**Peer Review and Communication History**

**MS Title**: Time spent playing two online shooters has no measurable effect on aggressive affect

**Author Names**: Niklas Johannes, Matti Vuorre, Kristoffer Magnusson, Andrew K. Przybylski

**Submitted:** Jan 26, 2022

**Editor First Decision**: Revise & Resubmit

Mar 9, 2022

Dear Niklas Johannes,

I have now received all reviews of your manuscript, “Time spent playing two online shooters has no measurable effect on aggressive affect” 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.

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. The reviewers and I appreciated the way you addressed methodological limitations in the discussion. Nevertheless, one challenge with secondary data is that analyses, and subsequent claims, don’t have the benefit of being constrained by pre-registration. Relevant to this concern, Rohrer (who signed her review) requested that you provide alternative operationalizations of the effect of interest and the correlation between random intercepts. Including these could add robustness to the findings you’re reporting.
2. The reviewers also make excellent points about ways that you could more fairly characterize the debate about video game violence (Ferguson, who signed his review), more clearly explain the design (Rohrer) and more effectively address issues like small effect sizes (Ferguson) and selection effects (Rohrer).
3. The materials file (i.e., the doc version of the Qualtrics study) posted on OSF is very difficult to read. Most pages have one row of text on them, and it’s quite tedious to get to the relevant information (e.g., scale items). It would be helpful to provide a more legible version of this information.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. 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.

I appreciate the opportunity to review this paper which is a secondary analysis of previous data investigating the impact of violent game play on aggression.

The article has many clear strengths. It is open data and methods, the study is rigorous and the authors have a clear conception of effect size interpretation. The authors address the secondary nature of the data clearly so I am not too worried about that.

I have only a few small suggested edits, after which I believe the manuscript warrants publication.

First, the authors claim that the prior literature offers “…little actionable insights.” I’m not sure that’s quite the right way to put it. Perhaps its better to say that it hasn’t resulted in consensus in the field? Surely the almost universal nature of null findings in preregistered studies offers somewhat actionable insights? I suppose it depends on the working definition of “actionable” here if the argument is for society to *not* take action.

Likewise the authors talk about researchers “entrenched in two camps”. There’s probably some technical truth to that but also may be a bit simplistic (surely some researchers have in-between or nuanced views). It may come across a bit…cliche, dramatic, overworn as a descriptor too. I don’t know if I find that narrative terribly helpful other than for the popcorn and soda crew, or perhaps I’m just becoming bored with it. It also risks making authors who refer to it as self-congradulatorily “above it all” which is seldom truly the case.

At one point the authors reference the Marthur & VanderWeele meta of metas, but I’m not sure that piece necessarily fits well with what they’re trying to do. It was actually a really weird article that…I’m not sure told us anything we didn’t already know and ignored really important issues (like methodological concerns and triviality in effect sizes). Aaron Drummond and Jim Sauer had a really good critique of the article that was posted online here: <https://psyarxiv.com/xms5u/>. At very least those criticisms of Marthur should be cited if they cite the article itself.

Lastly, they authors discuss the triviality of tiny effect sizes which is an important point. This has recently been quantified and that may help bolster the authors’ arguments. See Ferguson & Heene (2021 I think?)

Congrats to the authors on some nice work and I hope the editors will give this serious consideration.

Signed,
Chris Ferguson

**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.

In this manuscript, the authors investigate the effects of playing online shooters on aggressive affect by analyzing three waves of longitudinal data provided by video game companies. All things considered, it was a pleasure to read this manuscript – the data are quite unique and definitely relevant to the debate at hand, the analyses are reported in a very clear manner, and the whole text is well written. Most of the concerns I had while reading the paper were actually addressed in the discussion section, which I believe hasn’t happened to me before. So, this will be a brief\* review (\*within-subject standardized measure of brevity).

