

Peer Review Correspondence

Article: Rusz, D., Bijleveld, E., & Kompier, M. A. J. (2019). Do Reward-Related Distractors Impair Cognitive Performance? Perhaps Not. *Collabra: Psychology*, 5(1): 10. DOI: <https://doi.org/10.1525/collabra.169>

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Dr Christopher R Madan

University of Nottingham, GB
2018-07-04 00:39:09

Dear Ms Dorottya Rusz,

After review, we have reached a decision regarding your submission to *Collabra: Psychology*, "Do reward-related distractors impair cognitive performance? Perhaps not.". Our decision is to request revisions of the manuscript. These revisions may then undergo further peer review prior to acceptance.

All three reviewers thought the manuscript was well-written and interesting. However, aspects of the approach and background were not sufficiently described and the reviewers have provided several suggestions for substantive improvements. The full review information should be included at the bottom of this email.

To access your submission account, follow the below instructions:

- 1) login to the journal webpage with username and password
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- 4) download Reviewed file and make revisions based on review feedback
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Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please do contact us.

Please could you have the revisions submitted by October 31, 2018. If you cannot make this deadline, please let us know as early as possible.

Kind regards,

Dr Christopher R Madan

University of Nottingham

christopher.madan@nottingham.ac.uk

Reviewer 1:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?
- Does the conclusion (if present) reflect the argument, is it supported by data/facts?
- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

This is an interesting and well-motivated study that was conducted with care. The data analyses are appropriate and show very little evidence of value-modulated impairments in cognitive performance, with Bayes factors favoring the null hypothesis. The authors anticipate several of my thoughts and concerns in the Discussion. Still, the present study leaves me somewhat underwhelmed, and I think the conclusions would be substantially strengthened with some further experimentation. Publication feels a bit premature to me at this stage, but I reiterate that the current experiment was very well conducted and does add something to the literature.

Specific comments:

1. It would be very nice to see independent evidence that the test phase is sensitive to performance impairments due to distractors in some other domain, for example physically salient distractors. It is possible that I am just unaware of such evidence, as the authors do state that the task was "based on our previous work (Rusz et al., 2017)," although the citation is not included in the references list and a quick search for the first author did not produce a candidate paper. Without such evidence, it is unclear how much the null effect has to do with value vs an insensitivity to distraction in the paradigm more generally.

2. The authors are correct in that we really cannot know whether the reward manipulation was effective at all in the training phase. This would be less of a concern if the training phase involved an established task and procedure, but there are some differences that could matter for associative reward learning. One such difference would seem to be that the task is a more focused attention task with a higher working memory demand than training tasks more typically used in the value-driven attention literature, which could result in greater suppression of irrelevant features such as color and thus a reduced capacity to pick up on stimulus-reward associations.

3. Eye tracking data could be very informative here. If the authors could show reliable distraction at the perceptual level but no appreciable effect on math performance, that would go a much longer way towards arguing that value-driven attentional capture has minimal consequences for cognitive control processes. Demonstration of reliable attentional capture in eye movements would ameliorate the concerns raised in the prior two points, and would also provide an anchor point for understanding what strength of capture fails to translate to a performance decrement on the math task.

4. The distractors are not associated with a corresponding response (previously) related to the receipt of reward, which contrasts with the Krebs et al. studies the authors cite. There is mounting evidence that, under these sorts of conditions, value-driven attentional capture can have consequences for decision-making and action selection (see Anderson, 2017, *Curr Dir Psychol Sci*, for a review). This sort of situation also maps more naturally onto the real-world example the authors provide of food smell in the office (e.g., get up and approach the food vs stay at desk and continue with task). Might stronger effects been observed if the distractors were capable of activating a response that could compete with the cognitive actions needed to perform the task? The authors bring up a similar issue in the Discussion currently, but I think the issue of action associations might have consequences not only for learning (which the authors discuss), but also for what distractor-related impairment looks like in the test phase (i.e., what representations are activated by the distractor that could influence performance).

Minor:

1. There are some studies indicating that reward-associated distractors can break through and capture (spatially) focused attention (e.g., work by Munneke et al. and Wang et al.), which contrasts with the effects of physical salience on distraction (which generally does not break through focused attention). This seems relevant to the present study.

2. The authors indicate having collected several individual differences measures, but never examine correlations with these measures and performance in the task. In and of themselves, reporting the means of these measures across participants is not particularly informative, and the reader wonders why they were acquired in the first place (the authors do not even comment on the descriptives, with the exception of sleep < 6 hrs). It would be good to report these relationships, perhaps as exploratory analyses in supplemental material, to provide a more complete picture of the data.

3. What was the final sample size for each of the 3 experiments?

2) Figures/tables/data availability:

Please comment on the author's use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline

of Collabra here):

figures and tables are effective, the authors have made data and other materials available

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required.:

I did not catch reference to IRB approval, which should be added

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for improvement beyond the scope of this process.:

Yes

Reviewer 2:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?
- Does the conclusion (if present) reflect the argument, is it supported by data/facts?
- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

This manuscript describes a study investigating the effect of distractors that were previously associated with reward on 'cognitive performance'. During an initial training phase, one color indicated that reward was available; a second color was never paired with reward. In a subsequent test phase, participants saw a series of letter-number pairs (for 700ms each), and were required to add the numbers. On each trial, one of the (task-irrelevant) letters was presented either in the rewarded

or unrewarded color from training. Across three variations of the task, there was no significant evidence that the color of the 'distractor' letter influenced task accuracy, and a Bayesian analysis indicated moderate evidence for the null hypothesis. It is concluded that results suggest that reward-associated distractors do not disrupt cognitive control processes.

The manuscript is well-written and logically structured, and the topic is interesting. This was a pre-registered study, which is to its credit – statistical analyses seem sound, and I particularly appreciated the Bayesian analysis to quantify support for the null.

That said, I do have some concerns.

