Reviewer’s report

Title: Prognostic Factors in Sciatica: A Systematic Review

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Reviewer: Freek Lötters

Reviewer’s report:

The authors conducted a systematic review on prognostic factor in sciatica entitled : “Prognostic factors in sciatica: a systematic review”

They conclude that there’s no one strong or consistent predictor of persistent disability in sciatica cohorts. Furthermore they stated that the heterogeneity of the available studies makes it difficult to draw firm conclusions.

Although the systematic approach of the review seems alright, the conceptionalization is what I’m worrying about. I will elaborate on this in my general remarks.

General remarks.

It starts with the rather broad definition of sciatica used in the review “pain down the leg which spreads below the knee”. It is questionable whether this is an accurate definition of sciatica. Medical anamneses and physical examination on neurological signs (eventually followed by radiological findings) will yield a much more accurate and valid definition. Than solely built on selfreported symptoms. Besides summing up the different definitions used by the included studies, the authors need to address this more in their manuscript.

I have a bit a problem with the fact that the prognoses for sciatica is not well conceptualized. Prognosis says something about recovery. However the definition of recovery is broad as we can distinguish : medical recovery (i.e. diminished radiating leg pain; prolaps withdrawal etc.), or functional recovery (i.e. persistent disability in daily life, being able to work etc.). So taken recovery in a broad sense makes it difficult to interpret the data in an accurate and valid way. The authors need to address this in their manuscript.

In this respect it remains unclear what the authors mean with a ‘good outcome’ and a ‘poor outcome’. In table 6 only the poor outcomes found in at least three studies are addressed, however the study by Jensen (assessed as a high quality study) does show some significant good outcomes (defined by a combination of diminished pain intensity and functional disability). However these results are not are not considered further in the manuscript.

The background paragraph is very short. I miss the context of the research. Why is it important to know what the prognostic factors are for sciatica? And what perspective is been taken? The perspective of the medical doctor, societal perspective, treatment perspective? Now it looks like they are all taken into
account, resulting in a review with heterogenic studies. This is related to my foregoing remark on the kind of recovery that is taking into account.

Whether patients got a treatment or even the kind of treatment was not in the inclusion criteria. Only chirurgical procedures where excluded. However, the kind of treatment (even usual care) might influence the prognoses of sciatica. In all studies except one there was some kind of treatment applied. The authors need to say something about treatment influencing the prognosis. Also hospitalization was no inclusion or exclusion criteria, whereas this might be an indication of severity of complaints. People with high pain intensity or high functional disability tend to visit a medical specialist more often. Although most study included were on hospitalized patients, some where not and this might obscure the results of the review.

An other issue that is omitted in the manuscript is the natural course (or prognosis) of radiculopathy; approximately 80 % will be free of symptoms after 3 months. Medical guidelines also state not to interfere (except for pain killing) in first weeks and advice patients to stay active as much as possible. So, it would be appropriate to know what prognostic factors are for those with neurological signs due to a disc prolaps after 3 months of onset of the complaints. However, this aspect is not addressed in the manuscript.

Discussion.

Comparing studies on non-specific LBP and sciatica (specific LBP) is (in my opinion) evaluating the difference between apples and pears. Sciatica (radiating pain below the knee) is due to specific pathology in the lower back (disc prolaps). Long lasting sciatica is approached much more medically (eventually with surgery) whereas long lasting a-specific low back pain is approached from a cognitive behavioural way. In this regard it is recognized that the prognostic factors are different. As said before, the rational of knowing prognostic factors should be addressed more, also in the discussion.

Tables.

In the result section you mentioned that also in the study of Carregee et al. radiological findings were used. However this is not mentioned in table 4.

In table 5 (and also in the result section) the strength of association for the study by Carregee et al is presented as R=.50. Why did you present R and not a OR as measure of association? The authors of that study conducted logistic regression analysis, hence probably presented their result with ORs.

Also in table 5 the results of the study by Hasenbring et al. are presented by Beta (coming from the multiple regression analysis). However in this way it is hardly informative. You need to know the measurement level and model as a whole in order to be able to interpret these data. Moreover you described that pain intensity was the only outcome in that study. Was pain intensity the dependent variable in the regression analysis? If so how does the analysis contribute to the knowledge on sciatica prognosis? Pain intensity itself is not synonymous for
In table 6 you mentioned that the study by Miranda et al. did show no association between smoking and a poor outcome of sciatica. However in table 5 you presented a significant OR of 2.3. for the factor ‘ex-smoker’. You need to check this.

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

**Quality of written English:** Acceptable

**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