Reviewer’s report

Title: Comorbidities, Pain aspects and Implications of Pain in Local, Regional, and Widespread Pain - a Descriptive Population-based Survey

Version: 2
Date: 6 May 2015

Reviewer: Andrew Kittelson

Reviewer’s report:

I am glad to see this paper again. Epidemiological examinations such as this are valuable to our understanding of pain in society. My main concerns at this point have to do with a few of the statements made and the clarity in certain places. Here are my suggestions:

Minor Essential Revisions:

The introduction is a bit disorganized. It seems the authors are trying to suggest some problems with our understanding of pain in society: 1) we rarely assess pain in society--our understanding is mostly limited to clinical populations, 2) we define pain in a variety of different ways and it’s not clear if our definitions are really relevant to the clinical picture, and (perhaps) 3) because of the complexity of pain, the interaction of pain with other health issues or demographic factors is often not considered, even though these contextual factors are likely to be important in how we view pain. Although this may not quite describe the authors’ intention, I think they should try to shorten and focus the introduction around their main points. The bullet-points at the end of the introduction are quite succinct, in my opinion, and could be used as a guide.

I still do not like the phrase “implications of pain.” I recommend stating more explicitly what was examined (i.e. differences in daily functioning and healthcare seeking). The same goes for phrasing such as “pain aspects and consequences of pain,” and "severe characteristics." These terms are just too vague and make it difficult to interpret the actual meaning of results.

I disagree with the following statement (discussion): “the issue of multiple testing is here not a major concern as the study is more a descriptive study rather than a hypothesis testing study....” In fact, the authors are testing hypotheses and using inferential statistics to do so. It is true that multiple testing is not likely to cause type I error with extremely small p-values, but not all "significant" p-values are vanishingly small in this report. Setting statistical significance at 0.05 seems suspect.

The differences between pain categories remain difficult to conceptualize. Perhaps the authors could provide example manikins (either in the paper itself or in a supplement) to illustrate the different categories. Could the authors also report the average number of bodily pain sites for each pain category so that “spreading” is described numerically as well as theoretically/categorically?
Discretionary Revisions:

I understand numeric pain rating is an ordinal scale, but I still recommend treating it as continuous data when reporting. Mean (SD) is more informative than median (range), especially when range is 0-10 in all cases.

In the discussion: It does not follow that, because the IASP definition of pain is used by the authors, the self-report of bodily pain sites is necessarily valid and clinically relevant. I have administered pain assessments including body manikins in my own work, and it is not uncommon for a patient to simply forget to mark a site where they have pain (especially, in my experience, if it is less interfering or more intermittent than another marked site). Regardless, I question whether small changes in pain report (resulting in assignment to a different pain category--LP v RP v WSP) should really influence the clinical impression.

**Level of interest:** An article whose findings are important to those with closely related research interests

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

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests