Author's response to reviews

Title: Driving behaviour in adults with Attention Deficit/Hyperactivity Disorder

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Author's response to reviews: see over
Wednesday, 06 May 2015

Dear Dr Ledgerwood

**Re: MS: 1267989645154627 Driving behaviour in adults with Attention Deficit/Hyperactivity Disorder**

Thank you for considering our manuscript for publication in BMC Psychiatry. We were pleased to receive the reviewer’s comments and we feel that the manuscript has improved significantly as a consequence of the points they raised.

We now submit a revised version of the manuscript and we hope it will be clear that we have considered each of the reviewers’ comments very carefully. In general we have made the changes suggested by reviewers, including a fuller consideration of previous research and how our findings fit with these studies and more information about recruitment procedures and sample composition.

Our responses to each of the reviewers' comments are shown below. Changes to the manuscript are shown in bold type and the relevant page numbers are referenced below.

We look forward to hearing from you in due course.

Yours,

Madeleine Groom, PhD

**Assistant Professor in Applied Developmental Cognitive Neuroscience**
Reviewer 1 (Weafer)

1. Several previous studies have examined simulated driving performance in adults with ADHD (some citations below). The introduction would benefit from a review of the findings from previous studies, as well as specific mention of how the current findings will build upon the existing literature.


We thank the reviewer for providing these references. We have revised the introduction and discussion to provide a fuller consideration of previous research in the field and we have provided a fuller explanation of how our study builds on this work, on pages 4-6 (introduction) and pages 16-18 (discussion).

2. 17 of the 29 participants with ADHD enrolled in the study were taking stimulant medication, but it is not clear what number of the final 22 participants included in analyses were taking medication. Did the results differ between those participants prescribed medication and those who were not? Were there any differences in symptom severity as a function of medication?

We agree it is important to include full information about medication in the ADHD group. The description of group characteristics in the results section (page 13) now includes the number of participants on medication in the final sample. There were insufficient numbers of patients not prescribed medication (n=6) for statistical analysis. However the mean CAARS score for those taking medication (mean = 82.25) is very similar to the mean score for those not taking medication (mean = 83.17), suggesting these sub-samples are equivalent to one another. This information is now provided in the Results section on p.13.

3. The description of the driving simulator (section 2.2) provides information for the observational measures only. The dependent measures derived from the driving simulator (i.e., average speed, the proportion of the distance travelled in excess of
the speed limit, coefficient of variation of velocity, and standard deviation of lateral position) should be included here as well. *In our original submission we reported these measures within the data analysis section. However we agree that this information is better presented in the description of measures and we have moved it to section 2.2.1, page 8-9.*

4. The discussion should include a section that compares the current findings to those from previous driving simulation studies in adults with ADHD (see Comment #1) and offers potential explanations for any discrepancies in findings. *We have expanded our consideration of the links between our findings and those of previous studies. (see response to comment 1).*

5. Indication of significance of comparisons should be included in Table 2. *F values and significance levels have been added to Table 2. All statistical results presented in Tables have been removed from the text to avoid duplication of information.*
Reviewer 2 (Garner)

1) The contribution of this manuscript to the existing literature is not self-evident. Several studies have documented that significant differences exist between individuals with and without ADHD with regard to driving histories as well as performance in driving simulators. In addition, the authors note that a previous study has examined the role of drive type (urban versus motorway) in ADHD and non-ADHD samples and that a separate study examined differences in eye movements among individuals with and without ADHD. One area that seems relatively understudied, and could potentially be a novel aspect of the current study, is the examination of verbal expressions of frustration/anger across groups during driving. However, the introduction does not adequately address why group differences in frustration/anger were assessed in the context of driving. In summary, the contribution of this manuscript to the literature would be strengthened if the authors described the existent literature in greater detail by identifying weaknesses in the methodologies of the studies reviewed and stating how their manuscript addresses these weaknesses.

We agree that the manuscript would benefit from a fuller consideration of how this study further develops previous work in the field. To this end, we have now highlighted several aspects of the study aims and design in the introduction section to make it clear how we are further developing this work and also how we are overcoming the limitations of previous studies. Specifically, we now emphasise the importance of using a driving simulator rather than a desktop computer to assess driving performance (page 5). We also explain, on page 5, our reasons for measuring verbal expressions of negative emotions; specifically, that ADHD is associated with emotional dysregulation which can lead to greater tendency to express frustration and anger. On page 5 we also highlight our decision not to use financial incentives linked to performance as these may mitigate impairments in driving performance by enhancing motivation in the ADHD group. Finally, we have pointed out on page 6 that the only previous study to measure eye movements in the context of driving in ADHD was under-powered and did not use an in-car simulator. We believe these revisions show how our study design extends previous research.

2) More information is needed regarding procedures for establishing a clinical diagnosis of ADHD. What were the credentials of the individual diagnosing ADHD participants (e.g., clinical psychologist)? Was a structured or semi-structured clinical interview conducted? Who was the reporter for the interview (self-report, parent, significant other)? Where rating scales collected and used to determine diagnosis? Also, the authors note that individuals in the ADHD group met lifetime diagnosis of ADHD did they also meet diagnostic criteria based on current symptom presentation? This information is needed in order to determine whether the interpretation of results is appropriate. For example, it may be that differences between individuals with and without ADHD in anger/frustration during driving are only apparent in samples with a current diagnosis of ADHD versus those who have a history of ADHD but are not currently symptomatic.

