Reviewer's report

Title: Cognitive "branching" processes in major depression: evidence for a generalised learning deficit

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Reviewer: Ted Huey

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General Comments

This is an interesting and well-presented study that found that patients with depression did not show deficits in cognitive branching. This study has several strengths. The question is interesting and relevant. The measure of cognitive branching is innovative yet developed and tested in other populations. The design and findings are well-presented.

There are some weaknesses to the study, however. The number of patients and control subjects are small, and I am not convinced that the study was powered sufficiently to answer the question they are asking (more about that below). The authors draw some conclusions from their results that I think may not be warranted (see below).

Points to address

• My main recommendation is that the authors provide more information and discussion about the issues of power and the size of the effect they are looking for. This is essentially a negative study, which is fine, but it puts the burden on the authors to show that they have enough data to effectively accept the null hypothesis that there are not significant differences in cognitive branching between their patient and control groups. The authors should perform power calculations to support or refute this conclusion. It would probably not be appropriate to use Koechlin et al (1999)'s data as that was obtained from patients with gross frontal lobe dysfunction to perform these calculations, but they could perform power calculations using other neuropsychological tests that have shown differences between depressed patients and controls from one of their references.

• A related point, which should be discussed in greater length in the paper, is the difference in effect size between neuropsychological deficits in patients with frontal lobe damage and patients with depression. In the Discussion section of the manuscript, the authors state, “deficits have been found in cognitive branching with patients with APFC lesions (n=7) [19], arguing against the criticism that this task lacks sufficient to detect group differences”. In the literature, the magnitude of neuropsychological deficits in patients with frontal lobe damage is much larger than that observed in patients with depression. To equate them, as the authors do in that sentence, is misleading. In fact, if the power calculations suggested previously suggest that 11 depressed patients and
controls are insufficient to test for a difference that is shown by 7 patients with lesions, this could be a starting point for an interesting discussion comparing the relative differences in neuropsychological deficits between these two patient populations.

- In the Conclusion sections of both the abstract and the discussion, the authors state that, “our data argue for a generalized learning impairment underlying cognitive dysfunction in this disorder.” This is the opposite of the conclusion I would take from the study – at least in regards to the authors’ main hypothesis, the study is negative. The conclusion should reflect this.

- I do not understand why the patients and controls made more errors in the control condition than the apparently more difficult other conditions. Please explain.

- Because you ran the runs sequentially, you cannot separate out the condition and order effects. You should probably discuss this and how one might perform future experiments to address this issue.

- At the beginning of the Methods section, it is confusing when you talk about 12 subjects in each group when Table 1 and all of the analyses have 11 subjects in each group.