Reviewer's report

Title: A Proposed Diagnosis-Based Clinical Decision Rule for the Non-Surgical Management of Patients with Spinal Pain

Version: 5 Date: 5 July 2007

Reviewer: John Childs

Reviewer's report:

General

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

The authors have done a good job addressing the concerns. I appreciate what the authors are trying to accomplish and am always glad to see different viewpoints on how best practice can better be defined for patients with spinal disorders. However, I would reiterate my concern about how the ‘decision rule’ language is used in the paper. I do not object to the authors using decision rule language in the paper. However, as written, the authors imply that the publication of their 2 papers somehow means that a new de facto rule has been created and ready for practice.

Something like ‘A Theoretical Model for Developing a Diagnosis-base Clinical Decision Rule’ would be more appropriate. The authors should eliminate referring to this system as a decision rule because doing so implies that a new decision rule now exists simply because the authors have said so. Decision rules by definition are developed from prospective research, not theoretical models or non-systematic aggregation of existing literature. They also require validation and impact analysis testing prior to being widely adopted in practice. I appreciate that the authors have proposed a framework for a decision rule to potentially be developed, but its realization is years down the road.

I am not trying to be overly difficult here, but I think we do an injustice to the literature by describing theoretical models as ‘rules’, terminology that implies prospective research exists to support the underlying decision-making. Even compiling lots of corroborative evidence that indirectly supports some of the logic in the model (paper #2) is not adequate for labeling the model de facto as rule. It is a theoretical model that provides a framework for developing a rule in future prospective research. Although the authors certainly acknowledge the need for future research, this does not negate the need to remove inferences to this model being a rule itself at this point in its evolution.

Despite the authors’ apparent suggestion to the contrary, making these changes does not therefore mean that the model has absolutely no use in
decision-making now. Certainly in areas where no evidence exists, even a theoretical model may prove better than nothing. The subsequent work that ensues from this model will hopefully fill some of the gaps in decision-making for patients with spinal disorders. They are indeed plentiful. So long as the text does not convey that a new de facto rule now exists based on the publication of this paper, I do not have any further edits to provide and think the paper is ready for publication. Thank you for the opportunity to review this work.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

None

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Discretionary Revisions (which the author can choose to ignore)

None

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

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.