**Peer Review and Communication History**

**MS Title**: The (Virtual) Reality of Social Approach-Avoidance Behaviours: Operationalisation Development and Construct Validity Testing

**Author Names**: Ivane Nuel, Marie-Pierre Fayant, Nicolas Morgado, Baptiste Subra, and
Theodore Alexopoulos

**Submitted:** Oct 28, 2021

**Editor First Decision**: Revise & Resubmit

Feb 6, 2022

Dear Ivane Nuel,

I have now received all reviews of your manuscript, “The (Virtual) Reality of Intergroup Interactions: Approach-Avoidance behaviours and Group Evaluations” from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript has important strengths and also that there are some issues that need to be addressed. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology. Although there is enthusiasm for the work, I cannot make any firm predictions about the ultimate fate of the paper.

The reviewers did an outstanding job in their reviews. I will highlight issues I think are particularly salient here. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

1. Reviewer 1 and I both felt that there was a lack of clarity in the manuscript regarding the primary aim of the work. According to the pre-registrations, the primary aim was “to examine the construct validity of an approach/avoidance manipulation.” I was disappointed that the manuscript didn’t seem to follow through on this aim. First, in Footnote 1, the authors note that originally, all DVs were conceptualized as indicators of the validity of the manipulation. However, the authors decided to change their approach, instead treating the RST-PQ as the only measure relevant to validity, and the evaluative measures as relevant to a separate question. This shift requires further attention, as it reflects a change in the authors’ criteria for validity that occurred after the results were known. Second, the manuscript doesn’t discuss what the results mean for the construct validity of the IVR manipulation. In both Studies 1 and 2, the manipulation did not affect the RST-PQ in the ways that would provide evidence of its validity (i.e., higher BAS and lower FFFS in the approach vs. avoidance condition). The only finding that supports the manipulation’s validity is a significant effect on one of 6 BAS subscales (goal-drive persistence) that was not pre-registered, and did not replicate in Study 2. To me, this suggests that this manipulation did not meet the authors’ criteria for validity. This is certainly useful information for other researchers, especially given the other factors that might make IVR an appealing research tool. Perhaps future research could further investigate its validity, or modify the manipulation in order to improve its validity. Here, however, there is no reason to think that the manipulation is doing what it is supposed to do. Finally, the manuscript interprets some of the effects on evaluations (e.g., trait ratings on warmth-related items in Study 2), as evidence for a behavior-attitude link. Reviewer 1 has a slightly different take than me here, and suggests that you could draw firmer conclusions given that you did find some evidence consistent with pre-registered predictions about the effect of the manipulation on trait ratings. I would agree, except I see the lack of validity as something that undermines the ability to draw any further conclusions about the relationship between approach-avoidance behaviors and other constructs (because we can’t be confident that approach-avoidance behaviors have been captured). (As a side note, it was unclear to me whether the effect of the manipulations on warmth-related trait ratings was pre-registered – see point 3). All of this is to say, it seems that the original goal of this research was to evaluate the validity of this IVR approach-avoidance manipulation. This is a valuable goal, and the studies provide useful evidence; it seems that the present findings suggest a lack of validity. I think this should therefore be the main conclusion of the manuscript, and the authors should refrain from interpreting the relationship between approach/avoidance behaviors and evaluations.
2. Related to the above point, Reviewer 2 and I wondered if the RST-PQ is the best tool to gauge the activation of neurophysiological systems supposed to underlie approach-avoidance behaviors. As the authors note, this measure was initially intended to capture traits or dispositional sensitivities. Although the authors also say that these scales may also work as state measures, their use in this manner does not seem to be well-established. I am hesitant to suggest that the authors spend more time discussing these limitations, because I think it is important to communicate to readers that this was the measure chosen, a priori, as the primary indicator of validity. In other words, it could seem like biased assimilation to critique the assessment of validity after knowing that assessment yielded null results. Perhaps there is a way, then, of raising some limitations of the RST-PQ, while also communicating that this was nevertheless the measure selected as the best indicator of approach-avoidance neurophysiological systems.
3. Although the pre-registration was clear, and I believe the authors made an effort to ensure that they distinguished pre-registered from non-pre-registered analyses, there are still points that could be clearer. For instance, the effect of condition on warmth-related trait ratings in Study 2 is described in the “Complementary Analyses” section, but it was included in the pre-registration. (This effect was also described as “non significant” in the results section, but “significant” in the discussion section, and has a listed p-value of .04). Reviewer 2 noticed that the “presence” scale was not mentioned in the pre-registration, but was included in the “Confirmatory Analyses” section. And, the effect of condition on the goal-drive persistence subscale does not appear to have been pre-registered, but it is included in the abstract as evidence for the authors’ hypotheses. The results sections should be revised so that it is clear to readers when an analysis was pre-registered and when it was not. If the terms “confirmatory” and “complementary” are still used, they should be defined for the reader. Also, the results of non-pre-registered analyses should not be included in the abstract.
4. Reviewer 1 raised some concerns, that I shared, about the strength of past research investigating the effect of approach-avoidance behaviors on evaluations. It could be helpful to look into some of this work in more detail, and explain weaknesses or limitations wherever warranted.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I realize this may be more substantial revision than you are willing to conduct, but I hope you will consider it. I look forward to receiving your revision.

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 contact the editorial office at editorialoffice@collabra.org.

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Alexa Tullett

**Reviewer 1**

**Open response questions**

Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

Thank you for the opportunity to review the manuscript, “The (Virtual) Reality of Intergroup Interactions: Approach-Avoidance behaviors and Group Evaluations” for possible publication in Collabra: Psychology. This paper presents two preregistered experiments investigating the influence of approach/avoidance behaviors on the evaluation of social groups within a virtual reality context. I was impressed with how thorough the Methods and Results were. The authors reported all of their DV’s, even the ones that were non-significant, which creates confidence in the validity of these results. Generally, this paper is a fine example of transparent and preregistered work. I do have a few questions and concerns, which I detail below.

