Reviewer's report

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

Version: 3 Date: 29 May 2013

Reviewer: Lyn Turkstra

Reviewer's report:

Major Compulsory Revisions

Comments are grouped according to questions provided in the reviewer guidelines.

1. Is the question posed by the authors well defined?

The authors identify a clear gap in the literature on neurobehavioral characteristics of brain injury, which has tended to view apathy from the perspective of cognition, particularly executive functions, rather than social cognition, while social cognition has been studied in relation to other behavioral features associated with social withdrawal, such as alexythymia. The authors' main rationale for seeking a connection between apathy and social cognition is that structures typically associated with initiative and engagement (i.e., in which lesions are associated with apathy) overlap with structures linked to moral reasoning and social cognition. This in itself is not a compelling argument, as there is a long list of neuropsychological functions in which frontal brain regions are involved. The authors also make a more compelling argument that it is logical to consider social cognition as a possible contributing factor to apathy, as one might imagine individuals with poor social cognition not being motivated to engage emotionally or socially with others.

The one point that could be clarified is why moral reasoning and social cognition have been grouped together here. These are often related but dissociable constructs (e.g., a patient may have intact moral reasoning but not accurately identify emotions from facial affect), and the authors’ argument in favor of social cognition is stronger, which suggests that moral reasoning might have been added when the social cognition results were not significant.

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?

The last two sentences of the introduction, on page 5, could be clarified. The authors state that they also measured executive functions and depressive symptoms “to separate the possible effects of executive deficits and depression
on social cognition from those of apathy symptoms”, as there is literature to suggest that both apathy and also social cognition are associated with executive dysfunction and depression. This statement requires specific modeling approaches, which are not described in the methods.

2. Are the methods appropriate and well described?

The video-based Theory of Mind test should be described in further detail, specifically the method for “controlling for memory capacities.” Otherwise, readers will not understand terms such as “working memory” and “inhibition control” in Table 5, and likewise will not understand the implications of the significant difference on the working memory control items.

The amount of detail in descriptions must be consistent across tasks and tests. A published work needs to stand alone, without readers having to refer to other publications for basic methodological details. Thus, further details about the Brixton and FEEST tests should be provided as they were for the AES-1. A sample for the moral reasoning test also would be helpful, as done for the Social Awareness Test.

A rationale for including two additional measures of executive functions (Hayling and Stroop) also is needed. In the methods, the Brixton is listed as the main measure of executive functions, but in the results it is listed third after the Hayling and Stroop.

It is not clear why education is listed in Table 3 rather than Table 1. The inclusion of education suggests that the authors thought it might be a potential contributor to study results, which should be noted explicitly in the methods section.

The Beck Depression Inventory is not described in the methods and was omitted from the between-groups comparisons on page 11 of the results. Thus, there is no foundation for including it later on as a covariate.

The methods section needs a data analysis paragraph that outlines the planned analysis methods. This includes the procedure for deciding if covarying on the executive function and depression measures was necessary (i.e., rather than comparing the two groups then deciding to include the Brixton anyway though it didn’t differ significantly between groups); statistical methods used for the different steps of analysis; criteria for excluding data; the reason for using nonparametric tests rather than attempting to transform the data; justification for presenting odds ratios; and methods for addressing alpha slippage associated with the multiple comparisons shown in Table 6. A data analysis section also would resolve some questions about the results, such as why a “relationship between BDI scores and moral judgements” was reported, which suggests a correlation, but the statistic reported was from an ANOVA, and likewise why a Chi-squared test result on page 14 was reported as an association, which again suggests a correlation. The way the authors framed the results suggests that a logistic regression might have been the more appropriate statistical analysis method, with social cognition and moral reasoning scores predicting apathy vs.
non-apathy, and the two predictors entered into the regression after depression and executive function test scores.

The finding that typical adults were at ceiling on the Theory of Mind test is not sufficient justification for excluding a healthy comparison group for this task alone.

3. Are the data sound?

There is a potentially fatal confound of language and memory impairments, the former particularly in the six participants with left-hemisphere lesions and the latter in the 11 patients with high risk for significant hippocampal damage (anoxia and HSE) as well as those with TBI. The presence of language and memory impairments that would confound results is suggested by the legend for Table 3, which states that “it was not possible to assess all patients on every task” but does not give reasons, and by the authors’ choice of the Brixton Spatial Anticipation test because of “its limited semantic loading.” Declarative and working memory impairments have been found to contribute to impaired performance on social cognition tests in previous studies, specifically for multi-sentence-type questions as in the moral reasoning and social awareness tests used here. The authors themselves note that “semantic loading” (i.e., verbal working memory demand) was found to be a contributing factor to performance on story-based tests in previous research. Memory and language test scores should be reported.

The authors state that they excluded Moral Sense test data for 9 participants because “we felt such participants did not understand the stories.” They should provide objective reasons for this decision. Likewise, quantitative justification for the Social Awareness Test scoring is needed. 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.

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).

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?

A response to this question is pending the previously noted questions.

6. Are limitations of the work clearly stated?

There is no limitations section in the discussion. The authors state only that the interpretation of moral reasoning test scores is limited by not having obtained emotion response data as had been done in previously by Damasio and colleagues.

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?

The abstract, while technically accurate, does not fully convey what has been found, as there either were trends for social cognition measures (i.e., emotion recognition) to differentiate apathetic vs. non-apathetic participants, or there was no “socio-cognitive” profile connected to apathy (i.e., no links to social cognition). The term “distinctively” means “different from others” and it not clear that this is the meaning the authors intend.

Minor Essential Revisions

9. Is the writing acceptable?

The manuscript is clearly and succinctly written. There is a need for minor edits, such as wording around in-text citations. The authors also should use person-first language (e.g., patient with brain injury rather than brain-damaged patient).

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

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.