Author’s response to reviews

Title: Exploring Social Cognition in Patients with Apathy Following Acquired Brain Damage.

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Author’s response to reviews: see over
Dear Editor of BMC Neurology

Ref: MS: 1037322639839416: Exploring Social Cognition in Patients with Apathy Following Acquired Brain Damage.

We are pleased to submit for your journal’s consideration our amended manuscript: ‘Exploring Social Cognition in Patients with Apathy Following Acquired Brain Damage’, by Progress Njomboro, Shoumitro Deb, and Glyn Humphreys.

We have taken careful consideration to the reviewers’ suggestions, and improved our manuscript in light of these suggestions. Below we outline the amendments that we have made with regard to each of the reviewers’ suggestion. We hope the editorial board and the reviewers will be satisfied with our efforts to meet those suggestions.

We also would like to thank the reviewers for their effort to improve on our work. We have put the reviewers’ comments in **bold and italics**, to distinguish them from our responses.

Sincerely,

Progress Njomboro (on behalf of all the authors)
Response to reviewers.

Title: Exploring Social Cognition in Patients with Apathy Following Acquired Brain Damage.

Reviewer 1’s report:

1. *The authors’ last point on page 4, about “subconscious emotional signals that pre-bias and influence goal-directed social behavior” needs expansion – i.e., how would this theory influence the likelihood that apathy is specifically related to social cognition or moral reasoning, other than the same argument that similar structures are involved?*

   We have added: It’s possible that deficits in the mechanisms that generate these signals may underlie apathy related socio-cognitive dysfunction. The ventromedial prefrontal cortex is thought to play a crucial role in generating these signals [57], and has also been implicated in apathy [64]. Furthermore, patients with apathy show distinct deficits in their ability to initiate or sustain goal directed social interactions [13]. We also discuss how this may relate to our findings in the discussion section of the paper. (Lines 102-107)

2. *The authors added a sentence to state that they used three tests of EFs because some participants had deficits that precluded testing with one or another of the three. This rationale implies that the tests are psychometrically interchangeable, which is not the case. The authors should describe the process of comparing scores from different tests.*

   We have now clarified how we used this data by adding ‘We however did not take these separate scores as interchangeable. We treated each of the executive function scores as a distinct covariate in our analyses’. Lines 121 - 126

3. *The authors’ statement that, “We took ‘A’ responses to indicate that participants judged protagonists behaviour as normative” is not sufficient explanation for the scoring system. How many “healthy controls” provided “normative data”? What were the characteristics of the healthy controls and how were data collected? Why the 95% cutoff for agreement? Presumably the “normative” data (in quotes because it is not clear that standard procedures for norming a measure were followed) were not from the healthy comparison participants in the study, as that would mean that half of the items should not have been included because typical adults did not agree on answers.*

   We have now made specific reference to Table 3 that gives the participant composition on each of the test. (Lines 133- 136)

Since this table gives participant characteristics across all the tests, we had placed it at the end of the methods sections but we can see how this would create problems for the reader. We have now made specific reference to this table earlier in the methods section.

We agree the way we created our normative scores on the SAT test isn’t in line with how tests are normed. However we feel our method makes practical sense. By only using items...
where there is a 95+ agreement, we ensured that this agreement meets the statistical cut off equivalent to a .05 alpha level, which is the standard in most research. We feel this approach allows us to pick significant differences where they actually exist. We have now mentioned this in our manuscript. (line 352)

4. **The contribution of inadequate power to results should be addressed explicitly in the results, as non-significant findings often were in the expected direction (e.g., the large difference between healthy comparison/non-apathetic participants and those with apathy on the Ekman test, which does not make sense as stated, given that median values for healthy controls and non-apathetic patient groups were identical).**

We have now acknowledged this point both in the results section (Lines 410 – 412) and in the discussion. (lines 555-562).

We have also re-done the analysis of our ToM test results and used a MANOVA instead of multiple tests across different aspects of the ToM tests. (lines 382-395)

We have also mentioned that where multiple tests have been performed (on the Ekman and Hexagon results), a Bonferroni correction was imposed (lines 410-412)

5. **The authors do not state methods for addressing alpha slippage associated with the multiple comparisons shown in Table 6.**

We have now started the Benferroni correction performed to address these power slippages (lines 410-412), and also acknowledged this weakness in the discussion.

