Reviewer's report

Title: Alertness and visuospatial attention in clinical depression

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Reviewer: Karen Lidzba

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In their study, the authors studied differences in visuospatial attention between patients with depression and healthy controls, and the impact of sad mood and alertness on visuospatial attention. While differences in visuospatial attention between the two groups were not significant, alertness did influence visuospatial attention. The effects of sad mood were very small (non-significant and inconsistent).

The authors showed concern in choosing an appropriate control-group for their patients, which is an important prerequisite for a good neuropsychological study. Also the tests chosen are well established and therefore well interpretable. The findings add to our understanding of the interaction between basic (i.e., alertness) and higher-order (i.e., visuospatial) attentional functions.

I have, however, some major concerns concerning a) the rationale behind the choice of tests, and b) the interpretation of the results.

Major Compulsory Revisions:

Abstract:

1) I am not sure if the trendlevel result for the influence of sad mood on one of the three visuospatial tests is justification enough for the conclusion: “…alertness and – to a lesser extend – sad mood led to left omissions in visuospatial neglect tests”.

Background:

2) In the literature, the term “unilateral neglect” seems to be confined to phenomena in the wake of stroke, which lead to clear deficits in daily life. I am not convinced that the term “neglect” can be utilized for the phenomenon of slightly impaired visuospatial attention in the left visual field in the depressed patients of the study. In the hypothesis (p. 4) the authors seem to make a difference between “neglect” and “shift of visuospatial attention”, however, no definitions are provided, and in the rest of the text the terms “neglect” and “visuospatial deficits” seem to be used interchangeably. This issue should be better clarified.

Methods:

3) What was the rationale behind choosing the three different tests for neglect and visuospatial attention? Furthermore, the “Visual Scanning” test of the TAP is
originally not a neglect test, even if it could be interpreted as one in the modification introduced by the authors.

4) WAF neglect-extinction: What was the rationale behind summing the omission errors in the “extinction” and “no extinction” conditions? Would there not be differential effects to be expected?

5) Have the t-tests also been corrected for multiple comparisons? Did the authors check for interdependence between the three visuospatial tests?

6) Did the authors use only one item of the BDI as indicator of “sad mood”? Is this item answered with a rating scale, and is the expected variance large enough for the item to show effects in the regression analysis? (Maybe the variance could be added in the results section).

Results:

8) “However, the scores were compatible with more omissions in the left visual field - a rightward bias - in the depressive group, opposed to a more leftward oriented bias in the control group in all three tests.” This conclusion does seem convincing to me when checking table 2 (and when reading the detailed description of results which follows):

a) In the visual scanning task, patients show a leftward bias, as do the controls; in the neglect task, the “rightward bias” is tiny.

b) Controls show a true leftward bias only in the visual scanning task. In the neglect test, the “leftward bias” is tiny, in the neglect-extinction test there is no bias at all.

Discussion:

The authors tend to over-emphasize (in my opinion) non-consistent trend-level effects. The discussion should demonstrate much more caution with regard to the results:

9) The discussion regarding effects of mood has to be much more cautious, as there is only a trend-level effect in only one of three tests. Furthermore, when applying Bonferroni-correction for six tests, trend-level can be assumed at p< (0.1/6) = 0.017. While the effect is in the hypothesized direction, and can therefore be mentioned in the discussion, I find the authors’ conclusions much too confident.

10) The absent effect of sad mood on visuospatial attention in the group could perhaps be explained by a low variability within the whole group in the sad mood parameter. This leads to a missing train of discussion: Do the authors assume a specific effect of depression on visuospatial attention, or to they assume an effect of “sad mood”, which should be noticeable also in the non-depressed controls (if the variance is large enough). The same question should be answered with regard to alertness. Only if a continuous, not depression-specific effect is expected, it is feasible to collapse the two groups for the regression analysis.
11) 2nd paragraph: “In our patient sample there was a non-significant albeit consistent trend towards leftsided neglect seen in positive values in the number of omissions in the three neglect tests, i.e. an attentional bias towards the right visual field.“
This is not true, as the effect was noticeable (but nonsignificant) in the WAF, small in the TAP neglect and nonexistent in the TAP Visual scanning. So there is no consistent trend.

12) 5th paragraph: “TAP Neglect and WAF Extinction-Neglect revealed effects of alertness and mood”. This is not true, as is even described in the following paragraph.

13) 7th paragraph: In line with my previously mentioned concerns, this paragraph should focus more on the effects of alertness than on the effects of negative emotion, as is done in the conclusions.

Minor Essential Revisions:

Background:
1) The paragraph on the functional neuroanatomy of neglect symptoms in depression is not very clear. What, for example, is meant by “concerning clinical implications” in “Concerning clinical implications, more activation in the right hemisphere, especially in frontal regions, seems to be indicator of negative affect.”?

Methods:
2) Have all participants been included into the regression analysis or only the depressed patients?

Results:
3) Results of regression: This could be more structured with regards to results for the variables “sad mood” and “alertness”.

Minor issues not for publication

The text should be thoroughly reedited with respect to English language.

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'