Response to Reviews:

Criterial Learning and Feedback Delay: Insights from Computational Models and Behavioral Experiments

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Reviewer 1

Reviewer comment

The research question of the study is to investigate the effect of delay in criterion learning in category learning. Experiment 1 shows that in a continuous criterion learning task with known relevant dimension delaying the feedback but not increasing the ITI decreases performance. In other words, delaying feedback makes it more difficult to learn the position of a continuous criterion. Experiment 2 uses many binary dimensions in the category learning task and participants' goal is to learn the binary dimension. In this task, delaying feedback does not decrease performance. The researchers simulate the performance of three different models and show that a model that assumes the criterion is maintained in working memory with information drifting across time cannot predict the observed effect. Two models that make different assumption can predict the observed pattern.

Author response

We thank the reviewer for their careful reading of our manuscript and their thoughtful comments. We address each of the reviewer's points in turn below.

Reviewer comment

I am no expert in category learning, so cannot adequately judge the novelty of the observed results. Given this caveat, I really enjoyed reading the manuscript and felt it made a convincing argument using a rigorous and state-of-the-art methodology. Using parameter-space partitioning is an excellent and sadly underutilised way of comparing models. It goes much beyond the typical approach of comparing models in terms of their model fit and instead allows deriving qualitative differences between the candidate models. In addition, the two experiments are well designed and executed and provide the necessary empirical basis on which the models are compared. The paper is also overall clearly written. Taken together, I highly recommend this paper to be published after addressing a number of mostly minor concerns detailed below. My main issues with the current version relate to the presentation of the models and modelling results and whether the space of possible models is sufficiently explored.

Author response

We are grateful for the reviewer's positive assessment of our work and for their constructive suggestions.

Reviewer comment

1. Exploration of model space: When reading the description of the models it was not entirely clear to me whether the three presented models explore the space of possible models to a sufficient degree or whether there might be some models missing from consideration that could change the overall message. For example, from a theoretical point of view the main difference between the time-dependent drift model and the delay-sensitive learning model is that for the latter "the criterion remains stable over time and does not drift". However, when looking at the model descriptions more aspects seem to differ than solely the presence of drift, namely the presence of the feedback delay in the updating rule (Eq. 2 vs. 6). Is the change in the updating rule necessary for the models to just differ in the noise in the criterion or is it part of it? I am not sure if I just do not understand the difference or if a hybrid model with drift and feedback delay in the updating rule is possible. Likewise, are any of the assumption in the reinforcement-learning model, which seems to differ quite a bit from the previous models, somewhat arbitrary or are these always the only possible choices?

I am not asking for an extensive exploration of the model space using additional simulations, but some descriptions of the extent to which the presented models explore the possible model space or if there are other possible models that could potentially lead to different results is needed.

Author response

We thank the reviewer for raising this important point. We have used this as an opportunity more systematically explore the classes of possible models. In particular, we explore two classes of models: one class that assumes a criterion is learned and one class that assumes SR learning with no criterion. Within the criterial-learning class we explore three different binary assumptions: perceptual drift (yes or no), criterial drift (yes or no), and delay-sensitive updating (yes or no). Within the SR learning class there is no criterion to drift, but we do explore perceptual drift and delay-sensitive feedback (yes or no on each).

Reviewer comment

2. Simulation results figures: There were some aspects of the results figures for the simulation that I found difficult to understand:

(a) Even after reading the full paper and thinking about it for a while, I do not understand what panels B are supposed to show. Does this only show the subset of the parameter space in which one of the two relevant patterns was observed? If not, shouldn't there be data points everywhere?

(b) I do not understand why in Figure 1, for example, panel C shows some cases in which the predicted qualitative pattern is either "control impaired" or "other", but there is no corresponding bar in panel A.

(c) The bars in Figure 2A are smaller than in Figures 1A and 3A. Why?

Author response

With the new approach to model exploration, we have heavily revised the simulation results figures. We hope that the new figures are clearer and address the reviewer's concerns.

