Reviewer's report

**Title:** Is there a difference? Patients with chronic widespread pain and chronic local back pain in primary care - A comparative cross-sectional analysis

**Version:** 1 **Date:** 1 October 2013

**Reviewer:** Bård Natvig

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This article provides data on the difference between persons with local LBP versus LBP as part of chronic widespread pain in primary care (e.g. among GP practitioners). The data is descriptive and not surprisingly there are clear differences between these group. Even if this no more is surprisingly news it is important to describe these differences in different populations and in different settings. The results are of interest. In my opinion this article needs some revision before it is good enough for publication, and I have some remarks and suggestions:

**Major comments:**

1. In conclusions in the article as well as in conclusion in the abstract the authors conclude about treatment "should be urgently treated". That may well be so, but this study does not infere anything about treatment, neither efficacy, timing or modalities of treatment. In my opinion the conclusion should be restricted to the differences between the groups described in the study. The last sentence on page 5 about developing specific treatment approaches is more in line with the study, but such development has to be done in other studies than the descriptive study reported here.

2. The result presentation is based on rather short text presentation of the main results and five tables, and some of the tables are very large. In my opinion the data could be better presented with some more results from the tables highlighted in the text. Furthermore I think some of the details in the tables could be omitted. In my opinion table 1 with practice characteristics is of limited interest, and the main results from this table could be presented as one or two sentences in the text. Table 2 about sociodemographic data can also be reduced in size and details. Table 4 about therapy course has so limited content that it easily can be described as text.

3. I suggest that the discussion shoul focus more on the results from this study. Methodological limitations should be discussed in more detail in one section, and not in between more clinical focussed discussions, see line 5 and 6 on page 13.

**Minor comments:**

1. I am not a native English speaking person myself, so I am not the right person to judge the quality of the language, but I have a feeling that the article might
2. I am a little concerned about the use of the phrase psychosomatic symptoms in the discussion. The phrase psychosomatic perhaps infers some form of causality of the registered symptoms. In my opinion symptoms are symptoms, and we do not gain scientifically by dividing them into psychosomatic and somatic symptoms.

**Level of interest:** An article of importance in its field

**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.