1. The effect of attitudes (liking vs. disliking) on behavior (approach vs. avoid) is well established, but the reverse direction – the effect of approach/avoidance behavior on liking/disliking attitudes – is more novel. The authors build the current investigation on past research showing that simple approach and avoidance behaviors (e.g., pulling a joystick towards oneself) are sufficient to influence one’s evaluation of stimuli. However, I’m not sure how strong this foundation of research is. First of all, what is the proposed mechanism that causes approach/avoidance behaviors to influence evaluations of stimuli? Is it like the mere exposure effect on liking? Some of the research cited (e.g., Van Dessel et al., 2020) find only weak effects, or effects on implicit rather than explicit measures. Because the authors propose to extend this work into a new setting (virtual reality) with more complex stimuli, then the initial effect should be robust and well-established. It would be helpful if the authors spent a bit more time discussing the original effect (that simple approach/avoidance behaviors influence stimulus evaluation) in the Introduction to adequately set up the leap into a new domain and their new hypotheses.
2. Relatedly, what was the primary aim of the present investigation? Was it to establish a behavior-attitude link, that is – to replicate and extend past research that approach/avoidance behaviors influence social evaluations? Or is the primary aim to validate a novel immersive virtual reality (IVR) setting to examine approach/avoidance behaviors in social settings? The authors lean pretty heavily on the latter, concluding that “we advocated the use of IVR as an appropriate and insightful tool to expand the scope of investigation concerning the influence of approach-avoidance behaviours on group evaluations.” However, it seems to me that first establishing the relationship between approach/avoidance behavior and social evaluations is quite important.
3. Some of the main take-aways were unclear to me. For instance, some of the results did support their hypotheses that approach/avoidance behaviors in the IVR setting would influence approach motivation (Experiment 1) and positive social evaluations (Experiment 2). Yet, the authors primarily conclude that the hypotheses were not supported and that the main take-away is that IVR could be a useful tool for investigating approach/avoidance behaviors in social settings in the future. The results were messy, of course, and many effects were non-significant. However, because the authors preregistered their predictions it seems to me that the predictions that were supported should be more noteworthy. I do appreciate the caution with which the authors interpret their results, but I think that the pattern of results deserves some mention in the conclusions, beyond just the novelty of using IVR.
4. Relatedly, the authors focus on p-values when determining significance, and they use a Bonferroni correction to account for multiple tests. This type of correction is quite conservative and makes it very difficult to find significant results. Would it be better to focus on effect sizes and confidence intervals? In fact, in Experiment 2, the effect of approach/avoidance behavior on trait ratings was non-significant, but later in the discussion the authors describe this effect as accounting for “2% of the variance in one measure of group evaluations (i.e., trait ratings).” In other words, when focusing on effect sizes rather than p-values it sounds as if the behavior manipulation did have an effect on trait ratings. It would be helpful if the authors were more consistent with how they evaluated their results. And, because they went to the trouble to preregister their predictions, it might be helpful to focus on effect sizes rather than Bonferroni-corrected p-values. This choice will obviously be up to the authors and editor.

**Rating scale questions**

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  | ✔ |  |  |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  | ✔ |  |

**Reviewer 2**

**Open response questions**

Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

Overall, I appreciated the attempt of the authors to employ a novel tool to translate basic findings from past research to a more ecologically valid and immersive experience. As virtual reality tools develop, laboratory studies will continue to be able to more realistically simulate a real-world situation that we hope to generalize findings to. I agree wholeheartedly with the paragraph on Page 26 in the general discussion that advocates for use of an enriched paradigm that goes beyond minimalist laboratory settings. These results, although not supportive of the hypotheses, will be important for future researchers to consider as more studies with virtual reality are undertaken to answer questions of human interaction. I found the discussions to be appropriately tempered of what can be learned from these studies and not too focused on exploratory findings that may or may not be spurious.

Please see the below concerns that I feel need to be addressed before the paper would be suitable for publication:

-The term “Afro-Americans” used on Page 3 is outdated and should be replaced by “Black” (see APA Style: <https://apastyle.apa.org/style-grammar-guidelines/bias-free-language/racial-ethnic-minorities>)

-Would it be possible for the researchers to include links to a video of the virtual environment embedded in the paper, perhaps posted on the OSF? While Figure 1 is helpful in understanding the paradigm, the description in section 1.2 would be much aided by a video representation of how their virtual character moves in the environment.

-In section 2.1.3.1 on Page 11, the authors state that “For each encounter, participants had to consider their impression of the target individual”. Did the participants report their impression after each individual, or were these simply instructions for the participant to be considering their impressions as the task proceeded?

-Were the 16 different virtual individuals of the same race/ethnicity, weight, hair color, etc.? Were the same virtual characters presented to each participant, and in the same order?

-The RST-PQ was employed as a measure to see if the manipulation of body posture would affect ratings of the BAS and FFFS. However, the RST-PQ was intended to serve as a personality measure of trait rather than state activation of the approach and avoidance systems. Is there past precedence for using the RST-PQ as a measure of state activation of these personality systems? If not, do the authors believe that it is appropriate to use a measure with general trait-level statements to measure state activation of a particular system? Why was the RST-PQ chosen over other questionnaires (e.g. Carver and White’s BIS/BAS)?

-The preregistration did not mention the plans for the “Presence” scale. In the confirmatory analyses section (Page 14), it begins by mentioning that the mean was significantly above the midpoint of the scale, but I did not see this analysis mentioned in the preregistration. Therefore, I do not believe it should be in the confirmatory analysis section, and a justification for testing against the midpoint as a measure of feeling presence should be given. This comment also applies to Experiment 2 (Page 20).

-In section 2.2.2.3 (Page 19), please list the 6 positive and 6 negative traits used.