Reviewer comment

3. Calling the simulation Experiment 1: I found it extremely confusing that the simulation using parameter-space partitioning was called "Experiment 1". If I read "Experiment" I expect to see some empirical data obtained from participants and not solely a simulation. I suggest renaming this section to something clearer. For example "Simulation Study" or "Parameter-Space Partitioning Across Models".

Author response

We thank the reviewer for this suggestion. We have made this change in the revision.

Reviewer comment

4. Outliers in Exp. 2:

- While I think the argument for removing outliers in Exp. 2 is fine, the actual criterion defining an outlier is not actually given (which feels a bit ironic in a paper about criteria).

- How do the results look with these outliers included? Does the difference between condition vanish? Adding a footnote or so should be enough.

- The fact that some participants did not even learn a single criterion feels very surprising to me. Is this common in such experiments? Please add a sentence embedding this finding within your general experience running such experiments.

Author response

We thank the reviewer for the opportunity to clarify our method. Outliers were identified on the basis of the Gaussian mixture model fit to the trials-to-criterion data. We now say "Participants that were deemed to most likely belong in the low-performance Gaussian by this analysis were excluded from further analysis."

Reviewer comment

Minor points:

- Does Eq. 13 show results for trial n or n + 1? The text and equation are inconsistent.

- The description regarding the randomisation of Experiment 2 on page 12 (paragraph before "Procedure") seems to be inconsistent with Figure 4. Is the dimension picked at random for each trial or always the same for certain problem numbers (as implied by Figure 4)?

- In figure 4, the top says Problems "1 thru 13", but only shows 7 problems.

Author response

We thank the reviewer for catchign these points. All are corrected or revised for greater clarity in the revision.

Reviewer 2

Reviewer comment

Your action editor asked me if I could provide a review, as they have only been able to secure one reviewer after a considerable delay. Although I am editor in chief for the journal, I am acting in the role of a reviewer in this case. The action editor retains full authority to determine the decision outcome for this manuscript.

Author response

We thank the editor in chief for acting as a reviewer and for their careful reading of our manuscript and their thoughtful comments. We address each of the editor's points in turn below.

Reviewer comment

This manuscript reports a sophisticated approach to an important problem. I especially like the use of mathematical models.

Author response

We are glad the reviewer viewed our approach favourably.

Reviewer comment

I was concerned that the models seem to have multiple varying features. It might be more useful to isolate specific processes while holding other model elements constant. Based on the introduction, I was not clear on the theoretical motivation for comparing the time-dependent drift and delay sensitive learning models, as the latter did not seem to be just a version of the former but with delay as a relevant factor. Some additional justification and/or adjustments to the tested set of models is needed.

Author response

We thank the reviewer for raising this important point. In the revision we more systematically explore the classes of possible models. In particular, we explore two classes of models: one class that assumes a criterion is learned and one class that assumes SR learning with no criterion. Within the criterial-learning class we explore three different binary assumptions: perceptual drift (yes or no), criterial drift (yes or no), and delay-sensitive updating (yes or no). Within the SR learning class there is no criterion to drift, but we do explore perceptual drift and delay-sensitive feedback (yes or no on each). The motivation for these models and parameter choices is now more fully explained in the revision.

Reviewer comment

Despite these concerns about which models are the most relevant to compare, I was impressed by the simulation strategy and the focus on finding "signature" predictions that distinguish different pairs of models.

Author response

We are glad the reviewer viewed our simulation strategy favourably.

Reviewer comment

I think it makes more sense to have a prediction simulation section and Experiments 1 and 2 rather than to consider the simulation the first experiment.

Author response

We thank the reviewer for this suggestion. We have made this change in the revision.

Reviewer comment

The data in Figure 6 can likely be fit much more closely by an ExGaussian distribution than either the 1 or 2 component Gaussian mixtures that are shown.

Author response

We thank the reviewer for this suggestion. We now estimate both a single-component truncated ExGaussian and a two-component truncated ExGaussian mixture via MLE with proper normalization on the tasks bounded support $[9, 512]$, and compared them via AIC, BIC, and a parametric bootstrap likelihood-ratio test (LRT). The results from moving from the mixture of Gaussians used in our orginal submission to the truncated ExGaussian mixture used in the revision does not change the qualitative pattern of our results in any way. However, we agree that the ExGaussian and ExGaussian mixture better capture the skewed and bounded distribution of our trials-to-criterion data so we have adopted it in the revision.