1. First, the article is billed as investigating the effect of reward on 'cognitive performance', but it's not totally clear what this means. The authors note that many prior studies have shown that reward influences visual attention, but "less is known about whether reward-related distractors have broader cognitive and behavioral consequences. That is, in daily life at work and school, most tasks require complex interactions with information and not merely searching for target stimuli." But imagine the current studies HAD found poorer performance on the mental arithmetic task on trials with a reward-related distractor – why couldn't this be ascribed to an effect on attention? One plausible interpretation would be that the reward-related color distracts attention away from the simultaneously-presented number, so that participants don't encode the number and hence are impaired on the arithmetic task. Indeed, the manipulation in Expt 1c (increasing the spacing between distractor and target) seems to be trying to encourage this sort of pattern (see last sentence of first paragraph on p14).

So ultimately it wasn't clear to me how, if this study had found an effect, this could be confidently attributed to an influence on cognitive control processes rather than attention / capture – it may not be the performance of arithmetic that's disrupted, it may be the encoding of the numbers. Of course the study didn't find an effect, but extending the above argument this doesn't necessarily shed light on the (lack of) effect of reward on 'cognitive control processes'.

2. As the Discussion notes, this study has some important limitations. Perhaps the biggest is that there's no evidence that the participants learned the reward associations, and if this isn't the case then of course we would not expect reward to influence performance. Indeed, the 150 training trials used here is lower than typical for studies of reward-related attention (e.g., Anderson et al, 2011, used a minimum of 240 trials training, and up to 1008). Another issue is that the relatively slow presentation rate of stimuli (700ms per item) may well be more than enough for participants to correct for and overcome distraction by the reward-related stimuli.

As noted above, both of these limitations are mentioned in the Discussion. My concern is that they render the study difficult to interpret: because of these limitations, we end up not knowing much more after this study than we did before. Reward may not influence cognitive control, or participants may not have learned about reward – we don't know which. So the conclusion on p25 ("All in all, results from this study suggest that distractors associated with reward do not disrupt cognitive control processes") seems too strong.

3. On p21-22 the possibility is raised that the effect of reward-related distractors may be relatively short-lived. On this topic, I would point the authors towards recent work using attentional blink-type procedures that has looked at the influence of reward-related stimuli. Unfortunately results are somewhat mixed: one study (Failing & Theeuwes, 2015, *Vis Cogn*) found that distraction does not dissipate rapidly, while another (Le Pelley, Seabrooke, Kennedy, Pearson & Most, 2017, *Atten Percept Psycho*) found that the effect was short-lived. There may be others.

2) Figures/tables/data availability:

Please comment on the author's use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline of Collabra here):

Figures and tables are used appropriately. The experimental task, data and analysis scripts are freely available on OSF.

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required:

There is no statement regarding approval by a relevant ethics board. It is stated that participants provided consent.

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for improvement beyond the scope of this process:

The manuscript is clearly written throughout.

Reviewer 3:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?

- Does the conclusion (if present) reflect the argument, is it supported by data/facts?
- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

Rusz and colleagues report the results of 3 experiments showing that stimulus features that had been associated with reward had no effect on a subsequent mental arithmetic task when presented as irrelevant distractors – which is at odds with previous studies observing detrimental effects of reward-related distractors. The present study is certainly a valuable contribution to the discussion of whether and when reward-related distractor effects occur. I think the study is very well written, the design is elegant, and the methods are sound. I also appreciate the pre-registration approach and the clear dissociation between planned and exploratory analyses. My main comments concern the interpretation of the results in the light of existing work on reward-related distractor effects, as well as the missing information about performance in the training phase itself.

1) In very general terms, it seems obvious from the literature that the deciding factors in the discussion about reward-related distractor effects are related to task structure and stimulus properties rather than whether the task at hand is a visual attention or cognitive control task, or whether the task is difficult or easy (which seems to be implied at several places throughout the manuscript). Most importantly, the current task did not involve direct competition of stimuli and/or responses and the task structure allowed for strategic suppression of irrelevant information in the test phase (here, letters as an entire stimulus category). Potentially, participants were able to form an attentional set for numbers and could strategically ignore the letters in a sustained fashion (regardless of the distance between target and distractor). With regard to the paradigm by Krebs et al. (which indeed qualifies as typical cognitive control tasks), the discrepancy is not too surprising to me, although I would attribute this to a slightly different source (above and beyond mere task difficulty). The biggest differences in my view are that 1) the distractor colors in Krebs et al. are associated with a competing response and distractor effects could in principle reflect response capture rather than attentional capture, or a combination of both. Moreover, 2) color as a concept is actually always relevant throughout the task, which is different from the training-test approach used in the current study, and which makes it much harder to strategically ignore certain stimuli or features. In some way, this discussion seems to relate to earlier work of Serences et al. (2005) who report attentional capture by salient (target-colored) stimuli that occur in the “wrong” location. Similar to the Stroop task by Krebs et al. (and in contrast to the current task), this task was structured in a way that a strategic suppression of these features is neither possible nor globally beneficial as they are relevant to other aspects of the task. That said, it would be very interesting to see if reward-related distractors in the current task can have an effect if training and test trial types are interleaved, e.g., in mini-blocks, which makes a strategic focus on only numbers harder. This may help to bridge between obviously different findings in this domain. In any case, I would strongly suggest to incorporate the above considerations and papers in the current discussion of “whether and when” reward-based distractor effects occur – extending the discussion beyond the role of task difficulty and eye movement contribution (which are featured in the concluding sentence).

2) Above and beyond the above considerations, I absolutely agree with the third potential source of the absent effect that is put forward by the authors, namely that the reward manipulations might have been too subtle - and without additional studies using variants of the current task, it is not possible to disentangle these different sources. Is there any way to test whether performance in the training phase is related to the distractor effects in the test phase in the current study – and why was the test phase not analyzed to begin with? Especially if reward contingencies are implicit and have to be picked up by the participants, it is very informative to test whether reward-related features influenced participants performance in any way in the training itself. And even if this is not the case on average, one could test the relationship between training and test phase effects of reward-related features across participants.