-In Experiment 2, the authors state in the results section that “Although non significant, results revealed that participants evaluated the encountered group as warmer in the approach than in the avoidance condition…”, yet in the discussion they state, “This positive effect of approach is significant on the warmth dimension, which is considered to be the primary dimension in social judgment…”. The p value was .04, which would be traditionally considered statistically significant but not in light of the Bonferroni-adjusted alpha level. Please reconcile the language in these two sections to reflect whether a Bonferroni-adjusted alpha level was used for the exploratory analyses.

**Rating scale questions**

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  |  |  | ✔ |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  |  | ✔ |

**Author Response**
Mar 24, 2022

Dear Alexa Tullett,

Thank you again for the sincerity with which our manuscript has been handled. We integrated the expert feedback in a new version of the manuscript. This extremely constructive feedback ultimately helped us to improve the quality of our manuscript to hopefully meet the threshold for publication in *Collabra: Psychology*. In the present document we reply to expert comments and mention the changes we made in the manuscript, point-by-point.

Editor:

Author Name

I have now received all reviews of your manuscript, “The (Virtual) Reality of Intergroup Interactions: Approach-Avoidance behaviours and Group Evaluations” from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript has important strengths and also that there are some issues that need to be addressed. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology. Although there is enthusiasm for the work, I cannot make any firm predictions about the ultimate fate of the paper.

The reviewers did an outstanding job in their reviews. I will highlight issues I think are particularly salient here. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

1. Reviewer 1 and I both felt that there was a lack of clarity in the manuscript regarding the primary aim of the work. According to the pre-registrations, the primary aim was “to examine the construct validity of an approach/avoidance manipulation.” I was disappointed that the manuscript didn’t seem to follow through on this aim. First, in Footnote 1, the authors note that originally, all DVs were conceptualized as indicators of the validity of the manipulation. However, the authors decided to change their approach, instead treating the RST-PQ as the only measure relevant to validity, and the evaluative measures as relevant to a separate question. This shift requires further attention, as it reflects a change in the authors’ criteria for validity that occurred after the results were known.

Thank you for this comment. The shift was not intentional. These studies were conducted with the aim to develop a more realistic and ecological approach-avoidance operationalisation in order to test their evaluative influence. Indeed, the original aim of these studies was to examine the construct validity of the IVR operationalisation, but our ultimate goal was the examination of the link between approach-avoidance and evaluation. In other words, in our original reasoning, the evaluative influence of approach-avoidance behaviours was both the focal effect we were interested in and an indicator of construct validity. As this dual objective appeared as potentially confusing, we initially decided to frame the paper by focusing on the effect of approach-avoidance behaviours on evaluations. As transparency is a crucial aspect in our work, we already included a note specifying this in the first submission of the manuscript (“*In the preregistration, we presented the whole study as a construct validation of the approach-avoidance induction in virtual reality. To this end, the rationale was to replicate the behaviour-evaluation link and concurrently to gauge the activation of approach-avoidance states. In the current paper, we present on the one hand the measure of the neuropsychological systems as a construct validity assessment because it aims to measure the same construct involved in the manipulation, and on the other we consider evaluations as the main dependent variable as it refers to a distinct construct.*”), but thanks to your feedback we realised that this could be even more confusing for the reader and could be misinterpreted.

Consequently, we reframed the manuscript in line with the original spirit of the preregistration by considering both evaluations and RST-PQ as construct validity criteria. We hope that the primary goal of our research is now clearer for the reader.

Second, the manuscript doesn’t discuss what the results mean for the construct validity of the IVR manipulation. In both Studies 1 and 2, the manipulation did not affect the RST-PQ in the ways that would provide evidence of its validity (i.e., higher BAS and lower FFFS in the approach vs. avoidance condition). The only finding that supports the manipulation’s validity is a significant effect on one of 6 BAS subscales (goal-drive persistence) that was not pre-registered, and did not replicate in Study 2. To me, this suggests that this manipulation did not meet the authors’ criteria for validity. This is certainly useful information for other researchers, especially given the other factors that might make IVR an appealing research tool. Perhaps future research could further investigate its validity, or modify the manipulation in order to improve its validity. Here, however, there is no reason to think that the manipulation is doing what it is supposed to do.

Thank you for this relevant comment. As mentioned in the previous point, we reframed the paper to focus on the construct validity question and thus we tailored our conclusions accordingly: our IVR operationalisation did not meet our construct validity criteria (i.e., both on evaluations and RST-PQ). In the general discussion, we outlined potential explanations for not obtaining the expected effects and we also discussed the choice of construct validity criteria (although we specified that they were chosen because they appeared to be the a priori best criteria).

Finally, the manuscript interprets some of the effects on evaluations (e.g., trait ratings on warmth-related items in Study 2), as evidence for a behavior-attitude link. Reviewer 1 has a slightly different take than me here, and suggests that you could draw firmer conclusions given that you did find some evidence consistent with pre-registered predictions about the effect of the manipulation on trait ratings. I would agree, except I see the lack of validity as something that undermines the ability to draw any further conclusions about the relationship between approach-avoidance behaviors and other constructs (because we can’t be confident that approach-avoidance behaviors have been captured). (As a side note, it was unclear to me whether the effect of the manipulations on warmth-related trait ratings was pre-registered – see point 3).

Thanks to your general feedback about the claim of the paper, we believe this issue is solved in the revised version of the manuscript.

All of this is to say, it seems that the original goal of this research was to evaluate the validity of this IVR approach-avoidance manipulation. This is a valuable goal, and the studies provide useful evidence; it seems that the present findings suggest a lack of validity. I think this should therefore be the main conclusion of the manuscript, and the authors should refrain from interpreting the relationship between approach/avoidance behaviors and evaluations.

We hope that the new version of the manuscript as well as the conclusions and the discussion around the documented lack of validity fits better with your recommendation.