First, this is the biggest issue – I am not sure whether I 100% understood your design/analysis. So, you essentially aggregated video gaming time over the two weeks leading up to the affect measurement. But then those two weeks and the subsequent affect rating are considered “the same time point” – so, you’re actually checking whether how much people played in week 1 and 2 predicts their affect at the end of week 4, controlling for how much they played in week 3 and 4. Did I get that right? Maybe some sort of chart would be helpful to visualize what you aggregated how, and then what is supposed to predict what. If I understood your analyses correctly, I would like to see alternative operationalizations of the effect of interest. For example, assuming that somebody filled out the survey on Sunday, I’d like to see whether how much they played ca. Monday to Saturday predicts aggressive affect. You may even consider something like a 1-day lag. This may require you to move away from the RI-CLPM approach – which I think would be no issue, a simple fixed effects regression actually seems quite appropriate for this research question. Unless I’m missing something, given that there is not much going on in the data, I wouldn’t expect the type of dynamics that mess with fixed effects models (see Imai & Kim, 2019: <https://imai.fas.harvard.edu/research/files/FEmatch.pdf>) – and in any case some of these dynamics would mess with your RI-CLPM as well. Apart from that, in the case of an RI-CLPM, I’d personally always be interested in the correlation between the random intercepts; if substantial and positive, it (1) highlights how between person confounding might bias other studies and (2) potentially also reassures us that the aggressive affect measure taps into something.

Second, smaller points: How did you deal with attrition, did you simply limit analyses to complete cases?

In the discussion (which I quite like, overall), you bring up the issue of generalizability. I think if the matter is discussed in such general terms, people tend to shrug it off – every study in psych suffers from generalizability problems! And you mention selection bias and attrition at another point, but again in very general terms. Here, I believe, a central concern is that only a very minor fraction of the potential population of interest (players of Apex Legends, Outriders) actually decided to participate, and most dropped out. It seems quite plausible to assume that (1) the video game/aggression link varies between people (I think it’s even mentioned in the intro) and (2) this effect heterogeneity correlates with features that determine whether people participate in such surveys and whether they stick with them. One might think that generally more well-adapted people are not really negatively affected by the games they play (or even use them strategically to relax), and those are the people most likely to participate in such studies. Obviously, this is by no means unique to your study – it’s probably worse in all those lab studies – so there’s no reason to get defensive about it, and there might be added value in making the people in this field aware of the issue in some more detail.

p. 7: “For example, changes in the amount of leisure time may lead to more play, but also less frustration and feelings of anger, thereby biasing a true negative effect toward the null.”: Maybe I’m misunderstanding how you use the term negative effect. But, the confounding you describe would result in a spurious negative correlation between time spent playing and feelings of anger. So, the confounding can (1) amplify a negative causal effect, (2) induce a negative association in the absence of a causal effect, (3) bias a true positive effect toward the null. I don’t see how what you describe could bias a true negative effect toward the null.

p. 7, “Is it unethical for our team to publish on the same data twice? We believe it isn’t. First, the research question we answered here is conceptually distinct from Vuorre et al. (2021). Second, the data set has been public for many months and we explicitly invited other researchers to use it.”: I don’t disagree with the points here, but found it a bit peculiar that you’d bring them up – plenty of people re-use the same data over and over again, without anyone accusing them of salami-slicing. So, I looked at Vuorre et al. (2021), and the concern seems a bit narrower – the measure of anger used in this study is an item of a scale included in the other one, is that correct? That might be more likely to induce accusations of salami-slicing – at least there’s a style of paper in which one would analyze every single item as well as the scale, though if we are being honest, that is not how most people analyze and report their data, and it also leads to some issues in its own right (such papers may not be perceived as part of a certain narrow literature and thus ignored, plus frankly the introductions and discussions can be a mess to write). I think a transparent (and less defensive) way to deal with the redundancy between papers is to simply note that you are focusing on this specific narrow research question in this study, and that there’s Vuorre et al. (2021) which investigates effects on well-being more broadly. This might fit well on page 4, when you say “We used this item as our central outcome variable” (which made me think that you’d also look at the other items in this study, given that “central” outcome implies the existence of non-central outcomes). Then, in the discussion, you can simply conclude with an invitation to other researchers to work with your data.
Lastly, for full transparency, I’d like to declare that I did not check whether the raw data in combination with the provided code actually reproduce the results. I think I was invited to comment on the causal inference part of this; plus, I tend to assume that authors who conscientiously document their analysis pipeline also manage to create reproducible code (which may of course be a mistaken assumption, but at least everything is out there to check for those who are motivated to refute the authors).

Best regards,
Julia Rohrer (I sign all my reviews)

**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**
Apr 5, 2022

Dear Professor Tullet,

Thank you for giving us the opportunity to revise our manuscript. We appreciate the detailed and helpful feedback both from you and the two reviewers—we’re aware that reviews of this caliber are rare. We implemented your and their feedback and believe the manuscript is much improved as a result.

