**Peer Review and Communication History**

**MS Title**: The Big Five Across Socioeconomic Status: Measurement Invariance, Relationships, and Age Trends

**Author Names**: Bradley T. Hughes\*, Cory K. Costello, Joshua Pearman, Pooya Razavi,

Cianna Bedford-Petersen, Rita M. Ludwig, and Sanjay Srivastava

**Submitted**: Jan 4, 2021

**Editor First Decision­­—Revise & Resubmit**

Mar 14, 2021

Dear Bradley T Hughes,

I sent the Stage 2 paper to both reviewers and their comments are appended below. Both have done an excellent job throughout this process and served as ideal reviewers in the RR process. I thank them both for their service to this outlet.

Reviewer #1 essentially endorsed publication of this version whereas Reviewer #2 had some concerns about how previous studies were interpreted and offered some potential alternative explanations. I read the paper as well and had a few issues and observations for your consideration. In light of these reactions, I will conditionally accept this paper pending a last revision to consider the final issues raised in this letter and in the reviews. Please either modify the manuscript or provide counterarguments in the letter. I will make the final determination based on the revised manuscript and letter and I predict the time to a final decision is short.

First, I want to compliment the paper in terms of the approach and clarity. I think this is a nice model for moving beyond dichotomous judgments of invariance to a more practical and perhaps even more useful approach. The paper also avoids the “sky is falling” tropes that can sometimes characterize failures to support tentative conclusions about invariance. I appreciate your responsiveness and thoughtfulness throughout this process.

1. The comments of reviewer #1 made me think that it might be useful to consider the cross-sectional SES and personality studies as separate from longitudinal ones when considering the existing literature. I noted two studies that might be worth citing and considering (Ayoub et al., 2018 - Study 2, and Goldberg et al., 1998). I think your effect sizes are in the “ballpark” of those papers.
2. One issue that might affect effect size estimation is the impact of coarseness on estimates (see e.g., Aguinis, Piece, & Culpepper, 2009). Some of the SES variables here were coarsely categorized and that is understandable; however, it might create attenuation. It might simply be worth acknowledging the implications of the measurement approach.
3. Reviewer #2 had qualms about how certain studies were interpreted so please consider those concerns in your revision.
4. It be worth considering if there is anything special about the AIID sample that might generate constraints on generality (following Simons et al., 2017). Including a formal COG statement is a possibility.
5. The BFI has since been revised to the BFI-2. It might be useful to comment on whether any of the potentially questionable items persist to the new version.
6. General observation. I am sure you know there are ongoing debates over the virtues of short Big Five measures. One lesson that I took from this papers is that it is advantageous to have multiple items in case a few turn out to be problematic. It is useful to be able to test what happens when misbehaving items are dropped from scale composites for analyses. The more items one has, the more latitude one has. Thus, this is probably another reason to be worried about short forms especially when tackling invariance issues.

A few more minor comments/nit-picky issues…

1. There were a few places where Likert were used in ways that might be questionable. I think this is especially true of the note on Table 2. I think the best descriptor is “ordered categorical variable” or even just 1-5 scale. (I know the hair splitting over when something is truly a Likert scale is tedious. I wonder if it is easier to often delete Likert from papers and use response scale. RIP Rensis Likert, to be sure; however, I think avoiding the issue is a safe pathway in 2021.
2. I think I missed this at Stage 1 but I think there is something cloudy in the discussion about what is justified when only metric invariance is established versus strict invariance on page 6. I think an additional sentence telling readers what is so special about the invariance of residual variances (and when it matters) might prove helpful.

Aguinis, H., Pierce, C. A., & Culpepper, S. A. (2009). Scale coarseness as a methodological artifact: Correcting correlation coefficients attenuated from using coarse scales. Organizational Research Methods, 12(4), 623-652.

Ayoub, M., Gosling, S. D., Potter, J., Shanahan, M., & Roberts, B. W. (2018). The relations between parental socioeconomic status, personality, and life outcomes. Social Psychological and Personality Science, 9(3), 338-352.

Goldberg, L. R., Sweeney, D., Merenda, P. F., & Hughes Jr, J. E. (1998). Demographic variables and personality: The effects of gender, age, education, and ethnic/racial status on self-descriptions of personality attributes. Personality and Individual differences, 24(3), 393-403.

