From: **Matthew Crossley** matthewjohncrossley@gmail.com
Subject: Fwd: Submission XLM-2016-3063 - [EMID:1adaa610194f1191]
Date: December 24, 2016 at 2:00 PM
To: Greg Ashby greg.ashby@psych.ucsb.edu, Todd Maddox wtoddmaddox@gmail.com

MC

Certainly not the worst reviews ever.

Thoughts?

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Begin forwarded message:

From: "Robert Greene" <em@editorialmanager.com>
Subject: Submission XLM-2016-3063 - [EMID:1adaa610194f1191]
Date: December 24, 2016 at 4:00:32 AM PST
To: "Matt Crossley" <matthewjohncrossley@gmail.com>
Reply-To: "Robert Greene" <rlg2@case.edu>

CC: experimental.lmc@gmail.com, luis.jimenez@usc.es

XLM-2016-3063
Detection of Feedback Contingency Depends on Declarative Systems
Journal of Experimental Psychology: Learning, Memory, and Cognition

Dear Dr. Crossley,

Thank you very much for submitting your manuscript "Detection of Feedback Contingency Depends on Declarative Systems" for review and consideration for publication in Journal of Experimental Psychology: Learning, Memory, and Cognition. I sincerely appreciate the opportunity to review the manuscript. I have received comments on your manuscript from three experts in the area, and am able to make an editorial decision at this time.

For your guidance, reviewers' comments are appended below. As you can see, all reviewers agree in valuing the potential contribution of the study, but they point to very different sources of concern, and differ widely in their final recommendation. Reviewer #3 is particularly satisfied with the way in which you describe the rationale and methods of the study, but still raises concerns with respect to your discussion section. In sharp contrast, Reviewers #1 and #2 take issue with the way in which you introduce and motivate the study, and asked for a more clear and concise motivation, which should allow the paper to stand alone without continuous references to Crossley et al. (2013).

My opinion, I'm afraid, is more in line with the latter Reviewers, but I also agree with the requirement of Reviewer #3 to streamline the discussion and avoid tangential topics. In short, I believe that there is a strong message to be conveyed from the main prediction of the study: if a dual task intervention may increase the impact of some form of learning (even if this learning refers to the unlearning a previously acquired response tendency) this could be a very influential report. However, I think that there is a long way ahead before this manuscript could yield an acceptable status. By now, my decision is to reject this version of the manuscript, but to allow a resubmission if you think you might be able to take the following steps:

1. Give the paper a much more self-contained and concise format as a Research Report. Your writing must reflect the fact that the study is a conceptual and empirical addition to Crossley et al. (2013), and thus it should make explicit all the arguments needed to understand the model and its predictions, but it should also exclude all the extra information that is not essential to understand the motivation of the study (such as the figures corresponding to already published results).

2. Argue convincingly, if it is at all possible, about the extent to which the present study is consistent with your own predictions. If I understand something about the rationale of your study, it is aimed at understanding why relearning a procedural task after having experienced a period of extinction proceeds faster than the first learning process. However, if the results observed in this procedure just fail to show those savings, then, no matter how eloquently you could speculate about the role of fatigue in dual-task conditions, I guess that this just shows that the procedure has failed. My recommendation in this case would be to go back to the lab, remove the sources of this contaminating fatigue, and conduct better experiments.

3. Revise procedure and analyses: In addition to facing the most serious concern about the lack of savings and its potential causes, there is also another couple of problems concerning the dual-task procedure and the statistical analyses. As for the dual task, I'm worried that you might have included in your sample a large series of participants who performed the Stroop task only slightly better than expected by chance. As judging from the histograms, there were many participants in your samples who performed below 80 % of accuracy, which could be taken as a reasonable limit if participants are really complying with the described instructions, and there is still a number of participants who performed at chance. This is plainly unacceptable if you want to assume that participants are performing this secondary task.

Finally, you must exhaustively revise the statistical analyses reported in the study, especially with respect to the reported the degrees of freedom. I failed to understand how you conducted your statistical analyses, but my computation of the degrees of freedom corresponding to the reported designs is very different from those reported in the manuscript. I'm afraid that your inferences may be unwarranted if your computations are mistaken.

In sum, my opinion is that your study could make an impact in the field if it could successfully convey its purported conclusions. Howerver, I think that the manuscript is currently too far from achieving this goal. If you decide to submit a revision, I would probably ask some of the

think that the manuscript is currently too far from achieving this goal. If you decide to submit a revision, I would probably ask some of the same reviewers to consider your revision, but I would like to warn you that allowing a revision does in no way prejudice the final outcome of the process.