Besides directly addressing all points, we

1. Rewrote the introduction to provide a more nuanced background of the research done so far.
2. Added details on the design in several places in the Method section.
3. Discussed self-selection in much more concrete terms.

Thank you and on behalf of our team,

Niklas Johannes

# Your comments

1. The reviewers and I appreciated the way you addressed methodological limitations in the discussion. Nevertheless, one challenge with secondary data is that analyses, and subsequent claims, don’t have the benefit of being constrained by pre-registration. Relevant to this concern, Rohrer (who signed her review) requested that you provide alternative operationalizations of the effect of interest and the correlation between random intercepts. Including these could add robustness to the findings you’re reporting.

We agree with all points Dr. Rohrer raised and included the correlations between random intercepts as the last paragraph in the results. To cut to the chase: They don’t suggest much between-person confounding for other studies. As for trying out different lags: That was a result of us not describing the design properly. We didn’t make it clear that aggressive affect was measured with one rating referring to a two-week period. Because affect refers to two weeks, we can’t try out different lags in the sense that Dr. Rohrer recommended. Please see our response to her directly for clarification.

1. The reviewers also make excellent points about ways that you could more fairly characterize the debate about video game violence (Ferguson, who signed his review), more clearly explain the design (Rohrer) and more effectively address issues like small effect sizes (Ferguson) and selection effects (Rohrer).

We agree that the design needed a clearer explanation (see our previous response). Throughout the manuscript, we expanded on the design and made the time frame clearer. If you believe it to be helpful, we’re also happy to include the process figure in our response to Dr. Rohrer. We also removed the somewhat simplistic description of the field as two camps and instead revised the introduction to give a more balanced overview. Furthermore, we followed Dr. Rohrer’s recommendation and replaced abstract discussions of generalizability with concrete discussions of self-selection. Last, we incorporated Ferguson’s suggestion and discuss our effect sizes within the context of “crud correlations” to provide more context for readers.

1. The materials file (i.e., the doc version of the Qualtrics study) posted on OSF is very difficult to read. Most pages have one row of text on them, and it’s quite tedious to get to the relevant information (e.g., scale items). It would be helpful to provide a more legible version of this information.

Thank you for bringing this to our attention. Somehow exporting from Qualtrics doesn’t work that well with OSF preview. I uploaded a version that does.

# Reviewer 1

1. First, the authors claim that the prior literature offers “…little actionable insights.” I’m not sure that’s quite the right way to put it. Perhaps its better to say that it hasn’t resulted in consensus in the field? Surely the almost universal nature of null findings in preregistered studies offers somewhat actionable insights? I suppose it depends on the working definition of “actionable” here if the argument is for society to not take action.

We agree that there is no consensus in the field even though a handful of higher-quality studies are rather conclusive. However, what we meant was that even such higher-quality studies had to rely on suboptimal measures of video game play in lieu of accurate telemetry data. Therefore, even the evidence from these studies provides little actionable inside. That said, we realize that such a conclusion might be interpreted as too strong—in the absence of behavioral data, high-quality analyses of self-reports deliver the best available insights. And these insights indeed suggest to not take action. In the revision, we clarified what we mean in the abstract and removed reference to actionable insights, instead calling them preliminary.

1. Likewise the authors talk about researchers “entrenched in two camps”. There’s probably some technical truth to that but also may be a bit simplistic (surely some researchers have in-between or nuanced views). It may come across a bit…cliche, dramatic, overworn as a descriptor too. I don’t know if I find that narrative terribly helpful other than for the popcorn and soda crew, or perhaps I’m just becoming bored with it. It also risks making authors who refer to it as self-congradulatorily “above it all” which is seldom truly the case.

When writing the introduction, I (Niklas) wanted to keep it as brief as possible whilst giving a reader who knows nothing about the field a historical perspective. Two camps is how I would describe it to someone from a different field. That said, we understand your points. Talking about two camps might do more harm than good by neglecting nuance and making it easy for readers to dismiss our results because of that lack of nuance. In the revision, we toned down the camp framing in favor of a tempered overview, whilst still being accurate about the research field being contentious (a position all authors of this paper share).

1. At one point the authors reference the Marthur & VanderWeele meta of metas, but I’m not sure that piece necessarily fits well with what they’re trying to do. It was actually a really weird article that…I’m not sure told us anything we didn’t already know and ignored really important issues (like methodological concerns and triviality in effect sizes). Aaron Drummond and Jim Sauer had a really good critique of the article that was posted online here: <https://psyarxiv.com/xms5u/>. At very least those criticisms of Marthur should be cited if they cite the article itself.