1. Related to the above point, Reviewer 2 and I wondered if the RST-PQ is the best tool to gauge the activation of neurophysiological systems supposed to underlie approach-avoidance behaviors. As the authors note, this measure was initially intended to capture traits or dispositional sensitivities. Although the authors also say that these scales may also work as state measures, their use in this manner does not seem to be well-established. I am hesitant to suggest that the authors spend more time discussing these limitations, because I think it is important to communicate to readers that this was the measure chosen, a priori, as the primary indicator of validity. In other words, it could seem like biased assimilation to critique the assessment of validity after knowing that assessment yielded null results. Perhaps there is a way, then, of raising some limitations of the RST-PQ, while also communicating that this was nevertheless the measure selected as the best indicator of approach-avoidance neurophysiological systems.

Thank you for this insightful comment. We further developed the reasons that led us to choose the RST-PQ in order to measure approach-avoidance systems activation instead of other scales like the BIS/BAS scales of Carver and White (1994). Actually, the RST-PQ better fits with the latest version of the Reinforcement Sensitivity Theory (revised RST; Gray & McNaughton, 2000) that underlies these scales. As comparable RST scales succeeded in capturing contextual variations in approach-avoidance systems, we believed that the RST-PQ would be a good candidate. We specified this rationale in the introduction (p. XX):

“*Second, we gauged the activation of the neuropsychological systems supposed to underlie approach-avoidance behaviours with the Reinforcement Sensitivity Theory Personality Questionnaire (RST-PQ, Corr & Cooper, 2016). Although the RST-PQ is initially intended to capture dispositions that reflect regularities in the operation of BAS and FFFS (Corr & Cooper, 2016; Corr & Krupić, 2017; Corr & McNaughton, 2012), comparable measurement tools of these dispositions (e.g., BIS [Behavioural Inhibition System]/BAS scales, Carver & White, 1994) show sensitivity to contextual influences (Smith & Bargh, 2008; Haeffel, 2011). We therefore decided to rely on such measures to capture contextual activation of these systems. We chose the RST-PQ over other scales as the RST-PQ affords the distinction between the FFFS, governing active avoidance, and the BIS, governing passive avoidance (Gray & McNaughton, 2000)*”.

However, a recent study (which was not published when we conducted our own studies) suggests that the RST-PQ shows moderate fluctuations across situations, reflecting state approach-avoidance systems activation, but is better able to capture stable interindividual differences (Vecchione & Corr, 2021). There is no such analysis for other similar scales so it is not possible to compare their sensitivity to capture contextual variations. It is possible that the RST-PQ is not the best measure as we initially thought. A measure which is both in line with the revised RST and able to capture state variations in the activation of approach-avoidance systems is still needed. We decided to mention this in the discussion as a potential limitation that should be considered in future research:

“*As a second construct validity indicator, we decided to measure approach-avoidance neuropsychological systems activation. To this purpose we relied on the RST-PQ (Corr & Cooper, 2016) assuming it to be sensitive to contextual variations as other related scales (Carver & White, 1994). A recent study however reveals that the RST-PQ shows moderate fluctuations across situations and mostly captures stable interindividual differences (Vecchione & Corr, 2021). We are not aware of comparable information for other related scales that would have indicated better sensitivity to contextual variation, but this remains possible. If this is the case, the RST-PQ may probably not be the best measure as we initially thought. Nevertheless, one finding suggests that the IVR operationalisation of approach-avoidance could induce variation on this measure (i.e., the GDP component of the BAS in Experiment 1). This effect should be taken with caution as not replicated but might suggest that the RST-PQ could capture state variations and therefore is encouraging regarding our novel approach-avoidance operationalisation. Future research should examine this issue further for instance by relying on a measure of state variations in the activation of approach-avoidance systems.*”

1. Although the pre-registration was clear, and I believe the authors made an effort to ensure that they distinguished pre-registered from non-pre-registered analyses, there are still points that could be clearer. For instance, the effect of condition on warmth-related trait ratings in Study 2 is described in the “Complementary Analyses” section, but it was included in the pre-registration. (This effect was also described as “non significant” in the results section, but “significant” in the discussion section, and has a listed p-value of .04).

Thank you for this comment. We provide some clarifications. Concerning the status of warmth-related traits ratings analysis, we deem it important to keep its status as a non-pre registered analysis. Indeed, we preregistered traits ratings analyses in terms of overall positivity. More precisely, the planned evaluation score was based on the mean of traits ratings (with reversed negative traits ratings). A higher value on this evaluation score would reflect a more positive evaluation. Hence, we did not include warmth traits ratings analyses in the pre-registration. However, we corrected the mistake concerning the significance of the results in the discussion as the p-value exceeds the Bonferroni-adjusted alpha level. Thank you for noticing it.

Reviewer 2 noticed that the “presence” scale was not mentioned in the pre-registration, but was included in the “Confirmatory Analyses” section. And, the effect of condition on the goal-drive persistence subscale does not appear to have been pre-registered, but it is included in the abstract as evidence for the authors’ hypotheses. The results sections should be revised so that it is clear to readers when an analysis was pre-registered and when it was not. If the terms “confirmatory” and “complementary” are still used, they should be defined for the reader. Also, the results of non-pre-registered analyses should not be included in the abstract.

Thank you for this suggestion. We made some modifications in order to optimise the presentation of the statistical analyses. First, we renamed the “Confirmatory analyses” and “Complementary analyses” sections into “Preregistered analyses” and “Exploratory analyses” sections which are more commonly used terms. Then we moved the statistics for presence (and sickness due to VR) in the exploratory analyses section. As you mentioned, the analyses on these measures were not preregistered. Actually, comparing the grand mean for presence to the midpoint of the scale does not guarantee that the level is sufficient as no standardised benchmarks exist in the litterature. Therefore, we merely reported the descriptive statistics to suggest that our IVR settings did not seem to generate alarming levels of (lack of) presence and sickness due to VR. We believe that this is an improved way of presenting the results (p. XX and XX):

“*We measured levels of presence and sickness due to VR. In the literature, no standardised benchmarks exist for these indicators. Nevertheless, overall participants did not seem to report an excessively low level of felt presence (M = 3.14, SD = 0.71; midpoint of the scale = 3) nor an excessively high level of sickness due to VR (M = 1.17, SD = 0.37; the scale ranges from 1 to 4).*”

*“As in Experiment 1, we gauged the participants’ IVR experience through the levels of presence and sickness. Overall, and as in Experiment 1, participants did not seem to report an excessively low level of presence (M = 3.05, SD = 0.72; midpoint of the scale = 3) nor an excessively high level of sickness due to VR (M = 1.14, SD = 0.38; ; the scale ranges from 1 to 4).*”

Finally, as recommended, we deleted from the abstract the results on the Goal-Drive Persistence scale.

