From: **Robert Greene** em@editorialmanager.com
Subject: Submission XLM-2016-3063R1 - [EMID:6a5de7ca4fd5eace]
Date: October 16, 2017 at 10:31 AM
To: Matt Crossley matthewjohncrossley@gmail.com

RG

CC: luis.jimenez@usc.es

XLM-2016-3063R1
Estimation of Feedback Contingency Depends on Executive Function
Journal of Experimental Psychology: Learning, Memory, and Cognition

Dear Dr. Crossley,

Thank you very much for submitting your revision of the manuscript "Estimation of Feedback Contingency Depends on Executive Function" for publication in Journal of Experimental Psychology: Learning, Memory, and Cognition. I sincerely appreciate the opportunity to consider this revision. I have now received comments from the same team of reviewers who considered the previous submission, and am able to make an editorial decision at this time.

Reviewers have now commented on your paper. As you can see from their comments, which are appended below, two of the three original reviewers are now pretty satisfied with the revision. The other reviewer, however, is not equally satisfied, and still raises issues concerning what is seen as a lack of conceptual clarity, both at the abstract and at some other points over the introduction and discussion sections.

I believe that the main message of the experiment is strong and compelling, and that a paper reporting on these results could eventually deserve publication in JEP:LMC. After all, it is one of the very few experimental examples that shows some conditions in which learning can be facilitated by distraction, at least when this learning involves unlearning about previously acquired procedural tendencies. However, I tend to agree with Reviewer#1 in the impression that the message could have been transmitted more clearly over the abstract, and the introduction sections, and I also endorse Reviewer#1 diagnostic with respect to the need to mention and discuss the limitations of the study, specially with respect to the lack of significant differences between several conditions, which, according to your hypotheses, should have produced a difference.

If you undertake this subsequent revision seriously, I hope to be able to take a final decision myself, without asking for more external reviews. I would like to encourage to do so, and to submit your revised manuscript together with list of changes or a rebuttal against each of these points.

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 revision is clearer than the original—the introduction is greatly improved--and the results more convincing (e.g., Figure 6 is very nice). Very interesting findings! However, I do have remaining concerns as follows.

(1) It is still very hard for people like me who have not been immersed in this line of research to keep the argument straight. Could the underlying argument not be presented more simply and consistently in the abstract and introduction? As I think I understand it, for the present experiment (e.g., page 7) the argument is that detecting that feedback is random prevents unlearning, whereas not detecting the randomness enables it. If this is the case, could this not be stated more clearly in the abstract and earlier on? Particularly in the abstract, upon first reading the argument wasn't making sense to me. First it is stated that "our results suggested that modification of procedural knowledge (note: which I take to be unlearning of the original habit) is possible only if feedback contingency is high." This sentence led me to anticipate that unlearning should be facilitated by being able to detect a contingency during intervention. So this seemed on the surface to contradict a subsequent statement that "increasing cognitive load during intervention via the concurrent performance of an additional task should disrupt the accurate estimation of contingency, thereby keeping the gate on procedural learning open." Although the reasoning becomes somewhat clearer in the introduction, it all still seems more convoluted than would be ideal, and I'm still not sure I've got it right. (Or perhaps I've just got my own mental block on this—I'm feeling dense!) Would it help to try to cut down on the number of terms I wonder--for example, using "unlearning" more consistently instead of alternatives?

(2) The new discussion is very unsatisfying and oddly removed from the results. There is no explicit mention of limitations of the present study, nor any consideration of the implications of some of the findings. For example, what is the implication of the findings (see bottom of page 13) that there was no evidence of dose dependency or of any influence of the overlap of the dual task from acquisition to intervention? Although I am the reviewer who raised the question about the procedural nature of the original learning, given that this is a brief report, I don't think it is appropriate to take up half of the discussion reviewing the earlier evidence that it is. Instead I'd suggest the lack of direct evidence here might be acknowledged as a limitation, but a sentence citing earlier work that it is procedural be included.

(3) More minor considerations: (a) The term "II" is not defined before its first use (page 3) nor is RB (page 6). Both are defined only on page 14. (b) Figure 2 is referred to in the text after Figure 3 is.

Reviewer #2: This manuscript presents an interesting experiment on how feedback contingency is estimated and used to allow or prevent the learning of stimulus-response associations. I was a reviewer on the original version of this manuscript. The authors have been successful at addressing my earlier concerns, and I believe that the manuscript is now adequate for publication in JEP:LMC. I am listing below some minor suggestions that I would encourage the authors to consider before publication but I will leave these up to them:

Figure 3 is called before Figure 2. Please re-number figures.

It might be useful to give more meaningful names to the conditions instead of Condition 1, Condition 2, etc. I found myself going back and forth in the manuscript to remind myself of what the conditions are. Maybe something like Prior/100, Prior/200, ... Post/100 (with the word indicating if the dual task was introduced before or after the treatment and the number indicating the number of dual task trials).

p. 11: For the intervention ANOVA, why did you pick 4 blocks? Why not 3 or 5?

p. 12, ln 3: You begin your sentence with "First..." but there is no "Second...". Consider revising.

Reviewer #3: I am satisfied with the revisions and think the manuscript is ready for publication.

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.