Comments to the Author

Review of the preprint "Are the more flexible great-tailed grackles also better at behavioral inhibition?"

This is an interesting manuscript which can improve our understanding of a topic critical to behavioral ecology – the role that inhibition may (or may not) play in the evolution of behavioral flexibility. The authors investigate this by examining relationships between grackle performance on two inhibition tasks and two flexibility tasks. I recommend that this manuscript be published following major revisions. My main concerns include: 1) the effects of an outlier individual who was accidentally measured for too many trials; 2) the organization of the Results section; and 3) the lack of explanation/discussion of one of the significant findings. My recommendations/questions are outlined below by section:

Throughout:

- When discussing the relationship between inhibition and flexibility, I recommend mentioning them in this order since it follows the order of proposed causation (e.g., Lines 33-35, 54-55, 91, 147).

Abstract:

- Lines 37-42: These two sentences provide the same information – one could be cut to shorten the abstract.
- Lines 45-46: The authors should mention that the inhibition tasks did not correlate with each other before discussing their correlations (or lack thereof) with the flexibility measures.

Results:

- The way this section is currently organized is confusing. I recommend the author put all Prediction 1 sections under the same heading, with subheadings indicating the comparison being made (e.g., detour v reversal learning, # go/no go trials v multi-access box, etc.).
- Variables can be more clearly defined throughout.
- Prediction 1, go/no go and Reversal Learning --
    - Line 150 – Please provide p-values and/or slope estimates for "positive correlation" (even though they are provided in Table 2).
    - I am very concerned that the reported relationships would not hold if the individual who was accidentally tested too many times is removed -- doesn't seem like they would from Figure 1? Although this is acknowledged in Lines 206-207, the authors should provide additional analyses excluding this individual (or capping their # of trials).
    - It should be made clearer how a negative relationship between the number of go/no go trials and reversal learning rate (Lines 166-168) confirms the finding of a positive relationship between the number of go/no go trials and number of trials to reverse a preference (Lines 150-151).
    - Lines 194-200 – this paragraph is not a result and should be moved to the "deviations from preregistration" section as justification for the new cutoff.
    - Lines 201-294 – These summary statistics should come earlier in this section.
- Prediction 1, go/no go and Multi-access box
    - Lines 224-227 – The lack of correlation between flexibility measures is a separate result and should be reported in its own section.
    - Table 3 – what are the '-0.00' coefficients?

Discussion

- This section is missing a discussion of the possible drivers of negative relationship between go/no go task and multi-access box performance. Why might this be the case, especially since there is no relationship between the flexibility measures (Lines 224-225)?

Methods

- Why was there no prediction about the relationship between flexibility measures?