Reviewer’s report

Title: The clinical course over the first year of Whiplash Associated Disorders (WAD): pain-related disability predicts outcome in a mildly affected sample

Version: 1 Date: 30 July 2013

Reviewer: David Walton

Reviewer’s report:

Major compulsory revisions.

Methods:

1. ‘At this point in time no particular approval was needed for clinical protocols not including any intervention…’ I’ll admit I find that hard to believe, in that any protocol that involves collecting, analyzing, and reporting data on humans should require consent of the participants. I’m going to ask the authors to clarify this statement.

2. I’d like the authors to present (in their response to reviewers, not necessarily in the paper) their sample size calculation. 98 subjects for a 6-predictor regression with expected medium effect is high by most standards with which I’m aware. I’m fine of course if the sample is larger than required, better than being too small, but something doesn’t quite fit for me with this calculated number, so I’d like to make sure it’s being reported correctly.

3. Pain intensity: I agree that the measurement properties of a 0-10 NRS are generally reported as acceptable, but the ‘average over the past 2 weeks’ window is large and is not the common approach to assessing pain intensity. I would like to hear more about the properties of the NRS when used over this large a window.

Data management:

1. It appears as though questionnaires were retained if up to 25% of items were missing. Some of the scales used do have specific instructions for dealing with missing items, and the maximum number that can be imputed. At the least I would expect some discussion in the limitations section, or at least some defense for imputing this many items.

I’m finding the description of the analyses difficult to follow:

2. Why was a cut-off point chosen for deterioration? I’m not seeing where that was used.

3. Also, the authors report the use of Spearman’s rho (incorrectly described as Spearman’s r), which would indicate non-linear or non-parametric variables. But then in the next sentence the authors state that core assumptions of linearity were checked, but don’t describe how that was done.

4. In the next sentence, results of the multicollinearity analysis are presented, which should be reserved for the results section.
Further down, a statement is made ‘...consequently limiting the report of the backward selection method in the results section.’ which is unclear – what was limited?

Results:

1. The sample size numbers aren’t adding up. On page 5 the authors state that 40 of 145 eligible subjects didn’t return baseline forms, leaving a sample of 105. Then under the results there are 98 subjects indicated, with 73 included in the regression. I’m unclear as to how we got from 105 to 98 (>25% missing data perhaps?). And, at 73 subjects for regression, this falls short of your estimated sample size requirement of 98. More clarification is needed here.

2. Para 2: how were pre-injury health status and physical activity captured? I don’t see that in the methods section, and is notoriously difficult to validly measure.

Discussion:

1. The authors have repeatedly referred to a ‘prediction model’ throughout this paper, and an attempt to generalize it in an independent cohort. But I have seen no description of an actual model, in fact what I’ve read so far has been largely another exploratory study using linear regression, which doesn’t really add anything to the existing literature. What I’m able to get from this paper is that people who self-identify as being only mildly affected by WAD do indeed rate lower on most self-report scales measuring the magnitude of distress, which is largely intuitive. The association between baseline disability and follow-up disability has been well-established by now and is not novel. The only somewhat interesting aspect here is that fear of movement has been reported to increase (worsen) over the first year despite every other indicator of recovery showing improvement, but this finding is not discussed. Normally I would make suggestions regarding ways to re-focus a paper like this, but in this case I’m struggling to do so. I’m not sure what more I can offer to the authors, I’m sorry.

Minor Discretionary revisions

Introduction:

2. First sentence needs to be re-worded

3. Paragraph 2: ‘bad recovery’ – let’s find a different word to use here: prolonged disability maybe?

4. Para 3: ‘a common clinical apprehension’ – to what, by whom?

5. The MIAS and MOSAS acronyms are difficult to keep straight. Could you call them something more descriptive, like ‘MILD’ and ‘MOD-SEV’, or something along those lines?

Selection:

1. Who performed the physical examination to determine WAD grade? How many different examiners were there?

Variables and measures:

2. PDI: the sentence ‘A Swedish version of the PDI was used’ is redundant with
the first sentence of this section and can be removed, moving citation 17 to the first sentence.

**Level of interest:** An article of insufficient interest to warrant publication in a scientific/medical journal

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.