To submit a revision, go to http://xlm.edmgr.com/ and log in as an Author. You will see a menu item call Submission Needing Revision. You will find your submission record there.

Sincerely,
Luis Jimenez, Ph.D.
Associate Editor
Journal of Experimental Psychology: Learning, Memory, and Cognition

Reviewers' comments:

Reviewer #1: This paper reports an experiment adding an interesting link in a chain of evidence that this accomplished group is building in their attempt to determine how procedural knowledge (such as bad habits) can be unlearned. This line of research is elegant, being both rich in theory and also important for addressing real-world problems. The aim of the present paper is to show that detection of feedback contingency during an intervention (which their earlier work has suggested to be essential for unlearning of the original procedural knowledge) is based on declarative mechanisms. The current manuscript offers intriguing evidence that this is the case, but I have some general concerns about the present manuscript that lead me to question whether it can stand alone as a JEP paper.

1. One main concern is that at present the logic in this paper is hard to follow. I say this, being very familiar with much of this group's earlier work on Rule-Based and Information-Integration learning, though I had not read the series of Crossley studies on which the current paper builds. In fact, after reading the current manuscript, I went back to look at the Crossley JEP:General paper to help me grasp this one. We can't expect readers to do this, though, so the logic needs to be spelled out early here in a way that is self-standing from the original. Examples of sources of confusion for me:
(a) The key concept of feedback contingency is not defined clearly or consistently. The abstract states: "feedback contingency, defined as the degree of non-randomness between response and outcome, plays a vital role in controlling a gate that normally prevents knowledge from being modified during interventions".   As I read this for the first time, I wondered whether this meant actual feedback contingency or detected feedback contingency. The title suggests the latter, which seems to be what is actually meant, but that distinction is not stated clearly in the abstract, nor the first paragraph of the intro, nor, I think, through most of the text. In fact, adding to my confusion, feedback contingency is defined in two different ways in this paper, e.g., in the abstract "the degree of non-randomness between response and outcome" vs. in the discussion (p. 15) "the correlation between response confidence and response valence." These don't seem equivalent.
(b) Some statements seemed very general and vague and felt like double negatives. For example, I had trouble with the abstract's statement that increasing the cognitive load, ""should disrupt the accurate estimation of contingency, and thereby prevent the gate on procedural learning from closing." It seems it would be more direct to end with "thereby keep the learning gate open, so that the original habit can be unlearned."
(c) Much of the introduction describes the Crossley paper, and yet it is not clearly stated where that is the case (i.e., where the Crossley paper is being described vs the present experiment).   Just one example of this is at the beginning of the last paragraph on the first page of the introduction: on first reading I wasn't sure that this paragraph was still referring to Crossley et al. And Figure 3 shows Crossley data, but the Figure caption doesn't indicate this. Perhaps subheadings would help to differentiate the Crossley paper findings from the current study.
(d) In addition, right now the second paragraph moves into details of Crossley's evidence without making the broader picture clear.   The description of how the model evolved (in the first few pages of the intro) is difficult to follow, because the first-time reader doesn't know where we are headed or why. I think the paper needs to begin with a clear, concise, specific statement of the underlying hypotheses about unlearning (or overwriting) that are being tested here, before going into the details of the evidence in the earlier Crossley work that led to these hypotheses.

2. Another general concern is that the present paper, while offering intriguing data, seems to be missing some key components that would provide clearer evidence for the conclusions that are being drawn. For example, a key assumption underlying the conclusions drawn here is that the original category learning is not itself declarative. There is certainly evidence from the earlier work of this group that that is the case, but providing direct evidence for that here (using the current dual task) would strengthen the paper. Thus, it seems important to include a condition in which the dual task occurs throughout training; the dual task should not affect the original category learning, even though it is influencing contingency detection during the intervention phase. As another example of missing conditions, we don't have some of the clearest evidence regarding the extent to which unlearning has occurred because the present study lacks New Learning groups (of the sort
included in the Crossley 2013 paper). Thus, the present experiments are more like an addendum to the Crossley 2013 paper, than a freestanding study, and so they provide a more modest increment in evidence than is typically expected for a JEP paper. All of this would, perhaps, be one thing if the present paper could be framed as a brief report, but given the complexity of the argument, at least as presented in this manuscript, this doesn't seem feasible.

3. Related to point 2 above, as the manuscript indicates (page 14), some of these results are unanticipated in light of this group's earlier work, e.g. the lack of robust savings in any group here. That is, contrary to the idea (and their earlier findings) that random feedback during intervention leads to no unlearning (i.e., to savings of the original learning), in fact none of the present groups, including the control, showed such savings. Some plausible explanations for these unanticipated results are offered, but this situation seems to make those missing conditions mentioned in #2 more problematic.