Reviewer comment

Replication the criterial learning results from the currently-labeled Experiment 2 seems like a good idea given the fairly low sample sizes per condition.

Author response

We agree with the reviewer that a replication would be valuable but feel the data and models currently presented represent a suitable contribution to the field as is. For this reason, paired with the time and resource costs of conducting a replication, we have not pursued this in the revision.

Reviewer comment

Did stimuli remain on the screen across the feedback delay? I don't think so. If not, what happens if the stimulus reappears at the time of feedback? I could imagine this would eliminate or dramatically reduce the negative effect of feedback delay. If so, is that consistent with the conclusions? That is, would the models that predict a deleterious effect of feedback delay also predict that it should be eliminated with "reminder" stimuli at the time of feedback?

Author response

We thank the reviewer for the opportunity to clarify the design of our experiment. As shown in Fig. 5 and in Fig. 8 the stimulus was presented up until a response was made. At this time the stimulus was removed and replaced by a noise mask until feedback was delivered. We also thank the reviewer for raising the interesting question of whether the negative effect of feedback delay would be eliminated if the stimulus were to reappear at the time of feedback. We agree that this would be an interesting manipulation to explore in future work. However, we believe that their are many interesting followup questions that could be explored in future work and that the current manuscript represents a suitable contribution to the field as is.

Reviewer comment

For the currently-labeled Experiment 3, reporting fail-to-reject results for t-tests is not a valid method for establishing that feedback delay has no effect. This is especially a concern given the low participant count per condition and the fact that empirical effects go in the "right" direction (lower performance for delayed feedback). A different inferential approach is needed, and one possibility is reporting confidence intervals over effect size. Your conclusions rely on demonstrating that you can rule out meaningfully large effects (i.e., only negligibly small effect sizes are included in the confidence intervals). I suspect that the intervals will be wide and include a range of fairly big effects, of the same magnitude as the effects reported in Experiment 1. As such, this experiment does not establish that the effects of feedback delay are specific to a criterion learning task.

Author response

TODO: Greg, I'd appreciate your take on this comment.

I ran an equivalence test, and as the reviewer suspected, the data are inconclusive. I also have Experiment 2 data from an earlier semester. It shows the same pattern (though with fewer participants and more noise), but I left it out to avoid mixing cohorts. Just out of curiosity, I combined it with the current data and reran the analysis, but the results remain inconclusive. So I don't think we can make a strong claim that Exp 2 shows no effect of delay — at least not with this method.

I also tried testing whether the delay effect is larger in Exp 1 than Exp 2 (a difference-of-differences approach), but that too was inconclusive.

One possible way to address this is to add data from another archived experiment, where we examined criterion learning with a numerical Stroop dual task. Those data (n=30) showed no effect of the dual task. Including this might add some empirical value.

Thoughts?

Reviewer comment

I recommend running a bigger replication study that combines both tasks to demonstrate the claimed interaction (feedback delay impairs performance only for the criterion learning task). The replication study should be preregistered with a sample size justification based on the interaction effect defined by the across-experiment comparison in the current data. Adding a "stimulus reminder at feedback" condition to this new study might be helpful in clarifying the mechanisms producing the delay effect.

Signed, Jeff Starns

Author response

We thank the reviewer for their careful reading of our manuscript and their thoughtful comments overall. Here, we agree that a replication / extension study would be valuable but feel the paper as is represents a suitable contribution to the field.

Action Editor

Reviewer comment

The topic is made very clear in the first paragraph and notes the need for investigating how criterions are learned in decision models.

Author response

We thank the reviewer for their positive assessment of our work.

Reviewer comment

The final paragraph of the introduction summarizes the experiments. This seems more appropriate in the abstract, given that no information about the experiments are presented in the abstract.

Author response

We thank the reviewer for this suggestion. We have added more detail about the experiments to the abstract, but have elected to retain the summary of the experiments in the introduction as we feel it helps orient the reader to the structure of the paper.