3) Related to the role of different task regimes and trials structure, there are several recent papers that seem to be of relevance for this discussion: On the one hand, Sha and Jiang (2016) challenged the view that the findings by (especially) Anderson and colleagues are actually driven by value (see Anderson and Halpern 2017 for more discussion). On the other hand, Mine and Saiki (2015) showed reward-related distractor effects in different task variants, including one in which the task

structure of the training phase (flanker task) was different from the test phase (visual search), which suggests that the distractor effects transfer between tasks.

Additional points:

- It would have been valuable to actually measure eye-movements if this was a strong hypothesis. Then again, the capture effects reported by work by Anderson and related paradigms are based on covert attention shifts as far as I know (so in other words, overt eye-movements are not a prerequisite for attentional capture). In any case,, without having eye-movement measures, it is hard to draw a strong conclusion based on the manipulation and results of experiment 3.
- Behavioral results of the training phase are missing.
- Please elaborate on why additional measures were taken (Table 1). And if these are available, why were they not related to performance in the task? It might make sense to include the relevant ones as covariates in the analysis to further explore whether subtle inter-individual differences would be associated with performance variations.
- BIS is never defined I believe

2) Figures/tables/data availability:

Please comment on the author's use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline of Collabra here):

Figures and tables are adequate

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required.:

I did not find information about explicit ethical approval - apart from mentioning the written informed consent.

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for improvement beyond the scope of this process.:

Writing is of high quality

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Ms Dorottya Ruz

University of Glasgow, UK

2018-09-21 12:47:55

Dear Dr. Madan and Reviewers,

We would like to thank you for reviewing our manuscript titled Do reward-related distractors impair cognitive performance? Perhaps not. We would also like to thank for all your valuable and constructive comments.

We have addressed all the comments as shown in the revised manuscript (changes are indicated with color red). We added precision to the text, both in the introduction and discussion, added the analysis of the training phase, and incorporated all suggestions/extra literature. In the "Response to the reviewers" document (see attachment), we explain point by point what we changed in our manuscript.

Thank you again for reconsidering our manuscript; we hope that you will enjoy reading the new version.

Sincerely, also on behalf of my co-authors,

Dorottya Ruz

Ms Dorottya Ruz

University of Glasgow, UK

2018-09-21 12:54:55

Dear Dr. Madan,

As I am not entirely sure whether my previous email contained the attachment ""Response to Reviewers"" file, I am copy pasting it below.

Sorry for this inconvenience.

Best wishes,

Dorottya Rusz

Response to the editor and reviewers

Reviewer 1:

This is an interesting and well-motivated study that was conducted with care. The data analyses are appropriate and show very little evidence of value-modulated impairments in cognitive performance, with Bayes factors favoring the null hypothesis. The authors anticipate several of my thoughts and concerns in the Discussion. Still, the present study leaves me somewhat underwhelmed, and I think the conclusions would be substantially strengthened with some further experimentation. Publication feels a bit premature to me at this stage, but I reiterate that the current experiment was very well conducted and does add something to the literature.

Response:

We would like to thank Reviewer 1 for these positive comments. We completely understand that null results can feel somewhat underwhelming. In fact, we have this sentiment too. We nevertheless decided to report on this project at its current stage, for the following reasons:

- A. Our sample size was larger than most studies in this area ($N = 81$); our study was adequately powered.
- B. Our manipulation allowed us to test different versions of the learning phase (Experiment 1A vs. Experiment 1B) and different versions of the test phase (Experiment 1A/1B vs. Experiment 1C). Thus, our data are quite rich, allowing us to address our research question from different angles.
- C. The literature on value-driven attention is rapidly growing (e.g., 100+ papers have been published since 2011), but the possibility of publication bias is hardly ever mentioned in this area (Munafò et al., 2017; Open Science Collaboration, 2015). We believe it is important to publish null findings too, perhaps particularly in cases like this.

We hope that Reviewer 1 and you agree with these points.

1. It would be very nice to see independent evidence that the test phase is sensitive to performance impairments due to distractors in some other domain, for example physically salient distractors. It is possible that I am just unaware of such evidence, as the authors do state that the task was "based on our previous work (Rusz et al., 2017)," although the citation is not included in the references list and a quick search for the first author did not produce a candidate paper. Without such evidence, it is unclear how much the null effect has to do with value vs an insensitivity to distraction in the paradigm more generally.

Response:

Reviewer 1 is right that this information was lacking in the manuscript. First, several studies show that internal sources of distractions (i.e., thoughts or worries about one's performance) can disrupt math per-

formance, especially when math problems entail heavy working memory demands (Beilock & Carr, 2005; Beilock, Kulp, Holt, & Carr, 2004). Based on these previous studies, we selected a math task with difficult mental operations (e.g., carry operations) that place heavy demands on working memory. We expected that distractions from external sources, too, would harm performance during difficult math problems. In order to address this issue, in the new version of the manuscript, we added this information to our Introduction in line 132.

Second, in separate experiments, we found some evidence that reward-related (vs. no reward related) distractors harmed performance in the math task. Our paper about these experiments is not yet officially published—it has just gotten accepted for publication at PLoS One. Due to an error, the previous version of the manuscript did not include a reference to this paper. Now, we included it as a reference (line 591) and we uploaded a pre-print, which can be found via this link: <https://psyarxiv.com/vf84e>.

2. The authors are correct in that we really cannot know whether the reward manipulation was effective at all in the training phase. This would be less of a concern if the training phase involved an established task and procedure, but there are some differences that could matter for associative reward learning. One such difference would seem to be that the task is a more focused attention task with a higher working memory demand than training tasks more typically used in the value-driven attention literature, which could result in greater suppression of irrelevant features such as color and thus a reduced capacity to pick up on stimulus-reward associations.

Response:

We agree with Reviewer 1 that our paradigm did not include a well-established learning procedure. In order to address this comment, and to shed some more light on whether the reward learning task was effective in the first place, we analyzed participants' performance during the training phase (see also our responses to Reviewer 2/comment 2 and Reviewer 3/comment 2). The results are now presented in line 220, Table 2 (line 678) and Figure 3a-d (line 685-744).

