Reviewer's report

Title: MRI signal distribution within the intervertebral disc as a biomarker of spinal deformities

Version: 1 Date: 23 September 2012

Reviewer: David Stelzeneder

Reviewer's report:

General:
1. Is the question posed by the authors well defined?
   Yes.

2. Are the methods appropriate and well described?
   Yes, but revisions needed.

3. Are the data sound?
   Yes.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
   Yes.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
   Yes, but revisions needed.

6. Are limitations of the work clearly stated?
   Yes, but revisions needed.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
   Yes.

8. Do the title and abstract accurately convey what has been found?
   Yes.

9. Is the writing acceptable?
   Yes.

p5:
MINOR:
"The selected cases presented ... no treatments prior to the MRI acquisition"

MINOR:
"Siemens Medical Solutions" is now called "Siemens Healthcare".

MINOR:
"MR images were performed on a 1.5T system"
On a single 1.5T system? or multiple scanners? which type of scanner?

MINOR:
As I understand the L4/L5 or L2/L3 was selected. I think the signal histograms obtained from different discs can be different because of different shape and different size of the disc.
I think this is problematic and a limitation of this study, that should be mentioned. How many discs in which level were included in which group? please provide in a table.

MINOR:
Please label the axis in Figure 3.

MINOR:
"Figure 4: Segmented IVDs for control (a), scoliosis (b) and spondylolisthesis (c)."

Figure 4b shows a cupid's bow contour, which is not necessarily associated with scoliosis, but is also common in normal IVDs.
Figure 4 suggests that this shape is caused by scoliosis. Or do the authors have data suggesting this is more common in scoliosis?

see
Schmorl nodes of the thoracic and lumbar spine: radiographic-pathologic study of prevalence, characterization, and correlation with degenerative changes of 1,650 spinal levels in 100 cadavers.
Pfirrmann CW, Resnick D.
P.6
MINOR:
"the mean cancellous bone intensity of the above vertebra" was used for normalization of signal intensity.
Did the authors make sure, there are no abnormal findings in cancellous bone signal like Modic type changes?
"the mean CSF intensity was highly influenced by both the pathology (p=0.02) and the severity (p=0.04)."
Should the CSF signal intensity not be constant, when normalized to CSF?
Please explain this clearly in the manuscript.
If the normalization to bone is unreliable why did you use it in your manuscript?

"a reproducibility study proved that intra-operator variability was negligible"
Do the authors have data on that? Please mention numbers if available.

Are the Figures included in the manuscript the result of the described "automatic segmentation processes"?
Otherwise please provide such examples.

Table 1: please provide data on time of the day, age and BMI for each subgroup, maybe in a separate table.
Where the group characteristics comparable?
There are some typos.

General comments: The authors present a sound scientific paper, however, the clinical relevance of the used methodology has to be shown in future studies. The methodology should be explained more clearly.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**
I declare that I have no competing interests.