**Reviewer’s report**

**Title:** Computer-aided evaluation of inflammatory changes over time on MRI of the spine in patients with suspected axial spondyloarthritis: a feasibility study

**Version:** 0  **Date:** 20 Jun 2017

**Reviewer:** Torsten Diekhoff

**Reviewer's report:**

Thank you very much for this well-written manuscript. The authors propose a computer-aided approach for the assessment of inflammatory changes of the spine in patients of the SPACE cohort. Although the topic is of some interest in general and has potential to improve the assessment in an SpA study cohort, the presented pilot study suffers from some deficiencies. Therefore, I have concerns regarding the publication in the current form.

In general:

The authors compare the changes measured by the Berlin scoring method with the subjective change that was assessed using the CA-scoring. In my opinion, it is entirely unclear, why the authors did not use the same system to evaluate the change over time. Instead of using the Berlin score, the readers could have used the same change score (-2 to +2) that they applied for the CA method. That would have produced more comparable data. I admit that the use of a standardized scoring system is of some value. However, as the change in the Berlin score can take on a value of -3 to +3 the results are not comparable.

To prove the value of the new scoring method, I would have liked to see a comparison of the inter-method kappa to the inter-rater kappa, proposing that there is no severe difference in the methods if both kappas are similar. Kappa values are missing in the manuscript which is quite unusual for a study with two or more readers. Instead, the authors present descriptive data treating a vertebral unit as a subject and, thus, artificially enhancing their research collective. This approach is especially concerning because the cohort is an early cohort with only a few changes (see Table 1; only a few with sacroiliitis on imaging, changes of the spine are less likely). Therefore, data should be provided as sum scores on patient level. The total number of lesions should be given in the beginn of the results.

To show the benefits of the CA method clearly, an assessment of the requirement of time and the satisfaction of the readers would be interesting data. As the authors mention in their limitations, this would enhance the whole manuscript. However, in conclusion, statements like "readers were very pleased" and "accelerate the scoring process" should be avoided because they are not supported by data. Nonetheless, the fact that 5 out of 30 (17%) were excluded from the analysis
and 7 / 30 (24%) had to be manually adjusted (in total 44%) shows that the method is not robust enough for practice, in my opinion. The authors should discuss this in more detail under limitations.

The process of the CA method is described very well and easy to understand. As CA (for example voxelwise statistics) in the field of neuroimaging is used since many years for studies, the authors may wish to update their reference list and briefly discuss the benefits in the discussions.

A few remarks for specific details:

Abstract: Please confirm that the patient numbers in methods and results match the numbers in the manuscript. Please state clearly, how many and why patients were excluded. Please adjust the conclusion statement as mentioned above.

Methods:

Please avoid the term "enhancement" for the Berlin score as no contrast medium was applied.

The fact that STIR images only were used for the assessment should briefly be discussed. In my opinion, evaluation of STIR and T1 images is needed to judge for BME.

Discussion:

In my view, it is uncommon to see the limitations in the conclusion statement. Please provide a separate paragraph and discuss the limitations of your approach in more detail. Please also explain here, why you did not adjust the signal intensity, which is a potential source of error.

The paragraph dealing with stitched images of the spine can be deleted. Stitched images are standard - at least in our hospital - and the benefit to discuss this matter relating to CA is not clear.

Manuscript in general: Please provide vendor and country for software products and machines when appropriate.

In conclusion, the paper deals with a highly interesting topic, is well-written and easy to understand. However, I have some concerns with the methodology.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

Not relevant to this manuscript

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Please indicate the quality of language in the manuscript:

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