In short, we found no evidence for reward learning on the whole sample. However, when analyzing experiments separately, we found that reward learning seemed to be strongest when we applied a random reward schedule (Experiment 1B). We found weaker evidence for reward learning when reward delivery was constant (i.e., when red always predicted rewards; Experiment 1A). Finally, it seems that placing letters and numbers further away from each other made it difficult to acquire stimulus-reward associations (Experiment 1C). In sum, applying a random reward schedule made participants more effective in acquiring stimulus-reward associations. However, as this analysis was not preregistered, it should be interpreted with caution.

We agree that our learning task (i.e., responding to letter sequences) involved more focused attention and higher working memory demand than other procedures used in previous studies. In fact, we agree that this could be a potential reason for not finding a reward-based distraction effect in the test phase. However, as we discussed above, we found some evidence for reward learning in the training phase. Also, there are several prior studies that show that more focused attention learning tasks can also result in reward-based capture in the test phase (e.g., Anderson, 2016; Mine & Saiki, 2015). We now cite these studies in the Discussion in line 415.

Despite these studies, and despite that we found some indication of learning, we do agree with Reviewer 1 that a more focused attention tasks may generally give less room to learn stimulus-reward associations. To address this issue, we updated and extended our Discussion in line 407.

3. Eye tracking data could be very informative here. If the authors could show reliable distraction at the perceptual level but no appreciable effect on math performance, that would go a much longer way towards arguing that value-driven attentional capture has minimal consequences for cognitive control processes. Demonstration of reliable attentional capture in eye movements would ameliorate the concerns

raised in the prior two points, and would also provide an anchor point for understanding what strength of capture fails to translate to a performance decrement on the math task.

Response:

We understand that eye-tracking data would be very informative in this study (see our response to Reviewer 3/minor comment 1). By measuring eye movements, a similar study could shed more light on the temporal aspects of reward-based distraction – for instance, whether participant’s eye movements are captured by reward-related distractors first – and more importantly, whether despite any initial capture, participants would later still be able to perform mental additions.

We should note, though, that examining the temporal aspects of reward-based distraction was not the main goal of the current study (we set out to examine whether reward-related distractors have downstream effects on cognitive control). That said, we do understand how the addition of eye-tracking measurements could have made our conclusions more nuanced. So, to address this comment, we now discuss the lack of eye tracking measurements and draw a more cautious conclusion based on this limitation in the Discussion in line 363. We hope that Reviewer 1 and you agree with this solution.

4. The distractors are not associated with a corresponding response (previously) related to the receipt of reward, which contrasts with the Krebs et al. studies the authors cite. There is mounting evidence that, under these sorts of conditions, value-driven attentional capture can have consequences for decision-making and action selection (see Anderson, 2017, *Curr Dir Psychol Sci*, for a review). This sort of situation also maps more naturally onto the real-world example the authors provide of food smell in the office (e.g., get up and approach the food vs stay at desk and continue with task). Might stronger effects been observed if the distractors were capable of activating a response that could compete with the cognitive actions needed to perform the task? The authors bring up a similar issue in the Discussion currently, but I think the issue of action associations might have consequences not only for learning (which the authors discuss), but also for what distractor-related impairment looks like in the test phase (i.e., what representations are activated by the distractor that could influence performance).

Response:

We thank Reviewer 1 for raising this very important issue. We agree that the lack of response competition between distractor and target – as the distractor color was never paired with any sort of response – may be responsible for the lack of distraction effects in the testing phase. To address this issue, in the new version we discuss this as a limitation in the Discussion in line 389.

Minor:

1. There are some studies indicating that reward-associated distractors can break through and capture (spatially) focused attention (e.g., work by Munneke et al. and Wang et al.), which contrasts with the effects of physical salience on distraction (which generally does not break through focused attention). This seems relevant to the present study.

Response:

We would like to thank Reviewer 1 for directing us to these papers. In our view, results from these studies are in line with the rationale of the current study, that is, they inform the question of whether reward-associated distractors harm cognitive processes beyond sheer visual capture. We incorporated these interesting papers in our Introduction in line 104.

2. The authors indicate having collected several individual differences measures, but never examine correlations with these measures and performance in the task. In and of themselves, reporting the means of these measures across participants is not particularly informative, and the reader wonders why they were acquired in the first place (the authors do not even comment on the descriptives, with the exception of sleep < 6 hrs). It would be good to report these relationships, perhaps as exploratory analyses in supplemental material, to provide a more complete picture of the data.

Response:

Indeed we collected these measures for exploratory purposes, as we stated it in the pre-registration (<https://aspredicted.org/3j7gw.pdf>). In the new manuscript, we now explicitly specify this in the Method section, in line 172. We related these subjective scores to performance in the training and test phase too. Now we report these findings in the Results/Training phase (line 250) and in Results/ Exploratory (not pre-registered) analyses (line 307) sections.

3. What was the final sample size for each of the 3 experiments?

Response:

N = 27 per experiment. Now, we include this information in the Method/Participants section in line 162.

I did not catch reference to IRB approval, which should be added

Response:

We added our ethical approval number in the Method section in line 154.

Reviewer 2:

This manuscript describes a study investigating the effect of distractors that were previously associated with reward on 'cognitive performance'. During an initial training phase, one color indicated that reward was available; a second color was never paired with reward. In a subsequent test phase, participants saw a series of letter-number pairs (for 700ms each), and were required to add the numbers. On each trial, one of the (task-irrelevant) letters was presented either in the rewarded or unrewarded color from training. Across three variations of the task, there was no significant evidence that the color of the 'distractor' letter influenced task accuracy, and a Bayesian analysis indicated moderate evidence for the null hypothesis. It is concluded that results suggest that reward-associated distractors do not disrupt cognitive control processes.

The manuscript is well-written and logically structured, and the topic is interesting. This was a preregistered study, which is to its credit – statistical analyses seem sound, and I particularly appreciated the Bayesian analysis to quantify support for the null.