We read the critique and agree that the analysis is problematic on methodological grounds. We removed the reference from the revised manuscript and phrased the discussion not around inconsistent evidence, but around the quality of that evidence.

1. Lastly, they authors discuss the triviality of tiny effect sizes which is an important point. This has recently been quantified and that may help bolster the authors’ arguments. See Ferguson & Heene (2021 I think?)

Thanks for the pointer. We had not considered the paper in our first draft and included an example from Study 1 in the results. We also briefly interpret our findings in the frame of the crud factor in the discussion, as well as provide more context for why *r* = .10 is a recommended cut-off.

# Reviewer 2

1. First, this is the biggest issue – I am not sure whether I 100% understood your design/analysis. So, you essentially aggregated video gaming time over the two weeks leading up to the affect measurement. But then those two weeks and the subsequent affect rating are considered “the same time point” – so, you’re actually checking whether how much people played in week 1 and 2 predicts their affect at the end of week 4, controlling for how much they played in week 3 and 4. Did I get that right? Maybe some sort of chart would be helpful to visualize what you aggregated how, and then what is supposed to predict what. If I understood your analyses correctly, I would like to see alternative operationalizations of the effect of interest. For example, assuming that somebody filled out the survey on Sunday, I’d like to see whether how much they played ca. Monday to Saturday predicts aggressive affect. You may even consider something like a 1-day lag. This may require you to move away from the RI-CLPM approach – which I think would be no issue, a simple fixed effects regression actually seems quite appropriate for this research question. Unless I’m missing something, given that there is not much going on in the data, I wouldn’t expect the type of dynamics that mess with fixed effects models (see Imai & Kim, 2019: <https://imai.fas.harvard.edu/research/files/FEmatch.pdf>) – and in any case some of these dynamics would mess with your RI-CLPM as well. Apart from that, in the case of an RI-CLPM, I’d personally always be interested in the correlation between the random intercepts; if substantial and positive, it (1) highlights how between person confounding might bias other studies and (2) potentially also reassures us that the aggressive affect measure taps into something.

We could have done a much better job describing the design. Our affect measure was not just for one day, but referred to a two-week window:



At each survey wave (weeks 2, 4, and 6 of the study), participants were asked to report their aggressive affect (as a subset of other well-being items) for **the past two weeks**. For this two-week window, we also obtained behavioral data (aka telemetry). Each of the three waves, then, measured game play and anger during the past two weeks.

So when we talk about cross-lagged effects, it’s indeed telemetry during weeks (0, 2] predicting aggression during weeks (2, 4]. Thank you for bringing it to our attention that the design wasn’t clear. We added several clarifications to the manuscript. If you and the editor think the above figure helps here, we’re also happy to include it the revision.

As for the different lags: The aggressive affect measure spans two weeks, not one day (or “right now”). Therefore, we can’t try different lags as you suggested. We could, of course, predict each two week window of aggressive affect with 1-14 days of telemetry. However, given that there’s no effect of playing 2 weeks, we don’t think playing for anything less than that will have an effect.

However, we agree that adding the correlations between random intercepts is informative. We added a paragraph at the end of the results section describing them in both raw and standardized terms. [We also describe the autoregressive terms that show that there’s little consistency in aggressive affect and telemetry.]

1. Second, smaller points: How did you deal with attrition, did you simply limit analyses to complete cases?

We defined our population of interest as active players. Therefore, we only included those participants in the analysis who (1) had at least one wave of self-reported data and (2) had played for at least one wave (technically, played in the two weeks preceding a survey, see answer above). Therefore, the participants on whose data we based the analysis had a maximum of two occasions of 0s on play (and only one with >0 where they played), and a maximum of two missing values for aggressive affect (where they didn’t respond). These are the final sample sizes we report (1,092 and 1,488). We include all these persons (rows) in the analysis, and relied on full information maximum likelihood to account for missingness in the aggressive affect item.