6. **Memory and language test scores should be reported.**

We have now included these in the MANOVA on the ToM tests. (lines 383-389)

7. **Some statistical analysis results are still reported in a way that misrepresents the analysis – e.g., “Chi-square tests revealed that there was a significant the presence or absence of apathy in patients had an effect on whether or not patients correctly identified normative behaviour.” The statement “had an effect on” implies regression, although the sentence appears to be incompletely edited to this might be an oversight.**

We have noted and corrected this.(lines 356-367)

8. **The authors are again reminded to use person-first language (e.g., patient with brain injury rather than brain-damaged patient).**

We have now corrected this throughout the manuscript, substituting ‘brain-damaged patients’ with ‘patients with brain damage’ and also done the same with ‘apathetic patients’ and changing that to ‘patients with apathy’, and ‘non-apathetic patients’ to ‘patients without apathy’.
Reviewer 2’s report

1. the paper still lack a structure that makes it ready for publication. I would suggest that the authors give page, paragraph and lines for correction when replying to the reviewers. The paper still lack a rigor concerning the order of reporting both in the paper and the Tables. This specially denotes to the naming of areas under study and order of tests. The paper also needs to be improved on information of the control group as well as patients. This also goes for the methods section concerning statistical methods. The result section needs to be more structured with clear headings and subheadings. Tables need still to be more clear in their format. The discussion needs to state more clearly from the start what is the most important and sound finding of this study and would be more clinically usefull if they had one or two sentences following up on the rehabilitional value p 22, paragraph 2. Some minor miss spellings needs to be corrected.

We have re-visited the manuscript and made more amendments where we thought our presentation posed problems. For instance, we have created more sub-headings in the discussion section, and also strived to maintain the same order of presentation of tests across the entire report.

2. Order: In the Introduction TOM, Emotion perception, moral reasoning and social awareness is the order of the areas being studied. Later on in the Methods (M) and Result (R) and Discussion (D) section this is turned around with TOM reported as number three in the M and then as the last under R. The same lack of order is for the other areas being studied as well as the order of the tests. In Table 4 foreseen harm is first and intended last, but in the writing on page 7 intention comes first and also on page 12 under results. The same is with the TOM tests in the paper (p 8) and Table 3. This confuses the reader and one wonders why this is done like this. Mentioning and reporting should have logic and when changed should make sense and not disorder as this reporting does to me. For readers not familiar with the tests such an order will help the reading of the paper.

We have taken note of the suggestion and made sure across the entire manuscript the presentation of tests, their results, and their discussion follows the same order. The order we have now adopted is:

i. Clinical assessments (apathy and depression)
ii. Executive functions (Brixton, Hayling, and Stroop tests)
iii. Moral sense test
iv. Social awareness test
v. Theory of mind test (reality known and reality unknown)
vi. Emotion perception tests (Ekman 60 and Emotion hexagon)

3. Naming: Under R Moral sense test is a heading which seems to me to be a new word for moral reasoning? In the abstract facial expression of emotion is used which is not used in the introduction.

We clarified this throughout the manuscript.
4. Table 3 has a heading: “Participants across tests”. What does this refer to? Table 4 needs a more clear heading and I suggest Moral reasoning.
   Table 6 is named emotion recognition but named emotion perception other places.

We have amended the headings of these tables as the reviewer suggested.

5. The study also needs more information about the control group as well as the patients. How was the control recruited? And how did you control for ”neuroligically intact”? Why did you choose only to test them on the areas under study and not also apathy, depression and executive function? What is not tested for should be reported clearly. I would like a Table with sociodemography for both patients and controls. I also would like the scores of AES and BDI reported with mean value and SD.

We have added the information requested by the reviewer by adding information on how the controls were recruited and also making a direct reference to their demographic information in Table 3. We have also provided the requested means and standard deviations for depression and apathy scores (Lines 274-281)

6. The methods still needs, in my opinion, more information about the specific statistical methods used in this study. This would make it easier to understand the reporting of results for a bigger audience. Also the results section have in my opinion a paragraph that usually are part of the methods section: page 12, second paragraph.

   No cut off is given for depression and should be stated. Naming og the TOM tests should find place here, since they are named in the Tables 5 as reality unknown and known.

We have amended the methods section to accommodate the reviewer’s suggestions, and provided information on the cut off for depression (line lines 155-156)

7. I suggest all results have a heading and then if needed subheadings. I suggest that the first part of the R section gets a heading (p 11). The following reporting p 12-16 have headings, but do not make distinctions between mainheading and subheadings for the tests and why they are reported in another order than the order they are introduced in is unclear. I suggest areas studied get a main heading and all tests their subheadings following the order they were introduced in the introduction and under the methods section. The order should have a logic.

Last paragraph p 14 is unclear to this reviewer why it is put in here and it also includes (p 15, line1) a sentence that in my opinion has a place in the discussion section.

We have now made specific reference to Table 3 that gives the participants’ demographic characteristics on each of the test. (Lines 133-136)