1. Reviewer 1 raised some concerns, that I shared, about the strength of past research investigating the effect of approach-avoidance behaviors on evaluations. It could be helpful to look into some of this work in more detail, and explain weaknesses or limitations wherever warranted.

Thank you for raising this concern.

First of all, we provided additional evidence of the effect of approach-avoidance on evaluations to emphasise that this is not an isolated effect (p. XX):

“*For example, in the seminal work of Cacioppo and colleagues (1993), individuals evaluated ideographs as more positive when seen during arm flexion (i.e., as when pulling a stimulus closer to the self: approach), than when seen during arm extension (i.e., as when pushing a stimulus away from the self: avoidance). This suggests that approach and avoidance behaviours can serve as rudimentary determinants of evaluations. Extending this reasoning, research investigated how the repetition of approach (vs. avoidance) behaviours in response to stimuli contributes to the formation (Huijding et al., 2011; Hutter & Genschow, 2020; Laham et al., 2014; Van Dessel et al., 2018; Woud et al., 2008; Woud et al., 2013; Zogmaister et al., 2016) and the change of attitudes (Jones et al., 2013; Kawakami et al., 2008). This effect applies to various types of stimuli ranging from animals (Huijding et al., 2011; Jones et al., 2013), consumer goods (Zogmaister et al., 2016) to more abstract stimuli like colours (Hutter & Genschow, 2020), shapes (Laham et al., 2014), pronounceable nonwords (Van Dessel et al., 2018) or abstract domains (mathematics, Kawakami et al., 2008).*”

We were also more explicit about the fact that there is some degree of inconsistency in the literature about this effect (actually, it is this inconsistency that motivated us to embark in the development of a better operationalisation of approach-avoidance behaviours). To this aim, we added the following paragraph in the new version of the manuscript (p. XX):

“*However, there is some degree of inconsistency regarding the conclusions about the effect of approach and avoidance on evaluations. First, some studies failed to replicate the effect of approach and avoidance on evaluation (Krishna & Eder, 2018). Regarding social evaluation specifically, repeatedly pulling pictures of faces towards oneself (i.e., approach) with a joystick did not influence evaluations of these faces as compared to pushing pictures of faces away (i.e., avoidance, Vandenbosch & De Houwer, 2011). Second, some research questions the necessity to perform genuine approach and avoidance behaviours in order to obtain an evaluative effect. Although the available evidence points to the existence of an effect of performing these genuine behaviours, it appears to be weak (Van Dessel et al., 2016; Van Dessel et al., 2020). This research suggests that the emergence of the effect of approach and avoidance is heavily conditional on other variables and therefore calls for further examination of their operationalisation (Schwarz & Clore, 2016)*”.

Then, following the first suggestion of Reviewer 1, we mentioned the general reasoning underlying the evaluative influence of approach-avoidance behaviours (see p. XXX):

“*The general idea is that, due to the encoded relation between approach-avoidance behaviours and evaluations, when approach and avoidance behaviours are enacted, the corresponding evaluation is assumed to extend to encountered stimuli (Cacioppo et al., 1993; Eder & Klauer, 2009; Strack & Deutsch, 2004; Van Dessel et al., 2019)*”.

Doing so, we believe that the weaknesses and limitations of previous research are brought to light more straightforwardly. They did not sufficiently consider the full set of multi-sensory and contextual cues at stake during social interactions and thus, appear quite non prototypical of social behaviours. This may hinder the reactivation of the encoded relation between approach-avoidance behaviours and evaluations which is supposed to underlie the effect. We added a sentence (p. XX) to explain this.

“*Due to their lack of mundane realism, past approach-avoidance operationalisations appear relatively non prototypical of interpersonal behaviours. Therefore, it may be more or less difficult to solicit the encoded relation between these behaviours and evaluations, which is supposed to underlie the effect, thereby contributing to variability in the findings. As a consequence, previous work may offer a limited or twisted picture of how approach-avoidance behaviours influence evaluations*”.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I realize this may be more substantial revision than you are willing to conduct, but I hope you will consider it. I look forward to receiving your revision.

Thank you very much for all these insightful and constructive comments. We realised that the required revisions were necessary to facilitate the reading and the understanding of our work.

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 contact the editorial office at editorialoffice@collabra.org.

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Alexa Tullett

Reviewer 1:

Thank you for the opportunity to review the manuscript, “The (Virtual) Reality of Intergroup Interactions: Approach-Avoidance behaviors and Group Evaluations” for possible publication in Collabra: Psychology. This paper presents two preregistered experiments investigating the influence of approach/avoidance behaviors on the evaluation of social groups within a virtual reality context. I was impressed with how thorough the Methods and Results were. The authors reported all of their DV’s, even the ones that were non-significant, which creates confidence in the validity of these results. Generally, this paper is a fine example of transparent and preregistered work. I do have a few questions and concerns, which I detail below.