Simons, D. J., Shoda, Y., & Lindsay, D. S. (2017). Constraints on generality (COG): A proposed addition to all empirical papers. Perspectives on Psychological Science, 12(6), 1123-1128.

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,

Brent Donnellan

**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.

Authors were very thoughtful/responsive to previous conceptual and analytic concerns. My only additional thought is to consider some brief follow-up analyses in cases where decisions were arbitrary. For example, if subgroups were created arbitrarily in the MIMIC model, then authors might demonstrate that a different method of subgrouping yields similar results. Or if the income/prestige composite is arbitrary, then show that analyzing one of these elements on its own yields simlar results (with any reduction in reliability notwithstanding). These are just two examples, but authors might review the manuscript for others. The point is to make the findings as strong as possible by ruling out any skeptical arguments about cherry-picking whenever arbitrary decisions were allowed. Good luck in moving forward - I look forward to learning about what is found!

**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.

Regarding my previous comments and suggestions:

1. I was pleased to see that the authors added more details on splitting the data into SES groups
2. I was pleased to see that the authors removed references to replication and I think the paper framing is more in line now with the kind of data they have available. I also appreciated the switch to sensitivity analyses

The paper mostly did what it set out to do plus some additional analyses, and the authors kept track of which analyses were in the registered report and which were not, so that’s good. Overall, I thought the paper was interesting and a good potential addition to the literature, but there are still a few areas that could use some improvement:

1. The literature review contains some inaccurate statements about previous work. For example, several claims about the Damian et al., 2015 are inaccurate. –on p. 4 the authors wrote: "Similarly, an individual’s personality measured in high school can better predict future socioeconomic success than their parent’s SES (Damian et al., 2015)." I am pretty sure that paper never made such a claim. A quick look at the tables in that paper shows much larger effects of parental SES than personality.

–on p. 7 the authors wrote: "Further, conscientiousness has been shown to moderate individual SES over the life course, such that those high in conscientiousness from low-SES backgrounds are more likely to experience socioeconomic mobility later in life than low-SES, low-conscientiousness peers (Damian et al., 2015)." Despite the authors’ claim that they are describing the interaction effect found in the cited paper, the text quoted here does not describe an interaction but a main effect of conscientiousness. A better description of the interaction found in the paper they cite is that higher conscientiousness appeared to “help” people’s future SES *more* when they stemmed from lower parental SES (resource substitution).

There might be other inaccurate statements in the literature review, the above are just two examples.

–the authors reviewed several studies (many of which were longitudinal) and extracted a range of expected effect sizes for the link between personality and SES but then their sample, study design, and analyses did not seem comparable with those of the studies they reviewed. When the authors found smaller effects than those extracted from the cited studies, they concluded that previous studies might have overestimated effects due to analytic flexibility or publication bias. That’s certainly one possibility, but the authors should also highlight the ways in which their study design and analyses might have differed from those of the cited studies because a difference in methodology could also be at the root of the differences in findings (see further comments on this topic below).

1. on p. 10, the language describing correlational patterns in predictions is not always clear, here’s an example: "Education is associated with later normative age of marriage (Parker & Stepler, 2017; Wang, 2018) and family socioeconomic status is associated with greater teen pregnancy (Penman-Aguilar et al., 2013). Thus, applying the theoretical framework and findings of Bleidorn et al. (2013), we can generate a prediction that higher socioeconomic status will be positively associated with the age slope for neuroticism…" Specifically, when you use words such as positively/negatively associated, you don’t also need to say “higher SES”. However, when you write “SES is associated with greater teen pregnancy”, you DO need a qualifier for SES, such as “lower (?) SES”
2. Table 6 is difficult to read and understand. More description is needed in the text. For example, on p. 33, the authors write “to assess the distinctive contribution of each SES indicator we estimated multiple linear regression models, in which each Big Five scale was regressed on education, self-reported income, and occupational prestige in a single model; these are reported in Table 6.” But then, when I look at Table 6, I see indicators of SES categorized as “dependent variables”.
3. Given the present study design and available measures, I thought that the conclusion regarding the findings not supporting social investment theory was too strong. Specifically, the “tests” of social investment theory were indirect and based on few citations and logical arguments for what kinds of social investment timing lower/higher SES might lead to, but we don’t know that’s the case in this sample and no empirical data was provided, unlike in the cited Bleidorn paper where some national-level data was available regarding the age of first job and age of marriage, for instance. I think more limitations need to be added and the conclusions need to be toned down.
4. One of the conclusions of the present paper seems to be that previous studies may have overestimated the link between personality and SES. That’s certainly a possibility, but it’s also possible that differences are due to differences in study design, sample, measurement, and analyses. For example, it’s not clear to me that the studies reviewed (from which expected effect sizes were drawn) had similar designs. In fact, many were longitudinal. Perhaps effects of personality on status attainment cumulate with time and that’s why they appear larger in longitudinal studies. Perhaps measurements had more sensitivity in past studies (e.g., the present study had a more restricted range of educational attainment categories than previous studies and occupational prestige was derived using a new unpublished method, which we cannot properly evaluate without the cited paper). Indeed, the authors write “Directionally, the signs of the correlations were mostly consistent with predictions for self-reported income and education, but not for the two occupation-derived indices (prestige and income)”. Perhaps this has something to do with the novel measurement of occupation-derived indices. Anyway, none of this is meant to reflect badly on the present study, but I think that some of these posibilities should be mentioned in limitations and the conclusions regarding past literature might be toned down a bit given the lack of comparability across studies.

