Author's response to reviews

Title: Magnetic resonance imaging after most common form of concussion

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Author's response to reviews: see over
Dear Sirs,

We appreciate the constructive comments and suggestions for improvement of our article by the reviewers Erin Bigler and Yu-Chien-Wu.

Below we respond to the different points raised. The changes in the manuscript are in bold letters.

**Review of Erin Bigler**

We fully agree with the notes about the main shortcoming of our study, i.e. the use of the low field strength of the 1.0 Tesla MR. When we started our study in 2005 this was the only available machine in Kaunas. We hoped to finish the study within a time span of one year. However, because of the extraordinary strict inclusion and exclusion criteria, it took much longer time to include the intended number of patients and controls. Later, a new 1.5 Tesla machine was installed in Kaunas. However, during an ongoing study we could not switch over to the new equipment. We have already acknowledged the major methodological shortcoming in our manuscript and include the recent work by Niogi et al published in Brain in the discussion about the shortcomings of recent MR DTI studies, in particular, the lack of a homogeneous group of patients with concussion and loss of consciousness below 5 minutes as well as use of inadequate control groups.

**Review of Yu-Chien Wu**

Major compulsory Revisions

1. *The authors need to refine their article especially the Background and Discussion sections to make clearer statements.*

We agree with the referee and have made several refinements in the background and the discussion section (under conclusions).

2. *The author mentioned that it is not clear that whether the cause of PCS is organic or psychogenic. It seems that this is the motivation of this study. I would like to know the contribution of this study in answering that question.*

The contribution of this study in relation to the organic cause of PCS is now addressed in the conclusions. The study is of course not capable to give direct support for a pure psychogenic etiology.

3. *For the Background section, it would be better to add a third paragraph that very briefly addresses how the authors plan to do their experiment. Why they think convention MR is adequate to answer this problem? Why not CT, EKG or other advanced MR such as DTI, MT and fMRI?*

The last paragraph of the background section gives, in our opinion, a clear explanation for what was the aim of the study. For this aim (i.e., to see if there are detectable traumatic lesions if one restricts the investigation to only include cases of concussion < 5 minutes) the
use of MR was a natural choice. The reason why we used a 1.0 Tesla machine is explained in the answer to the first referee. A DTI MR technique was not available at the beginning of the study.

4. In Table 1 and 2, there are five patients and two controls with positive MR finding. Please explicitly explain why the final statistic testing used only three and one for study and control group, respectively. Please define what the “positive MR finding” caused by PCS is.

In two patients and in one control the findings had no potential for being traumatic (subependymal periventricular heterotopia and ependymal cyst in left lateral ventricle in concussed patients; pineal cyst in a control). These findings were accidental and had obviously no relationsship with the trauma. It would, therefore, be incorrect to include them in the statistical evaluation.

5. In the Discussion section, second paragraph, is the magnetic field strength a limitation of this work or not? If it is a limitation, what would be the benefits from higher field? If it is not a limitation, did those studies with lower field strength (how low?) reach acceptable detecting power?

In this paragraph we say clearly that the use of a 1.0 Tesla MR is a limitation and here as well as in the refined conclusion section we address the issue of detection power.

6. I am not sure the purpose of mentioning diffusion tensor imaging (DTI) in the Discussion section. What are the authors trying to say? Is it potentially useful for similar studies? Do results from other DTI study support results from this study or not? How alcohol abuse and DTI relevant to this study?

Since this technique has been used in recent years because of its ability to detect discrete signs of axonal injury, we think it is important to inform the reader about the methodologically shortcomings and the inconsistent results of the MR DTI studies done hitherto. Only after this information is given one is able to understand the proposal in the conclusions that if future studies use DTI after common concussion, it should be with a strict controlled design, in particular avoidance of non-injured healthy individuals as controls. This is mainly because, as we say, there is a link between alcohol consumption and injuries. Consequently, concussion populations most likely have higher average alcohol consumption than non-injured controls. If this precaution is not taken, discrete positive findings of axonal injury in concussed patients may merely mirror the fact that injured patients may have a higher alcohol consumption with ensuing higher probability of discrete axonal lesions than non-injured controls.

7. I would recommend rewrite the conclusion. The conclusion should summarize the contribution, the potential application and the take home message of “this” study.

The conclusion has been rewritten
Minor Essential Revisions

1. In the Background section, the third sentence beginning with “Several authors …”, please cite appropriate papers.

We have included citations of two articles.

2. In the Background section, the last sentence of the first paragraph starting with “In recent studies …”, please rephrase this sentence or make it shorter. Also why is it important to mention the “insignificant” results of previous studies here? Do you imply PCS is psychogenic?

We have omitted the whole sentence.

3. In the Methods section, the second paragraph, one sentence does not make a paragraph.

We have joined this sentence with the last paragraph.

4. In the Method section, about the MR imaging, please include the total scan time, slice number and the anatomical coverage.

These details have been included.

5. In the Results section, description of patients’ age range and mean should be in the Methods section.

Since patients’ and controls’ age range and mean are not known in advance and hence can not be defined in the method’s section, we think that it belongs to the result section.

6. In the Result section, please refine the section of MR findings. Please only report those findings that are important to this study. For instance, I am not sure subcutaneous hematoma has critical meaning to the whole study.

We have refined the section of the MR findings.

7. In the Discussion section, I am not sure it is a good idea to repeat so many details of other researches. Please make key points clear and explicit. Are those studies relevant to this study? Do their results support, compliment to or conflict with results of this study? Do their results help to answer the bigger question introduced in the Background section?

We have taken away the section about the details in earlier studies and only address their methodological shortcomings. In addition, it is told that there were inconclusive or lacking results concerning the issue of common concussion in these studies.