1. The effect of attitudes (liking vs. disliking) on behavior (approach vs. avoid) is well established, but the reverse direction – the effect of approach/avoidance behavior on liking/disliking attitudes – is more novel. The authors build the current investigation on past research showing that simple approach and avoidance behaviors (e.g., pulling a joystick towards oneself) are sufficient to influence one’s evaluation of stimuli. However, I’m not sure how strong this foundation of research is. First of all, what is the proposed mechanism that causes approach/avoidance behaviors to influence evaluations of stimuli? Is it like the mere exposure effect on liking? Some of the research cited (e.g., Van Dessel et al., 2020) find only weak effects, or effects on implicit rather than explicit measures. Because the authors propose to extend this work into a new setting (virtual reality) with more complex stimuli, then the initial effect should be robust and well-established. It would be helpful if the authors spent a bit more time discussing the original effect (that simple approach/avoidance behaviors influence stimulus evaluation) in the Introduction to adequately set up the leap into a new domain and their new hypotheses.

Thank you for requiring this specification. We modified the introduction to fit your comment.

First, we mentioned additional work about the effect of approach-avoidance on evaluations to illustrate the fact that this effect applies to different types of stimuli/situations (e.g., words, animals, consumption products).

Here, is the corresponding paragraph (p.XX):

“*For example, in the seminal work of Cacioppo and colleagues (1993), individuals evaluated ideographs as more positive when seen during arm flexion (i.e., as when pulling a stimulus closer to the self: approach), than when seen during arm extension (i.e., as when pushing a stimulus away from the self: avoidance). This suggests that approach and avoidance behaviours can serve as rudimentary determinants of evaluations. Extending this reasoning, research investigated how the repetition of approach (vs. avoidance) behaviours in response to stimuli contributes to the formation (Huijding et al., 2011; Hutter & Genschow, 2020; Laham et al., 2014; Van Dessel et al., 2018; Woud et al., 2008; Woud et al., 2013; Zogmaister et al., 2016) and the change of attitudes (Jones et al., 2013; Kawakami et al., 2008). This effect applies to various types of stimuli ranging from animals (Huijding et al., 2011; Jones et al., 2013), consumer goods (Zogmaister et al., 2016) to more abstract stimuli like coulours (Hutter & Genschow, 2020), shapes (Laham et al., 2014), pronounceable nonwords (Van Dessel et al., 2018) or abstract domains (mathematics, Kawakami et al., 2008).*”

Second, we added a paragraph concerning the proposed reasoning underlying the effect of approach-avoidance behaviours on evaluations. We kept it relatively general, focusing on the general principle underlying the evaluative influence of approach-avoidance. This was done as we believe that a detailed outline of the different theoretical accounts is beyond the scope of this paper. More specifically, we wrote (p.XX):

“*The general idea is that, due to the encoded relation between approach-avoidance behaviours and evaluations, when approach and avoidance behaviours are enacted, the corresponding evaluation is assumed to extend to encountered stimuli (Cacioppo et al., 1993; Eder & Klauer, 2009; Strack & Deutsch, 2004; Van Dessel et al., 2019)*”

Third, as you mentioned, within the literature on the evaluative influence of approach-avoidance, there is some variability in the findings. The effect is sometimes not replicated or appears as relatively weak. Actually, one potential explanation for this variability came from a mis-operationalisation of the focal behaviours. We already outlined the various problems with previous operationalisations in the section “1.1.3. Limitations of the Examination of the Approach-Avoidance Influence on Social Evaluations”. However, in this version we made more explicit our reasoning about the link between this mis-operationalisation and the observed variability (p. XX):

“*However, there is some degree of inconsistency regarding the conclusions about the effect of approach and avoidance on evaluations. First, some studies failed to replicate the effect of approach and avoidance on evaluation (Krishna & Eder, 2018). Regarding social evaluation specifically, repeatedly pulling pictures of faces towards oneself (i.e., approach) with a joystick did not influence evaluations of these faces as compared to pushing pictures of faces away (i.e., avoidance, Vandenbosch & De Houwer, 2011). Second, some research questions the necessity to perform genuine approach and avoidance behaviours in order to obtain an evaluative effect. Although the available evidence points to the existence of an effect of performing these genuine behaviours, it appears to be weak (Van Dessel et al., 2016; Van Dessel et al., 2020). This research suggests that the emergence of the effect of approach and avoidance is heavily conditional on other variables and therefore calls for further examination of their operationalisation (Schwarz & Clore, 2016)*”.

And p. XX

“*Due to their lack of mundane realism, past approach-avoidance operationalisations appear relatively non prototypical of interpersonal behaviours. Therefore, it may be more or less difficult to solicit the encoded relation between these behaviours and evaluations, which is supposed to underlie the effect, thereby contributing to variability in the findings*”.

Following the editor’s recommendations we reframed the paper to focus on the issue of construct validity. Reframed this way, the use of IVR to operationalise approach-avoidance appears clearly as one insightful solution to address the mis-operationalisation issue (rather than a new domain to which we want to extend the study of the original effect).

Concerning the distinction between explicit and implicit measures, we did not develop this point further in the manuscript. Indeed, there are inherent conceptual limitations associated with the explicit-implicit distinction (Corneille & Hutter, 2020). And we do not have strong theoretical and empirical reasons to expect the effect more on one aspect than on the other (i.e., the effect appears sometimes on what is called explicit and sometimes on what is called implicit measures, or even both). Therefore, we merely detailed this reasoning in a footnote (p. X) but avoided thoroughly discussing this distinction applied to approach-avoidance behaviours.:

“*We decided to include both direct and indirect measures as the effect has been obtained on both measures with fictitious groups (Van Dessel et al., 2018), and as we have no a priori theoretical reason to favour one measure over the other.*”

1. Relatedly, what was the primary aim of the present investigation? Was it to establish a behavior-attitude link, that is – to replicate and extend past research that approach/avoidance behaviors influence social evaluations? Or is the primary aim to validate a novel immersive virtual reality (IVR) setting to examine approach/avoidance behaviors in social settings? The authors lean pretty heavily on the latter, concluding that “we advocated the use of IVR as an appropriate and insightful tool to expand the scope of investigation concerning the influence of approach-avoidance behaviours on group evaluations.” However, it seems to me that first establishing the relationship between approach/avoidance behavior and social evaluations is quite important.