We apologise for omitting this information in the first submission. ADHD participants were recruited from an adult ADHD clinic run by CH, a Consultant Child and Adolescent Psychiatrist with extensive experience of ADHD assessment and
diagnosis across the lifespan. Diagnoses were therefore already established prior to study participation. In this clinic lifetime and current ADHD diagnosis is assessed by the Diagnostic Interview for ADHD in Adults (DIVA 2.0) and by the Conners Adult ADHD Rating Scale (CAARS observer- and self-report). All participants met criteria for current as well as lifetime diagnosis as evidenced by the high CAARS scores (T score>80). This information has been added to page 6-7.

3) More information about the sample is needed. In particular, what was the average age, years driving experience and frequency of driving the overall sample? Did the two groups differ on any of these variables or other relevant demographic variables such as gender? If the groups differed on any of these variables then they should be covaried in subsequent analyses. Also, I am a bit concerned that the control group was not adequately screened for absence of ADHD. The authors report that controls were eligible if they had never received a diagnosis of ADHD but is there the chance that individuals in this group could meet diagnostic criteria at time of participation? Did the two groups differ on the Conners’ Adult ADHD Rating Scale (and Autism Quotient for that matter) as one might expect? If they did then this information would help alleviate some of my concern.

We agree these demographic and driving history variables are important. These are presented in Table 1 in addition to group comparisons on each measure. The groups did not differ in age, driving experience (number of years since passed driving test) or frequency of driving (number of hours per week).

Participants in the control group completed the CAARS and were excluded if they scored above the recognised screening cut-off point for ADHD (score >65 on the CAARS ADHD Index). 5 such individuals were excluded from the study, as reported in the methods on page 7-8.

4) More information regarding the psychometric properties of the measures used in the study is needed. This information is critical for determining the scientific rigor of the study. The authors should include psychometric properties of both commonly used scales (e.g., Conners’ and Autism Quotient) as well as the Linguistic Inquiry and Word Count text software.

This information has been added to page 7 (CAARS and AQ) and page 10 (LIWC).

Minor Essential Revisions
1) The authors are encouraged to change the first line of the abstract as it is misleading. The authors seem to suggest that they will assess how ADHD-related cognitive impairments impact driving but there are no measures of cognitive functioning in the study. We used the term ‘cognitive’ because we measured eye movements, which are known to index shifts of attention, and reaction time to timed events, an index of attention, motor preparation and motor execution.

2) The Data Analysis section is difficult to follow because there are so many planned analyses. The headings used in the Results section are very helpful for orienting the reader. It is recommended that the same headings be used in the Data Analysis section.
Thank you for this suggestion. We have re-structured the Data Analysis section (page 11-12) and added further sub-headings to help orient the reader to the different analyses.

3) Analyses assessing the correlation between ADHD symptom dimensions and driving variables. The authors need to clarify whether the entire sample was used for this analysis or whether only the ADHD sample was used. I would argue for using the entire sample because this will increase the variability in responses to symptoms of inattention and hyperactivity/impulsivity. If the authors used the ADHD group for these analyses, the inattention variable would likely have very little variability because the sample would likely have high levels of inattention. Thus, lack of variability in inattention symptoms may be reason that inattention was not correlated with driving outcomes. Symptoms of hyperactivity/impulsivity likely have greater variability because of the developmental progression of ADHD (a general decrease in symptoms of hyperactivity/impulsivity). I would recommend that the authors re-run analyses with the entire sample if they have not done so already. Alternatively, they should examine the distribution of the inattention variable and comment on this as a potential limitation in the Discussion.

We agree this was not well described in the original submission and have now clarified (on page 12) that the correlations were restricted to the ADHD group. This approach was chosen because we wanted to examine relationships between variability in symptoms and driving performance in the ADHD group alone. Due to the non-random selection of samples (ADHD, non-ADHD), the distribution of scores on the CAARS sub-scales would be bimodal, and would therefore essentially reflect the already reported group differences on each measure. We felt that this natural grouping of scores on the CAARS would simply act as a proxy for our grouping variable and would therefore not provide any new information over and above the group comparisons. Moreover, from a theoretical stance, we wanted to first establish where the impairments lay in the ADHD group and then understand what drives these impairments.

We appreciate the suggestion to check the distribution of the hyperactivity-impulsivity scores and the inattention scores to make sure differences in the degree of variability between variables are not influencing our findings. We checked this visually using box-plots, shown below, which show that the two variables show a very similar distribution in the ADHD group with slightly greater variability for the hyperactivity-impulsivity measure. The inter-quartile range was also similar, 15 for hyperactivity-impulsivity and 17 for inattention. This increases our confidence that the reported correlations are not influenced by differences in variability between measures. We have given a brief account of this on page 18 in the discussion.
4) Please provide F-statistics and p-values for non-significant findings as this information is helpful for meta-analyses.

Statistics for non-significant findings have been added to Tables 2 and 4. Statistical results that are now presented in tables 1 to 4 have been removed from the text to avoid duplication of information in tables and text.