Reviewer’s report

Title: Hypothalamic-pituitary-adrenal axis functioning and dysfunctional attitude in depressive patients with and without childhood neglect

Version: 1 Date: 25 November 2013

Reviewer: Sarah Doucette

Reviewer’s report:

The authors present a study examining the relationship between self-reported childhood neglect, the cortisol awakening response, and dysfunctional attitudes in patients with depression compared to controls with no depression. The design and methodology appear sound; however, several aspects could be elaborated on to help the reader interpret the findings.

Major Compulsory Revisions:

1) The authors make no mention of the age, sex, or any other demographic component of the study sample. Although it is reported that there is no difference on these variables between the groups and that the control group was age and sex matched, it is still important to report these variables for comparisons across other studies. Was the sample an elderly sample or in early adulthood? This makes a large difference when interpreting the findings. A mean age, and distribution of sex must be reported.

2) How many patients were originally recruited, and was there attrition? Do the authors have any information on those subjects who dropped out (if any)?

3) Tables 1 and 2 require major revisions.

   TABLE 1: It is not clear which groups the authors are comparing. Please indicate what the p-value is representing. Please revise accordingly. For clarity, could the authors please re-label the groups to something more informative than G1, G2, etc. The title of the table could also be revised to be clearer. For example, differences in HPA axis functioning, DAS, and SSRS between depressive and control groups with and without childhood neglect.

   TABLE 2: It is not clear what values the authors are presenting under each column. Is it the difference in scores between the groups? A more clear way of presenting these findings would be to present the mean values for each group separately then the p-value following these estimates horizontally. Please revise this table to more clearly articulate the results.

4) Do figures 1 and 2 represent the entire sample? Please indicate which subjects these figures represent?

5) The authors need to be careful in over interpretation of the findings. The findings and the nature of the analysis do not lend knowledge of effect modification of SSRS. Suggesting that SSRS might modify the severity of depression resulting from childhood neglect is incorrect. A correlation between
SSRS and HAMD scores does not indicate this. Similarly in your conclusions, please omit the statement “social support can adjust depression associated with childhood neglect”, as this is not valid based on the results.

6) Please elaborate your limitations section. There are several limitations that need mentioning including the following:

a) The self report nature of the measures used, particularly the measure of childhood neglect. Were the patients in depressive episodes when completing these measures? The authors need to comment on how this may have biased the findings as well as the limitations of using a self report measure of early trauma.

b) The very simple unadjusted analyses.

c) Potential unmeasured confounders, particularly associated with the cortisol collection.

Minor Essential Revisions

1) The authors label the control groups as “normal subjects” in their abstract. Please change this to control subjects. Labeling groups as normal and not normal runs into issues of ethics.

2) Please state where the sample was from in the methods and in the abstract.

3) Can the authors please explain how the controls were recruited in the methods section? Are there any possible selection biases associated with how they were recruited?

4) Are there any validation studies available on the social support rating scale used? The authors do not comment on the reliability or validity of this scale is there a reason for this?

5) Cortisol methodology: Do the authors have any information on the sleep quality, or current mood state when subjects were completing the cortisol collection? These are major potential confounders. Were the depressive patients in a current episode, or were they in remission? What about BMI, as this is also associated with elevated cortisol.

6) The legend of table 2 indicates healthy control without childhood trauma. I am assuming this is a typo and should be neglect?

7) The manuscript should be edited by a native English speaking individual. There are several grammatical errors, and the language could be improved for clarity.

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 related to this manuscript.