Some of the main take-aways were unclear to me. For instance, some of the results did support their hypotheses that approach/avoidance behaviors in the IVR setting would influence approach motivation (Experiment 1) and positive social evaluations (Experiment 2). Yet, the authors primarily conclude that the hypotheses were not supported and that the main take-away is that IVR could be a useful tool for investigating approach/avoidance behaviors in social settings in the future. The results were messy, of course, and many effects were non-significant. However, because the authors preregistered their predictions it seems to me that the predictions that were supported should be more noteworthy. I do appreciate the caution with which the authors interpret their results, but I think that the pattern of results deserves some mention in the conclusions, beyond just the novelty of using IVR.

Following yours as well as the editor’s comments, we restated more clearly the primary aim of the present work, which is to validate a novel IVR operationalisation of approach-avoidance behaviours. In line with our original thinking, this goal stems from the observation that past studies on the evaluative influence of approach-avoidance behaviours in interpersonal contexts relied on overly decontextualised operationalisations. Nonetheless,the evaluative influence of approach-avoidance is used as a criterion for construct validity in the present contribution. By recentering the manuscript around the construct validity issue, we can only conclude from the following results that, according to our criteria, our IVR operationalisation is not valid and that future research should further pursue and take up this challenge.

1. Relatedly, the authors focus on p-values when determining significance, and they use a Bonferroni correction to account for multiple tests. This type of correction is quite conservative and makes it very difficult to find significant results. Would it be better to focus on effect sizes and confidence intervals? In fact, in Experiment 2, the effect of approach/avoidance behavior on trait ratings was non-significant, but later in the discussion the authors describe this effect as accounting for “2% of the variance in one measure of group evaluations (i.e., trait ratings).” In other words, when focusing on effect sizes rather than p-values it sounds as if the behavior manipulation did have an effect on trait ratings. It would be helpful if the authors were more consistent with how they evaluated their results. And, because they went to the trouble to preregister their predictions, it might be helpful to focus on effect sizes rather than Bonferroni-corrected p-values. This choice will obviously be up to the authors and editor.

Thank you for this comment. However, we decided to keep p-values in the manuscript rather than focusing only on effect sizes (ES) and confidence intervals (CI). Although p-values have their pitfalls, they allow a pragmatic decision on the existence of an effect. On the other hand, ES and CI provide information on the importance and precision of the effect. As we initially aimed to rule on the existence of the effects of approach-avoidance behaviours on an important number of measures, we believe that the use of corrected Bonferoni p-values still brings relevant information while avoiding the risk of Type 1 errors.

Reviewer 2:

Overall, I appreciated the attempt of the authors to employ a novel tool to translate basic findings from past research to a more ecologically valid and immersive experience. As virtual reality tools develop, laboratory studies will continue to be able to more realistically simulate a real-world situation that we hope to generalize findings to. I agree wholeheartedly with the paragraph on Page 26 in the general discussion that advocates for use of an enriched paradigm that goes beyond minimalist laboratory settings. These results, although not supportive of the hypotheses, will be important for future researchers to consider as more studies with virtual reality are undertaken to answer questions of human interaction. I found the discussions to be appropriately tempered of what can be learned from these studies and not too focused on exploratory findings that may or may not be spurious.

Please see the below concerns that I feel need to be addressed before the paper would be suitable for publication:

1. The term “Afro-Americans” used on Page 3 is outdated and should be replaced by “Black” (see APA Style: <https://apastyle.apa.org/style-grammar-guidelines/bias-free-language/racial-ethnic-minorities>)

Thank you for your feedback. Indeed, we missed this recommendation. We have changed the term accordingly.

1. Would it be possible for the researchers to include links to a video of the virtual environment embedded in the paper, perhaps posted on the OSF? While Figure 1 is helpful in understanding the paradigm, the description in section 1.2 would be much aided by a video representation of how their virtual character moves in the environment.

This is an excellent suggestion. We created a video of an approach condition trial and an avoidance condition trial in the virtual environment that we made publicly available through the OSF page of the project. We specified this p. XXX

“*A video of an approach condition trial and an avoidance condition trial in the virtual environment is available in the* [*OSF project*](https://osf.io/9jb8a/?view_only=eb885e8942654089ba7cff2fd880ed98)”.

1. In section 2.1.3.1 on Page 11, the authors state that “For each encounter, participants had to consider their impression of the target individual”. Did the participants report their impression after each individual, or were these simply instructions for the participant to be considering their impressions as the task proceeded?

Thank you for this comment that helped us clarify the procedure. Participants had to bear in mind these instructions as the task unfolded and were prompted to generate an impression about the target individual in their mind’s eye (not via self-report). This was done to put participants in an evaluative mindset and increase the evaluative meaning of approach-avoidance (for a similar procedure, see the seminal work by Cacioppo et al., 1993). We modified the sentence in accordance:

“*In order to increase the evaluative connotation of approach-avoidance behaviours we activated an evaluative mindset (Cacioppo et al., 1993): For each encounter, participants had to consider in their mind their impression of the target individual.*” (p. XX)

1. Were the 16 different virtual individuals of the same race/ethnicity, weight, hair color, etc.? Were the same virtual characters presented to each participant, and in the same order?

Thank you for pointing out the missing procedural details.. Indeed, the 16 virtual individuals were of the same race/ethnicity, of comparable body mass index and age. However, they varied in terms of some facial characteristics and in terms of hair and eye colour. We included a screenshot of each virtual individual in OSF (<https://osf.io/9jb8a/?view_only=eb885e8942654089ba7cff2fd880ed98>) for transparency.

In order to clarify this in the manuscript, we added the following note in the procedure (p. XX):

“*All virtual individuals were white, of comparable body mass index and age but varied in terms of facial characteristics, hair and eye colour (see the OSF project for virtual individuals screenshots).*”.

This note also clarifies the fact that all participants encountered the same virtual characters.

Concerning the presentation order, it was randomly generated for each participant.

