Reviewer's report

Title: A Gaze-independent Audiovisual Brain-Computer Interface for Detecting Awareness of Patients with Disorders of Consciousness

Version: 0 Date: 06 Nov 2017

Reviewer: Camille Chatelle

Reviewer's report:
The study investigates the use of an audiovisually-based BCI for assessing patients with disorders of consciousness. The development of alternative tools based on EEG that could reliably detect signs of consciousness in this population is of real interest as we know that misdiagnosis is not rare.

General comments:
Method:
The CRSR was done before the experiment (please provide a range x to Y days before): is it possible to have the diagnosis on the day(s) of the experiment (was a CRSR performed during the experiment?)? Indeed, it is possible that some patients were more responsive than before. In addition, only one assessment was performed. This should also be stated as a limitation of the study (cf. Wannez's recent paper highlighting the need for minimum 5 assessments to get a reliable diagnosis). As the purpose of the current paper is to develop a system that would supplement the behavioral diagnosis, the characterization of the behavioral profile of these patients is key.

In the current version of the paper, the behavioral profile of the patients is unclear. VS4 for example is described as having emerged during the experiment (i.e. suggesting a diagnosis of MCS at the time of the study), then in the discussion, the authors describe this case saying that he/she recovered one month after the experiment. I would suggest the authors to look for such discrepancies and make sure there is as much information as possible to better characterize the patients included.
There is also no information in the Method about some "outcome assessment" (eg after the experiment [when, if any?]).

It is unclear to me why the authors performed 2 different offline analyses of the data (healthy subjects and DOC). It also looks from the result section that an ERP analysis was also performed in DOC but it is not defined in the method section.

I would suggest the authors to review for English and make sure all abbreviations are properly defined and used (eg, JFK CRSR vs. CRSR)

I would also suggest to add a paragraph on study limitations.

Specific comments:

Abstract:
"The BCI system was first evaluated by both online and online data from ten healthy..." (error?)

Introduction:
It has now been suggested that VS should be replace by unresponsive wakefulness syndrome (cf see Laureys 2010)

p3-line31: replace "acquisition of" by "recovery of".

p3 - line 47: these data are based on a re-assessment of patients using a standardized behavioral scale (i.e., the CRSR). Therefore, the point made by the authors cannot be used as it is here. What could be used are the findings obtained using active tasks such as Cruse et al, Owen / Monti et al. papers.

p3-line 49: I think the authors should also cite their own study here (reference 14). I am also wondering why only the Study from Lulé is discussed in this section.

P5-line9: please modify this sentence, as not all DOC patients lack the control of eye movements.

P5-line 29: is there an error in this sentence (see also comment for abstract)? This part is also pretty unclear to me.

p5-line 9-31: I would reduce this section as a lot of information provided here should be part of the method and not the introduction. I would focus on the aims of the present study and hypotheses to smooth the reading.

p5-line 40-46: if this is not being asked by the journal, I would remove these sentences.
Method:
I would try to avoid the repetitions when not necessary (e.g., instruction, or what is congruent vs incongruent)
P8-line 38-41: this part in unclear. Could you clarify when were the 5 different runs administered? (eg, over the course of 2 days? 5 days? several in one day?)
p10-line 50: p values below .05 (please correct)

Results:
Please provide p values.
p12 line 50: error in parentheses? (< 37.3%)
p13 - line 34: as the CRSR was done before the experiment (which should also be stated as a limitation of the study in the discussion section), this cannot be considered as emergence, the patient should therefore we characterized as MCS instead of VS for the study (see general comments above).
VS4: How long after the experiment did VS4 emerge from MCS?

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.
Unable to assess

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