Decision letter

Discrimination Reversal Facilitates Contextual Conditioning in Rats’ Appetitive conditioning

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Dear Editor,

Thank you for giving us the opportunity of resubmitting our manuscript, “Discrimination Reversal Facilitates Contextual Conditioning in Rat’s Appetitive Conditioning”. We thank you and the reviewers for your thoughtful and constructive comments giving us the opportunity to revise our manuscript.

We have considered all of the suggestions and have incorporated them into the revised manuscript. As a result, the manuscript has been substantially changed, and very much improved. Changes with respect to the previous version of manuscript are written in red font. Deletions have not been marked, to facilitate reading of the manuscript. For the same reason, all the figures have been changed but they are presented in black font. Finally, the list of references has been updated, as the changes conducted involved including some new citations in the manuscript.

A detailed explanation of all the changes is presented below. We hope you will consider the manuscript suitable to be published in Psicologica.

Looking forward to hearing from you. Sincerely,
José Andrés Alcalá
I suggest including them as a full Experiment 2 section, with detailed description of Method and Results, provided that the two set of data can be collapsed (taken into account data from Phase 1 as well).

We included a full description of the second set of data as Experiment 2 (pp 18-22). Data from the discrimination training in that Experiment are presented as well (as suggested by Reviewer B).

I recommend taking into account Reviewer B’s suggestion about reanalyzing data. She/he notices that this new analysis might lead to a significant result opposite to the expected according to the theoretical framework presented in the Introduction (better CS3 conditioning in group Discrimination). If that were not the case, null results still needs to be interpreted carefully.

Data are presented as elevation ratios in the new version of the manuscript. We chose elevation ratios rather than the elevation scores suggested by reviewer B because statistical results were cleaner with elevation ratios. As expected by Reviewer B, this reanalyses led to a significant result opposite to the expected. This reanalyses led to a new discussion of the results of Experiment 1, making an emphasis in the role differential context conditioning in groups R and D play in these differences, and taking the lead to justify Experiment 2.

In order to state that your data add evidence supporting the hypothesis that reversal learning does not facilitate CS processing you may use Bayesian statistics.

As the use of elevation ratios led to significant differences, no Bayesian statistics were needed to justify the strength of a null result.

The interpretation of the difference in preCS performance found in Phase 2 (collapsed data coming from two experiments that you introduce at the end of the results section) which is dampened to some extent by the fact that a similar difference is found at the start of Phase I. Que al inicio del entrenamiento de la primera fase también se había producido un mayor número de respuestas en ese mismo grupo, lo que puede estar indicando una mayor tendencia de los animales a responder en presencia del contexto.

We expected no differences in magazine training in the absence of the CS at the beginning of training. Presenting pre-CS data together with discrimination data in the new version of the manuscript show that the differences detected at the beginning of Phase I were not consistent, and disappeared when animals begun to learn the discrimination. This was not the case in Phase II, where the differences are consistent throughout the session. The new Figure 3 gives a clearer view of this difference.

The so-called Attentional theory of Context Processing is quite poorly explained as well. I would have expected to read a more detailed explanation of the psychological mechanisms proposed in modulating attention to the context.
In page 5 we added new information about ATCP with the goal of give the more detailed explanation requested by the reviewer. We also include references to two recent reviews of the theory that discuss its benefits and problems in more detail.

- Also, some mentions to the Pearce and Hall theory are confusing. For example, it is wrongly established (p.4) that according to this theory the acquisition of a second learning erases the first. In its formalization, the theory computes separately the excitatory and the inhibitory strengths. In addition, it is not obvious how the authors have derived predictions from this model for the present conditions. Given that the context alone is never reinforced in both the experimental conditions, and given that the third stimulus is never presented in compound with other stimuli, it seems that there is no reason to expect any difference between the groups neither in context conditioning either in the rate/level of conditioning to the third stimulus.

The reference about Pearce-Hall model within the models that do not allow for survival of first learning when second learning takes place was a mistake and has been deleted. We appreciate the reviewer pointing us up to that embarrassing typo. As for how we have derived that Pearce-Hall model may be relevant for this situation, while we agree that there is not a direct prediction of the model, it is also true that the model suggests that when learning conditions become ambiguous participants tend to attend more to stimuli with uncertain outcomes, and that is basically all we say in the manuscript, that ambiguity may affect CS processing, according to Pearce-Hall model.

- It would not be necessary to introduce a control group with no training on Phase 1, to assess more fairly any possible facilitation of conditioning in the reversal condition?

The proper control group is always a matter of discussion. When selecting our control group we tried to follow the maxima of making it equal to the experimental group in everything but in that we want to measure. The advantage of the group we chose is that animals had the same experience with the outcomes and with the CSs, something that would not be the case if we would have chosen a control group with no training on Phase 1. However, we can see the disadvantages implicit in the reviewer comment, and we will take his/her comments into account in future experiments, including both, control groups with no training on Phase 1, and with training with different CSs (CS3+, CS4-).

- When presenting evidence supporting a direct relationship between uncertainty and attention (p.5), it would be fair to do some mention to the wide literature in human predictive learning (mostly by Mike Le Pelley and his colleagues) which supports just the opposite: more attention to accurate predictors than to inaccurate predictors.

We agree with the reviewer and have included a new paragraph in pp 8-9 in which we discuss the relationship between predictiveness and attention and the work of Le Pelley and colleagues. We returned to this issue in the General discussion (pp. 23-24).