Minor points:
1. On page 6, is the paragraph concerning the mixed-feedback really necessary here? It seems the goal here should be to present only as much of the detail of the original Crossley paper as is needed to set up the present study.

2. On page 9, end of middle paragraph, I think that the trial numbers indicated here for condition 4 (400-650) seem to be inconsistent with what was said in the middle of page 7?

3. Bottom of page 11: It is stated that in Fig 5, the red lines are always below the blue line during reacquisition. This does not seem to me to be true of Fig 5D.

4. Page 14, summary: Contrary to what the third sentence here says, this paper does not investigate the "neural mechanisms" in any direct way, so I think better to not overstate.

Reviewer #2: This manuscript presents an interesting experiment aimed at testing the effect of feedback contingencies on relearning and savings. The authors hypothesized that estimating feedback contingency is an executive function, and that therefore a dual task condition would interfere with accurate estimates of feedback contingency. The results support the hypothesis. This is a very interesting topic, and the experiments were well-conducted. However, there are some issues with the clarity of the manuscript and the data analysis that need to be addressed before it is ready for publication. Below are some more specific comments.

1. The introduction is very confusing. There is a lot of material that is explained in a very short amount of space. I am not sure why the authors go into so much details of Crossley et al. (2013) and why they even include figures of past results. I would suggest removing the figures and many details. Also, breaking the introduction into subsections might help to streamline the presentation. The authors might also cover work that was not performed in their lab.

2. In Fig. 2, what is SPN?

3. p. 6 "More specifically, when the feedback is random the correlation between response confidence and feedback valence is zero."

This is true, but there are other cases when the correlation between confidence and feedback is zero. For example, when one begins a task and has no idea how to proceed. Confidence is near 0, and some of the responses are correct (randomly). Why are the TANs not closing the gate?

4. p. 12: The number of df in the analyses are uncharacteristically large. Did the authors use each trial as a df instead of averaging the data by subject (e.g., t-tests with over 900 df)?

5. p. 12 "The Condition × Block interaction was also significant [$F_{(4, 2598)} = 14.64$, $p < 0.001$, $\Omega 2 = 0.02$], reflecting the slower change in performance in the dual-task conditions relative to the no dual-task control."

The authors would need to decompose the interaction in order to make that claim.

6. p. 14 "...whereas savings in the other dual-task conditions were all marginally less than in the control condition..."

The p values are all >= .1, and some even > .2. It is misleading to consider them as "marginally" less. As the authors mentioned in Footnote 1, these tests should also all be corrected for multiple comparison -- so your significant result might also not be significant.

7. p. 14: I would suggest removing the regression model since it is not bringing a new understanding of the data and further increases the number of statistical tests on the same data.

8. p. 15: "Specifically, our goal was to determine whether prefrontal-based declarative memory mechanisms mediate contingency estimation."

Similar claims are also made on p. 19:

"Our results indicate that prefrontal networks likely do play an important role in controlling the estimation of feedback contingency, and therefore may provide an accessible cortical target for electrical or magnetic intervention."

The experiment supports the hypothesis that estimating contingencies depends on executive functions (or a declarative mechanism), but there is no direct test of the role of the PFC in the estimation of contingencies so I suggest removing these claims from the paper.

Reviewer #3:
In this manuscript the authors test the hypothesis that declarative mechanisms are responsible for signaling feedback contingency, which is used to control whether previous procedural learning is protected from unlearning. The argument is based on previous models developed by the authors that propose a critical role for striatal TANs in controlling whether striatal synapses continue to be plastic (and therefore unlearning can occur), or prevent plasticity (thereby protecting previous learning). This strong theoretical basis in a well developed computational model is a strength of the paper.

I did not have significant concerns with the experiment. The primary manipulation (use of a numerical Stroop task) is appropriate. The statistical analyses are appropriate and complete.

I thought the Introduction was particularly well written: the authors successfully laid out the logic of the study, which required integration across neural, computational, and prior behavioral work.

My main concerns are with the Discussion. It lacked a clear conclusion and "take-home" message for the reader. In addition most of the

discussion was taken up by two somewhat tangential topics (lack of savings and the procedural nature of category learning). The discussion should be broadened, and the treatment of these two topics condensed.

APA asks that you please take a moment to give us your feedback on the peer review process as you experienced it, by completing a short survey, available at http://goo.gl/forms/qzKP6Zkqx9.