1. In the discussion (which I quite like, overall), you bring up the issue of generalizability. I think if the matter is discussed in such general terms, people tend to shrug it off – every study in psych suffers from generalizability problems! And you mention selection bias and attrition at another point, but again in very general terms. Here, I believe, a central concern is that only a very minor fraction of the potential population of interest (players of Apex Legends, Outriders) actually decided to participate, and most dropped out. It seems quite plausible to assume that (1) the video game/aggression link varies between people (I think it’s even mentioned in the intro) and (2) this effect heterogeneity correlates with features that determine whether people participate in such surveys and whether they stick with them. One might think that generally more well-adapted people are not really negatively affected by the games they play (or even use them strategically to relax), and those are the people most likely to participate in such studies. Obviously, this is by no means unique to your study – it’s probably worse in all those lab studies – so there’s no reason to get defensive about it, and there might be added value in making the people in this field aware of the issue in some more detail.

Thank you for bringing this up. Indeed, your thinking largely aligns with ours: In Vuorre et al. we discuss exactly such a mechanism that more experienced players might be more likely to a) experience a positive effect (or null), and b) do researchers on gaming a favor and participate in the study. Both would bias a negative effect towards the null. For the revision, we followed your recommendation and discussed selection bias and generalizability in more detail, whilst being transparent that we’ve made similar or identical discussion points in Vuorre et al.

1. p. 7: “For example, changes in the amount of leisure time may lead to more play, but also less frustration and feelings of anger, thereby biasing a true negative effect toward the null.”: Maybe I’m misunderstanding how you use the term negative effect. But, the confounding you describe would result in a spurious negative correlation between time spent playing and feelings of anger. So, the confounding can (1) amplify a negative causal effect, (2) induce a negative association in the absence of a causal effect, (3) bias a true positive effect toward the null. I don’t see how what you describe could bias a true negative effect toward the null.

Thank you for pointing this out. Indeed, the situation we described would mean biasing a positive effect toward the null. We corrected the sentence.

1. p. 7, “Is it unethical for our team to publish on the same data twice? We believe it isn’t. First, the research question we answered here is conceptually distinct from Vuorre et al. (2021). Second, the data set has been public for many months and we explicitly invited other researchers to use it.”: I don’t disagree with the points here, but found it a bit peculiar that you’d bring them up – plenty of people re-use the same data over and over again, without anyone accusing them of salami-slicing. So, I looked at Vuorre et al. (2021), and the concern seems a bit narrower – the measure of anger used in this study is an item of a scale included in the other one, is that correct? That might be more likely to induce accusations of salami-slicing – at least there’s a style of paper in which one would analyze every single item as well as the scale, though if we are being honest, that is not how most people analyze and report their data, and it also leads to some issues in its own right (such papers may not be perceived as part of a certain narrow literature and thus ignored, plus frankly the introductions and discussions can be a mess to write). I think a transparent (and less defensive) way to deal with the redundancy between papers is to simply note that you are focusing on this specific narrow research question in this study, and that there’s Vuorre et al. (2021) which investigates effects on well-being more broadly. This might fit well on page 4, when you say “We used this item as our central outcome variable” (which made me think that you’d also look at the other items in this study, given that “central” outcome implies the existence of non-central outcomes). Then, in the discussion, you can simply conclude with an invitation to other researchers to work with your data.

Indeed, aggressive affect is an item of a scale, and Vuorre et al. analyzed the entire scale. That’s the reason we wanted to be “aggressively transparent” and preemptively address potential salami slicing (mostly because we’re rather critical of publishing in the style that you describe: one paper per item). In the process, this section has probably become too defensive—thank you for the feedback and for providing a solution on how to deal with this issue. We followed your advise and clarified the nature of aggressive affect and its relation to Vuorre et al. in the method section. We then deleted the paragraph on salami slicing and instead extend the invitation you suggested.

**Editor First Decision**: Revise & Resubmit

Apr 8, 2022

Dear Niklas Johannes,

I have now read your revised manuscript. I appreciate your careful attention to the concerns the reviewers and I raised. I am happy to provisionally accept your manuscript for submission. However, I found a few small things I would like you to address.

Minor revisions:

1. In your letter, you included a visual of the study methodology and mentioned you would be happy to include it in the manuscript. I’d like for you to make that addition, as I think the figure will be helfpul to readers.

I look forward to receiving your final revision and accepting it for publication in Collabra: Psychology.

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

**Author Response**
Apr 11, 2022

**Editor Final Decision:** Accept

Apr 11, 2022

Dear Niklas Johannes,

I have now had a chance to read over your manuscript "Time spent playing two online shooters has no measurable effect on aggressive affect", 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