Reviewer comment

Equation 1 needs some explanation, as this implies that the decision Rn is made immediately on stimulus presentation. In other words, Rn is not an outcome of some time-extended cognitive process. Am I misunderstanding equation 1?

Author response

We thank the reviewer for raining this point and for the opportunity to clarify. In essence the reviewer is correct. Equation 1 indicates that the response is made immediately upon stimulus presentation. We therefore are not explicitly modelling the time-extended cognitive processes that govern evidence accumulation and the corresponding response time. We now say this explicitly in the revision.

Reviewer comment

What was the reason behind the choice to have equation 2 only be sensitive to negative feedback? How is "optimal" defined in the current case, given that response time does not factor in the update rule?

Author response

We thank the reviewer for the opportunity to clarify this point. The rationale behind the choice to have equation 2 only be sensitive to negative feedback is that if the response is correct, then the observer has effectively gained zero information about how their criterion should be modified. Also, in this case otimal refers to the true criterion value that the observer is trying to learn. We now say this explicitly in the revision.

Reviewer comment

The use of parameter space partitioning is sensible given the many differences among the three models. However, PSP operates on non-stochastic models. In addition, the way you have approached PSP is not how the original PSP is implemented (i.e., using MCMC). This should be mentioned explicitly to avoid confusion.

Author response

We thank the reviewer for raising this important point. We have revised the manuscript accordingly.

Reviewer comment

The three-dimensional plots in figures 1B, 2B, and 3B do not come out right on 2D. You could improve these by having 2D projections on the planes. Better still, three boxplots would be more appropriate as there is no additional information by using 3D plots.

Author response

We thank the reviewer for this suggestion. We have heavily revised the simulation results figures. We hope that the new figures are clearer and address the reviewer's concerns.

Reviewer comment

Although not a focus in this manuscript, I wondered whether the models are able to account for finer-grained data patterns such as complete RT distributions and speed-accuracy tradeoff functions.

Author response

We agree that these are interesting questions. The models in their current form do not account for RT distributions or speed-accuracy tradeoff functions. However, we believe that this could be a fruitful avenue for future work.

Reviewer comment

The experiments are numbered 2 and 3, instead of 1 and 2. The models were following the methods of experiment 1, which is missing. Experiment 1 is also mentioned in the discussion separately with experiment 2. It looks like experiment 1 has been deleted from this manuscript, but the model simulations and discussion still refer to it.

Author response

We thank the reviewer for catching this confusion. No study was deleted from the manuscript. Rather, whether or not the computational modelling section should be considered an "experiment" was a point of oscillation when writing. We hope our revision clarifies this point.

Reviewer comment

In figure 6, it is clear that the participants form two subcohorts. However, the use of a Gaussian seems odd given that there can not be any negative values for trials-to-criterion. Given that the authors model this frequency histogram to exclude participants, more appropriate distributions, such as Poisson should be used.

Author response

We thank the reviewer for this suggestion. We now estimate both a single-component truncated ExGaussian and a two-component truncated ExGaussian mixture via MLE with proper normalization on the tasks bounded support $[9, 512]$.

Reviewer comment

Given the experimental results, what is the added benefit of the three models? Experiment 2 is the critical experiment showing that 3.5-0.5 leads to more trials-to-criterion than 0.5-3.5 or 0.5-0.5. The models do not provide any detailed understanding of mechanisms and they are not fitted to actual data. The general message from the experiments is that any updating rule should be sensitive to feedback delay. The models do not go beyond this.

Author response

Thank you for raising this but we respectfully disagree that our models do not provide a detailed understanding of mechanism. To the contrary, our models all offer detailed mechanistic accounts for how criterial learning may occur in cognition. They articulate that delay effects may arise from any of several distinct cognitive mechanisms including the decay of a memory representation over time, the discounting of learning magnitude by feedback delay of this representation, or the discounting of learning in a reinforcement-learning stimulus-response framework. In this way, the models contribute the most important piece: they pin down the process-level mechanism consistent with the experiments.