1. First, the article is billed as investigating the effect of reward on 'cognitive performance', but it's not totally clear what this means. The authors note that many prior studies have shown that reward influences visual attention, but "less is known about whether reward-related distractors have broader cognitive and behavioral consequences. That is, in daily life at work and school, most tasks require complex interactions with information and not merely searching for target stimuli." But imagine the current studies HAD found poorer performance on the mental arithmetic task on trials with a reward-related distractor – why couldn't this be ascribed to an effect on attention? One plausible interpretation would be that the reward-related color distracts attention away from the simultaneously-presented number, so that participants don't encode the number and hence are impaired on the arithmetic task. Indeed, the manipulation in Expt 1c (increasing the spacing between distractor and target) seems to be trying to encourage this sort of pattern (see last sentence of first paragraph on p14).

So ultimately it wasn't clear to me how, if this study had found an effect, this could be confidently attributed to an influence on cognitive control processes rather than attention / capture – it may not be

the performance of arithmetic that's disrupted, it may be the encoding of the numbers. Of course the study didn't find an effect, but extending the above argument this doesn't necessarily shed light on the (lack of) effect of reward on 'cognitive control processes'.

Response:

We would like to thank Reviewer 2 for this helpful insight. We now see that, in the previous version of the manuscript, we did not precisely explain what we mean by "harming cognitive performance", which gave room for different interpretations. To be sure, we assume that performance in our task is impaired as follows:

1. The distractor stimulus that was previously associated with reward, attracts attention – exactly like it is documented in the literature (Anderson, Laurent, & Yantis, 2011).
2. As a consequence, the distractor stimulus gets more priority in downstream processing – for instance, previous research shows that reward-related stimuli gain processing priority in working memory (Gong & Li, 2014; Klink, Jeurissen, Theeuwes, Denys, & Roelfsema, 2017).
3. In turn, the distractor stimulus gets greater potential to impair cognitive control processes.

Importantly, it was never our aim to disentangle between effects on attention vs. effects on cognitive control. Instead, we think that an effect on attention is probably the first step, perhaps even a precondition, for downstream effects on cognitive control to emerge. However, we do understand that this issue needed additional clarification, and that our previous text was ambiguous in this respect. We thus made two changes to the manuscript:

1. We now explain what we mean by "distraction of cognitive performance" in the Introduction, i.e., we clarify that we believe that an effect on attention may be just the first step in a longer cascade of processes (line 117). We also consistently apply this idea throughout the manuscript, for instance in line 79 and line 128.
2. We mention in our limitation section that further improvement to our paradigm could be made to zoom in on component processes that together cause distraction during cognitive control tasks. For instance, we discuss that eye-tracking would be a good way to test whether participant's eye movements are captured by reward-related distractors and whether optimal performance is still possible after this initial capture (line 363).

2. As the Discussion notes, this study has some important limitations. Perhaps the biggest is that there's no evidence that the participants learned the reward associations, and if this isn't the case then of course we would not expect reward to influence performance. Indeed, the 150 training trials used here is lower than typical for studies of reward-related attention (e.g., Anderson et al, 2011, used a minimum of 240 trials training, and up to 1008). Another issue is that the relatively slow presentation rate of stimuli (700ms per item) may well be more than enough for participants to correct for and overcome distraction by the reward-related stimuli.

As noted above, both of these limitations are mentioned in the Discussion. My concern is that they render the study difficult to interpret: because of these limitations, we end up not knowing much more after this study than we did before. Reward may not influence cognitive control, or participants may not have learned about reward – we don't know which. So the conclusion on p25 ("All in all, results from this study suggest that distractors associated with reward do not disrupt cognitive control processes") seems too strong.

Response:

We agree with Reviewer 2 that in order to shed more light on reward learning in the current study, it makes sense to analyze data from the training phase. We now did so, and we present the results in line 220, in Table 2 (line 678), and Figure 3a-d (line 685-744) (See also our response to Reviewer 1/ comment 2 and Reviewer 3/comment 2).

Summarizing our results, we found no evidence for reward learning on the whole sample, but we did find indications for reward learning in the different versions of the experiment. Specifically, reward learning seemed to be strongest when we applied a random reward schedule (Experiment 1B). We found weaker evidence for reward learning in the experiment, in which reward delivery was constant (i.e., when red always predicted rewards; Experiment 1A). Finally, participants did not pick up on stimulus-reward associations when the letters and numbers were placed further away from each other (Experiment 1C). In sum, applying a random reward schedule made participants most effective in acquiring stimulus-reward associations, regardless of the relatively short training (n=150 trials). We should mention, though, these results should be interpreted with caution as these analyses were not preregistered.

We are aware that most previous studies in this area used longer training sessions. However, we also note that some prior work reported a value-driven capture effect with less training trials (e.g., 144; Sali, Anderson, & Yantis, 2014) than we had in the current study. We added this information in the Discussion in line 416.

We further agree that 700 ms for stimuli presentation is rather slow for visual attention types of task. However, in our task, people had to perform several demanding mental operations at the same time, such as keeping a number in mind (i.e., the current sum), adding a new digit to this number (e.g., the current sum + 8; note that this often required a carry operation), and updating the number that was kept in mind. Also, previous work on mental arithmetic shows that simple mental additions may well take 800-900 ms (Ashcraft, 1992; Ashcraft & Battaglia, 1978). We now added this information in the Introduction, in line 138.

Taken together, we agree with, and are grateful for, Reviewer 2's insights. At the same time, we think this issue is quite nuanced. So, we formulated a more cautious conclusion in line 437. We hope this solution is in line with what Reviewer 2 had in mind.

3. On p21-22 the possibility is raised that the effect of reward-related distractors may be relatively short-lived. On this topic, I would point the authors towards recent work using attentional blink-type procedures that has looked at the influence of reward-related stimuli. Unfortunately results are somewhat mixed: one study (Failing & Theeuwes, 2015, Vis Cogn) found that distraction does not dissipate rapidly, while another (Le Pelley, Seabrooke, Kennedy, Pearson & Most, 2017, Atten Percept Psycho) found that the effect was short-lived. There may be others.