We clarified this in the manuscript:

“*After completing these training trials with the same virtual individual, participants randomly encountered 16 different virtual individuals*”(p. XX).

1. The RST-PQ was employed as a measure to see if the manipulation of body posture would affect ratings of the BAS and FFFS. However, the RST-PQ was intended to serve as a personality measure of trait rather than state activation of the approach and avoidance systems. Is there past precedence for using the RST-PQ as a measure of state activation of these personality systems? If not, do the authors believe that it is appropriate to use a measure with general trait-level statements to measure state activation of a particular system? Why was the RST-PQ chosen over other questionnaires (e.g. Carver and White’s BIS/BAS)?

Thank you for this comment, we agree that this point needs further clarification. We detail our reasoning in the Overview section (p.XX):

“*Second, we gauged the activation of the neuropsychological systems supposed to underlie approach-avoidance behaviours with the Reinforcement Sensitivity Theory Personality Questionnaire (RST-PQ, Corr & Cooper, 2016). Although the RST-PQ is initially intended to capture dispositions that reflect regularities in the operation of BAS and FFFS (Corr & Cooper, 2016; Corr & Krupić, 2017; Corr & McNaughton, 2012), comparable measurement tools of these dispositions (e.g., BIS [Behavioural Inhibition System]/BAS scales, Carver & White, 1994) show sensitivity to contextual influences (Smith & Bargh, 2008; Haeffel, 2011). We therefore decided to rely on such measures to capture contextual activation of these systems. We chose the RST-PQ over other scales as the RST-PQ affords the distinction between the FFFS, governing active avoidance, and the BIS, governing passive avoidance (Gray & McNaughton, 2000).*”

1. The preregistration did not mention the plans for the “Presence” scale. In the confirmatory analyses section (Page 14), it begins by mentioning that the mean was significantly above the midpoint of the scale, but I did not see this analysis mentioned in the preregistration. Therefore, I do not believe it should be in the confirmatory analysis section, and a justification for testing against the midpoint as a measure of feeling presence should be given. This comment also applies to Experiment 2 (Page 20).

Thank you for your comment. Indeed, analyses on the feeling of presence was not-preregistered and thus cannot appear in the Confirmatory analyses section. Therefore, we moved the relevant statistics for presence (and sickness due to VR) in the Exploratory analyses section (we renamed the “Complementary analyses section” into “Exploratory analyses section” which is a more common label). To this day and to our knowledge, no standardised benchmarks exist in the litterature that define a necessary or sufficient threshold for the level of presence in an IVR paradigm. For this reason, we merely reported the descriptive statistics to suggest that our IVR settings did not seem to generate alarming levels of (lack of) presence and sickness due to VR. We believe that this way of presenting the results is more optimal (p. XX and XX):

“*In the literature, no standardised benchmarks exist for these indicators. Nevertheless, overall participants did not seem to report an excessively low level of felt presence (M = 3.14, SD = 0.71; midpoint of the scale = 3) nor an excessively high level of sickness due to VR (M = 1.17, SD = 0.37; the scale ranges from 1 to 4).*”

*“As in Experiment 1, we gauged the participants’ IVR experience through the levels of presence and sickness. Overall, and as in Experiment 1, participants did not seem to report an excessively low level of presence (M = 3.05, SD = 0.72; midpoint of the scale = 3) nor an excessively high level of sickness due to VR (M = 1.14, SD = 0.38; ; the scale ranges from 1 to 4).*

1. In section 2.2.2.3 (Page 19), please list the 6 positive and 6 negative traits used.

Thank you for this comment, we list the corresponding traits in the Traits-ratings task section:

*“Then, using a pencil and paper questionnaire, participants evaluated the group encountered in the IVR setting (i.e., “According to you, to what extent the Alesophiles/Udesophiles are …”) on six positive (i.e., warm, intelligent, honest, competent, cultivated, sociable) and six negative (i.e., lazy, snob, unpleasant, stupid, shallow, boring) traits using on a 10-points response scale ranging from 1: not at all to 10: extremely (see the OSF project for the material).*” (p.XX)

1. In Experiment 2, the authors state in the results section that “Although non significant, results revealed that participants evaluated the encountered group as warmer in the approach than in the avoidance condition…”, yet in the discussion they state, “This positive effect of approach is significant on the warmth dimension, which is considered to be the primary dimension in social judgment…”. The p value was .04, which would be traditionally considered statistically significant but not in light of the Bonferroni-adjusted alpha level. Please reconcile the language in these two sections to reflect whether a Bonferroni-adjusted alpha level was used for the exploratory analyses.

Thank you for notifying us of this mistake. Indeed, according to a Bonferroni-adjusted alpha level the effect of approach-avoidance behaviours on warmth judgment is not significant. We deleted this sentence from the discussion (p. XX).

We want to thank you and the reviewers again for the excellent feedback on our manuscript. We believe our updated manuscript is stronger than the original manuscript. We hope we have adequately addressed all of the concerns raised by you and the reviewers, and that our manuscript is now suitable for publication.

Best regards

**Editor Final Decision:** Accept

Mar 29, 2022

Dear Ivane Nuel,

I have now had a chance to read over your manuscript “The (Virtual) Reality of Social Approach-Avoidance Behaviours: Operationalisation Development and Construct Validity Testing”, along with the letter describing the changes you made. Thank you for your responsiveness to the concerns that the reviewers and I raised. I am happy to say that your paper is now officially accepted for publication in Collabra: Psychology. Congratulations on this excellent work, I think it will make an important contribution to the literature and I look forward to seeing it published! I hope your experiences with Collabra: Psychology have been positive and that you will continue to consider it as an outlet for your work.

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

You will be receiving separate correspondence regarding any production and technical comments, data deposits, as well as publication charges. We work with the Copyright Clearance Center to process any applicable APC charges. Please note that your APC transaction must be completed before your article gets published.

You will have an opportunity to check the page proofs before we publish your article. Thank you again for publishing in Collabra: Psychology.

Sincerely,
Alexa Tullett