: In the Abstract, line 3, the authors specify "physical therapists" for interest in their study. This is inappropriate. I would suggest a general category such as "spine pain practitioners".

Response: We prefer to keep the focus on PT but have changed the wordings to; “…challenging for health care providers such as physical therapists.”

Comment 2: Same for Introduction: "It is important for physical therapists to predict which patients with WAD are more likely to..."
Response: We changed the sentence to; “It is important for health care providers such as physical therapists to predict which patients with WAD are more likely to….”

Comment 3: Same for Methods: "medical history improve the ability of physical therapists to predict recovery." (This study is not just for physical therapist to use!) Same for Discussion: "Information about prognostic factors incorporated in the model is easily gathered in the medical history taken by a physical therapist" Same for Conclusion: "Our model can guide physical therapists to assess medical history information that are important for predicting recovery". This sentence is particularly problematic: "Patients with WAD frequently seek physical therapists and this model may be an important tool to help physical therapists in their management of these patients."

Response: We fully agree that patients with WAD have the option to seek care from a variety of health care disciplines. However, our primary aim was to study patients consulting PT. Therefore, we prefer to keep the sentences mentioned above as they were submitted as they are specific to our research question.

Comment 4: What does the fact that these subjects consulted a PT have to do with the model other than the selection bias imposed by the authors? These changes are further justified because:
a. it is reported that all subjects in used in this study also consulted an MD. and,
b. the authors indicate that "We used the data collected in the baseline questionnaire as a proxy for medical history collected by a physical therapist. It is possible that patients would answer differently when consulting a physical therapist." Therefore, the data was not obtained through any direct interaction with a PT anyway! If the authors do not agree with this logic, then they should provide a justification in the Introduction and then in the Discussion as to why it is important to report this model as only useful for PT's.

Response: Our objective was to build a prediction model for patients who consult a PT. Therefore, we restricted our sample to those who reported to have sought care from a PT. We do not believe that restriction introduce selection bias. We do not understand why Dr. Vernon suggests that clearly defining our sample created a selection bias. As mentioned above (Comment 1) we have included a justification in the background and discussion.

Comment 5: Introduction:
2. Based on the authors' own text: "Although the prognosis of WAD is favourable, previous studies have found as much as 50% of the affected individuals to be symptomatic one year after the injury.[5, 9]", I would suggest amending this to "generally favourable" (in the Abstract as well).

Response: We agree and have followed the suggestion. The text was edited accordingly.

Comment 6: Methods:
It is a little confusing to read about the large population cohort for SGI which the authors describe as "we created", and then read "included in our study cohort were patients...who consulted a PT..." It seems to me that you want to indicate that the study sample was selected from a previously formed (and reported on) cohort for the SGI. Then, specific inclusion criteria for that selection should be listed.
Response: We have made changes on page 6 (Design and study population) and hope that will clarify how the SGI cohort and our cohort were established. We made the following changes:
- “In this study we used data from the Saskatchewan Government Insurance (SGI) study, a population-based inception cohort study of 8634....”.
- “Eligible participants for the SGI study were....”.
- “The sample used in this analysis is a sub-cohort of the SGI study. The sample includes patients with WAD ...”.

Comment 7: In Data Collection, please correct the word "rational" to "rationale".

Response: We have made the change on page 7.

Thank you for considering our revised manuscript for publication in BMC Musculoskeletal Disorders.

Sincerely

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