Response:

We agree that these are very interesting and relevant papers to our study. We incorporated these studies in the Discussion, in line 344 and in line 346.

There is no statement regarding approval by a relevant ethics board. It is stated that participants provided consent.

Response:

We added our ethical approval number in the Method section in line 154.

Reviewer 3:

Rusz and colleagues report the results of 3 experiments showing that stimulus features that had been associated with reward had no effect on a subsequent mental arithmetic task when presented as irrelevant distractors – which is at odds with previous studies observing detrimental effects of reward-related distractors. The present study is certainly a valuable contribution to the discussion of whether and when reward-related distractor effects occur. I think the study is very well written, the design is elegant, and the methods are sound. I also appreciate the pre-registration approach and the clear dissociation between planned and exploratory analyses. My main comments concern the interpretation of the results in the light of existing work on reward-related distractor effects, as well as the missing information about performance in the training phase itself.

1) In very general terms, it seems obvious from the literature that the deciding factors in the discussion about reward-related distractor effects are related to task structure and stimulus properties rather than whether the task at hand is a visual attention or cognitive control task, or whether the task is difficult or easy (which seems to be implied at several places throughout the manuscript). Most importantly, the current task did not involve direct competition of stimuli and/or responses and the task structure allowed for strategic suppression of irrelevant information in the test phase (here, letters as an entire stimulus category). Potentially, participants were able to form an attentional set for numbers and could strategically ignore the letters in a sustained fashion (regardless of the distance between target and distractor). With regard to the paradigm by Krebs et al. (which indeed qualifies as typical cognitive control tasks), the discrepancy is not too surprising to me, although I would attribute this to a slightly different source (above and beyond mere task difficulty). The biggest differences in my view are that 1) the distractor colors in Krebs et al. are associated with a competing response and distractor effects could in principle reflect response capture rather than attentional capture, or a combination of both. Moreover, 2) color as a concept is actually always relevant throughout the task, which is different from the training-test approach used in the current study, and which makes it much harder to strategically ignore certain stimuli or features. In some way, this discussion seems to relate to earlier work of Serences et al. (2005) who report attentional capture by salient (target-colored) stimuli that occur in the “wrong” location. Similar to the Stroop task by Krebs et al. (and in contrast to the current task), this task was structured in a way that a strategic suppression of these features is neither possible nor globally beneficial as they are relevant to other aspects of the task. That said, it would be very interesting to see if reward-related distractors in the current task can have an effect if training and test trial types are interleaved, e.g., in mini-blocks, which makes a strategic focus on only numbers harder. This may help to bridge between obviously different findings in this domain. In any case, I would strongly suggest to incorporate the above considerations and papers in the current discussion of “whether and when” reward-based distractor effects occur – extending the discussion beyond the role of task difficulty and eye movement contribution (which are featured in the concluding sentence).

Response:

We would like to thank for Reviewer 3 for raising this issue. We find this comment extremely helpful. Indeed, it seems plausible that the lack of response competition and a strong top down attentional set would decrease the chance of finding any sort of interference from reward-related distractors. In the current version, we included this consideration in the Discussion, in line 389.

2) Above and beyond the above considerations, I absolutely agree with the third potential source of the absent effect that is put forward by the authors, namely that the reward manipulations might have been too subtle – and without additional studies using variants of the current task, it is not possible to disentangle these different sources. Is there any way to test whether performance in the training phase is related to the distractor effects in the test phase in the current study – and why was the test phase not analyzed to begin with? Especially if reward contingencies are implicit and have to be picked up by the participants, it is very informative to test whether reward-related features influenced participants performance in any way in the training itself. And even if this is not the case on average, one could test the relationship between training and test phase effects of reward-related features across participants.

Response:

We agree that in order to shed more light on reward learning in the current study, the training phase should be analyzed. We now did so, and the results are now presented in Table 2 (line 678), Figure 3a-d (685-744), and in line 220 (also, see our response to Reviewer 1/comment 2, and to Reviewer 2/comment 2). In short, we found no evidence for reward learning when considering the whole sample. Nev-

ertheless, we examined all task variations separately and found that reward learning seemed to be the strongest when we applied a random reward schedule (Experiment 1B). We found weaker evidence for reward learning in the experiment, in which reward delivery was constant (i.e., when red always predicted rewards; Experiment 1A). Finally, participants did not acquire stimulus-reward associations when the letters and numbers were placed further away from each other (Experiment 1C). In sum, applying a random reward schedule seemed to have made participants more efficient in learning stimulus-reward associations. Yet, we interpret these results with caution as the analysis was not preregistered.

As Reviewer 3 recommended, we also tested the relationship in between the training and test phases. We explain the procedure and results of this analysis in line 315.

3) Related to the role of different task regimes and trials structure, there are several recent papers that seem to be of relevance for this discussion: On the one hand, Sha and Jiang (2016) challenged the view that the findings by (especially) Anderson and colleagues are actually driven by value (see Anderson and Halpern 2017 for more discussion). On the other hand, Mine and Saiki (2015) showed reward-related distractor effects in different task variants, including one in which the task structure of the training phase (flanker task) was different from the test phase (visual search), which suggests that the distractor effects transfer between tasks.

Response:

We agree that these are relevant and very interesting papers to our study. We thank Reviewer 3 for recommending these papers to us. Now we incorporated them in line 442 and in line 416.

Additional points:

It would have been valuable to actually measure eye-movements if this was a strong hypothesis. Then again, the capture effects reported by work by Anderson and related paradigms are based on covert attention shifts as far as I know (so in other words, overt eye-movements are not a prerequisite for attentional capture). In any case,, without having eye-movement measures, it is hard to draw a strong conclusion based on the manipulation and results of experiment 3.

Response:

We agree with Reviewer 3 that measuring eye movements could be a valuable addition to the study (see our response to Reviewer 1/comment 3) as we could then test whether despite initial saccades to the distractor, participants would still be able to perform mental additions well. This would be especially interesting given the design of Experiment 1C – in which we increased space between target and distractor. So, in sum, we agree that eye movements would be a nice addition to our study.

However, we did not measure eye movements in this study for two major reasons. First, the current goal of the study was to assess whether reward-related distractor harms beyond visual attention and whether they disrupt performance on a task that require cognitive control processes. Disentangling between different underlying processes of distraction (e.g., whether it starts with an initial visual capture) was beyond the scope of this study. Second, as Reviewer 3 mentioned, prior research shows that eye movements may not even be necessary for impairment by reward-related distractors (Anderson & Yantis, 2012). So, when we designed the experiment, we reasoned that if there is indeed a capture (especially in Experiment 1C), it should be possible to detect an effect on a behavioral level, e.g., slower RTs/worse accuracy.

Regardless, we agree that eye-tracking make our conclusions more nuanced, so we extended our Discussion about the lack of eye movement measures and draw a more cautious conclusion based on this limitation (line 363).

Behavioral results of the training phase are missing.

Response:

Results of the training phase are now reported in Table 2 (line 678), Figure 3a-d (685-744), and in line 220.

Please elaborate on why additional measures were taken (Table 1). And if these are available, why were they not related to performance in the task? It might make sense to include the relevant ones as covariates in the analysis to further explore whether subtle inter-individual differences would be associated with performance variations.

Response:

Indeed we collected these measures for exploratory purposes, as we stated it in the pre-registration (<https://aspredicted.org/3j7gw.pdf>). Now we specify this in the current version of the manuscript in line 172). We related these subjective scores to performance in the training and test phase too. Now we report these findings in the Results/Training phase (line 250) and in Results/ Exploratory (not pre-registered) analyses (line 307) sections.

BIS is never defined I believe

Response:

In the new version, we defined BIS = Barratt Impulsiveness Scale in the method section in line 170.

I did not find information about explicit ethical approval - apart from mentioning the written informed consent.

Response:

We added our ethical approval number in the Method section in line 154.

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Dr Christopher R Madan

University of Nottingham, GB

2018-11-28 03:16:38

Dear Ms Rusz,

After review, we have reached a decision regarding your submission to Collabra: Psychology, ""Do reward-related distractors impair cognitive performance? Perhaps not."" Our decision is to request revisions of the manuscript.

The full review information should be included at the bottom of this email. The reviewers were generally satisfied with the revisions, however, as pointed out by Reviewer 1 there appears to be a critical oversight in initially disclosing the related study, ""Reward-associated distractors can harm cognitive performance"". While this reference is included in the revision, it is only cited in passing. However, it was conducted by the same authors and uses a very similar procedure, yet comes to an opposing conclusion and even has a similar but contradictory title in relation to the current manuscript. Potential publication of the current manuscript will be highly dependent on the discussion of how these two manuscripts are related, in a manner that is direct and clear. Addressing the comments from Reviewer 3 regarding the misleading statements in the abstract and discussion are also necessary for publication of this manuscript to be further considered.

To access your submission account, follow the below instructions:

- 1) login to the journal webpage with username and password
- 2) click on the submission title
- 3) click 'Review' menu option
- 4) download Reviewed file and make revisions based on review feedback
- 5) upload the edited file
- 6) Click the 'notify editor' icon and email the confirmation of re-submission and any relevant comments to the journal.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please do contact us.

Please could you have the revisions submitted by January 31, 2019. If you cannot make this deadline, please let us know as early as possible.

Kind regards,

Dr Christopher R Madan

University of Nottingham

christopher.madan@nottingham.ac.uk

Reviewer 1:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?
- Does the conclusion (if present) reflect the argument, is it supported by data/facts?

- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

On the one hand, the recently accepted manuscript offers some evidence for the sensitivity of the test phase (and by proxy the efficacy of training phase), and the analysis of training phase data offers weak evidence for the efficacy of the training manipulation. The added discussion does a more comprehensive job of discussing the limitations of the present study. Although I still feel like there is substantial opportunity to strengthen this paper with a more solid proof-of-concept for the task design, I don't think this is alone sufficient to serve as a barrier to publication.

On the other hand, the accepted study that the authors now provide a reference for essentially reports the exact same experiment, but with an additional manipulation of trial-by-trial motivation during the test phase. The unrewarded test phase trials are basically the same as the test phase trials of the present study. The contribution of the present study is thus quite incremental by itself, which coupled with my prior sentiment described above, substantively dampens my enthusiasm. I am very surprised that the authors do not discuss this other study in greater detail, explicitly mentioning that it uses a very similar paradigm and discussing why we would predict different results in the present study (or what the present study adds beyond a conceptual replication of the authors' prior work). The authors do not even mention their prior study in the Introduction when motivating the study or in the Discussion--reference is only found in passing in the "Present research" section as having influenced the design of the task. The authors motivate the present study as if that prior evidence did not exist, and somewhat ironically the conclusions that are drawn are somewhat in opposition (compare titles). I find this lack of transparency highly inappropriate.

2) Figures/tables/data availability:

Please comment on the author's use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline of Collabra here):

OK

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required:

OK

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for

improvement beyond the scope of this process.:

OK

Reviewer 2:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?
- Does the conclusion (if present) reflect the argument, is it supported by data/facts?
- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

The authors have done a good job of addressing the issues that I raised with regard to the previous version. It's a shame that the study produced null findings, but it's a useful addition to the record nevertheless. I have only minor issues remaining.

1. With regard to the new analyses of data from the training phase. P11 states: "In sum, as results from Experiment 1B were clearest, applying a random reward schedule increased people's tendency to acquire stimulus-reward associations". In order to draw the conclusion that applying a probabilistic reward schedule *increased* learning of reward associations would require showing that the effect of reward was significantly *greater* under the probabilistic schedule (i.e., in Expt 1B) than under the deterministic schedule (i.e., in Expt 1A). None of the reported analyses test this contrast, so the stated conclusion is not valid. Likewise the claim on p19 that "our analysis of the training phase revealed that random reward schedule seems to be more effective than a full reward schedule".

2. Various new correlational analyses are now reported (e.g., paragraph 1, p12; paragraph 3, p14). The text notes that these analyses were not pre-registered; it should also note that p-values in these analyses have not been corrected for multiple comparisons.

3. The premise of Experiment 1C is that increasing target-distractor separation should increase the influence of distractors on performance. In fact existing evidence suggests the opposite: distraction effects are typically greater when target-distractor separation is small than when it is large (e.g. Gaspar & McDonald, 2014, Suppression of salient objects prevents distraction in visual search, *Journal of Neuroscience*; Hickey & Theeuwes, 2011, Context and competition in the capture of visual attention. *Atten Percept Psychophys*).

4. p8 suggests that the rewarded and non-rewarded colors were counterbalanced across participants. Given that there was an odd number of participants in each experiment, this counterbalancing can't have

been complete.

5. p14, paragraph 3: The measure of "RT when a high-reward distractor was present minus RT when no-reward distractor was present" would be more accurately described as "amount of reward-related capture" than "amount of capture".

6. p18-19: "Third, our training phase was shorter than training phases in previous studies (e.g., 240)". This is unclear: presumably the "240" here refers to the number of trials in the training phase of the current study? This should be clarified.

2) Figures/tables/data availability:

Please comment on the author's use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline of Collabra here):

fine

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required.:

fine

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for improvement beyond the scope of this process.:

fine

Reviewer 3:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?
- Does the conclusion (if present) reflect the argument, is it supported by data/facts?
- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

The authors thoroughly responded to my initial comments, and I think the paper makes a nice contribution to the domain in that it shows that reward-related distractors will not impair task performance under all circumstances.

That said, I still think that the authors provide a somewhat unspecific and even misleading interpretation of their findings, especially in the abstract and conclusion section. This relates back to the first comment in my initial review. The authors argue that the main deciding factor for the presence or absence of reward-distractor effects is whether the task requires prominently spatial attention or cognitive control (or "complex mental operations").

Although this argumentation may be sufficient when comparing the results to spatial attention studies by Anderson et al., it does not hold when considering previous work by Krebs et al., who in fact found reward-related distractor effects in a task that certainly requires cognitive control. While this is now acknowledged in the revised discussion section, the general conclusion put forward in the abstract and discussion (reward-based distractor effects do not occur in a task requiring cognitive control) remains unspecific and misleading in my view. I would thus recommend to qualify the respective statements (examples below) acknowledging that the opposing findings seem to arise from more specific paradigmatic differences related to task-relevance of reward features and direct response competition.

Exemplary passages:

Abstract: "...findings point to the idea that task-irrelevant reward cues may not hurt performance on tasks that rely on cognitive control."

Introduction: "...we investigated whether distractors associated with reward indeed harm performance on a task that requires cognitive control."

Discussion: "In the current research, we found no evidence that reward-related distractors harm cognitive performance on a task that requires complex mental operations."

And a final small comment: Since the Journal explicitly asks to include a statement about informed consent, I would recommend to slightly adjust the wording in this regard (p. 8, l. 165) to something like this: "First, participants gave their written informed consent to participate in the study..."

2) Figures/tables/data availability:

Please comment on the author's use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline of Collabra here):

The use of figures and Tables is adequate.

The underlying data is available for the test phase, but I was not able to find the training phase data (maybe just an oversight on my end).

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required:

The required information is provided.

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for improvement beyond the scope of this process:

The manuscript is very-well written.

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Ms Dorottya Ruzs

University of Glasgow, UK

2018-12-07 12:24:10

Dear Dr. Madan,

First of all, we would like to thank you again for reconsidering our manuscript. We highly appreciate the

positive comments about the changes we made, your time, and your further constructive comments on our manuscript. We understand that the previous state of our manuscript still raised some important concerns.

In the revised version of the manuscript, we addressed all comments by all reviewers. To summarize the two main changes: First, it was never our intention to be non-transparent or misleading in any way. In line with Reviewer 1's comments, we now discuss our recently-accepted paper (in PLoS ONE) throughout the new version of our manuscript (i.e., in the Introduction on page 6 and 7, in the Discussion on page 16-17, and in the Conclusion on page 21). To explain ourselves further, we provide more background information about the PLoS ONE paper in the Response to the Reviewers letter (page 1-2). Second, in line with Reviewer 3's comments, we took care to avoid any conclusions that could be construed as misleading or incomplete. In the new version of the manuscript, all changes appear in red.

Thank you again for reconsidering our manuscript. We sincerely hope that we managed to clarify the connection between our previously accepted and the current manuscript and that you agree with the choices we made. We also hope that you will enjoy reading the new version.

Sincerely, on behalf of my co-authors,

Dorottya Ruz

Note: I added the Response to the Reviewers letter as an attachment to this email, but also uploaded it to the Supplementary materials.

File: <https://s3-eu-west-1.amazonaws.com/ubiquity-partner-network/ucp/journal/collabra/169-2609-1-SP.docx>

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Dr Christopher R Madan

University of Nottingham, GB

2019-02-12 23:52:02

Dear Ms Dorottya Ruz,

We have reached a decision regarding your submission to Collabra: Psychology, ""Do reward-related distractors impair cognitive performance? Perhaps not."" and are happy to accept your submission for publication, pending the completion of copyediting and formatting processes. In particular, I appreciate the detailed timeline and discussion of the relationship between the two papers provided in response to Reviewer 1 (along with the respected changes to the manuscript) and the thoughtful edits made in response to the overstatements highlighted by Reviewer 3.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point.

The accepted submission will now undergo final copyediting. You will be contacted once this is complete to answer any queries that may have arisen during copyediting and to allow a final chance to edit the files prior to typesetting. If you wish to view your submission during this time, you can log in via the journal website.

The review information should be included in this email.

Kind regards,

Dr Christopher R Madan

University of Nottingham

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