**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**

May 21, 2021

Dear Dr. Brent Donnellan,

Our thanks to you and the reviewers for your insightful comments. We have addressed each

below.

Editor

1. The comments of reviewer #1 made me think that it might be useful to consider the cross sectional SES and personality studies as separate from longitudinal ones when considering

the existing literature. I noted two studies that might be worth citing and considering (Ayoub

et al., 2018 - Study 2, and Goldberg et al., 1998). I think your effect sizes are in the

“ballpark” of those papers.

Thanks for pointing us towards these studies. In the introduction we cited several longitudinal

studies that support a connection between Big Five domains and SES indicators. However, the

estimate of effect sizes (r = .10 - .30) was primarily informed by the cross-sectional zero-order

correlations reported in the studies. Prompted by this comment, we went back and reviewed these articles (Damien et al., 2015; Jonassaint et al., 2009) again. We also looked at the studies you suggested (Ayoub et al., 2018; Goldberg et al., 1998). Goldberg et al. was also consistent with our (and your) characterization of effect sizes; however, we also noted that it found a negative relationship between educational attainment and agreeableness (the opposite of our other literature based predictions in the introduction). We have therefore added a footnote citing Goldberg et al. and addressing this inconsistency in the discussion. Ayoub et al. looked at parental SES, and thus was not a direct comparison to the present study. In light of this, and our later exchange about the registered report process, we decided not to make changes to the introduction. The reviewer’s speculation about longitudinal effects getting larger is counterintuitive and worth future investigation, but it was outside of our ability to address in the present cross-sectional data.

2. One issue that might affect effect size estimation is the impact of coarseness on estimates

(see e.g., Aguinis, Piece, & Culpepper, 2009). Some of the SES variables here were coarsely

categorized and that is understandable; however, it might create attenuation. It might simply be worth acknowledging the implications of the measurement approach.

Thank you for bringing this to our attention. We added a few sentences on p. 44 that address the

impact that coarseness may have had on our results.

3. Reviewer #2 had qualms about how certain studies were interpreted so please consider

those concerns in your revision.

We appreciate that Reviewer 2 identified instances where the findings from previous work were not accurately reported. We were able to address these factual errors in the introduction through edits to a sentence on p. 4 (begins with, “Similarly, an individual’s personality measured in high school…”) and one on p. 7 (begins with, “Further, conscientiousness has been shown to moderate individual...”). We think these changes are in line with the RR policy that only factual errors can be changed in the intro after stage 1 acceptance. And, importantly as discussed in our email exchange, these edits did not impact the rationale for the study or the hypotheses tested.

4. It be worth considering if there is anything special about the AIID sample that might

generate constraints on generality (following Simons et al., 2017). Including a formal COG

statement is a possibility.

We updated the limitations section of the discussion to directly address the constraints on generality (p.48) and added a sentence about how the unique demographics of the AIID sample may limit generalizability.

5. The BFI has since been revised to the BFI-2. It might be useful to comment on whether any

of the potentially questionable items persist to the new version.

Thanks for this suggestion. We have added a paragraph (p. 42) that addresses this.

6. General observation. I am sure you know there are ongoing debates over the virtues of short

Big Five measures. One lesson that I took from this papers is that it is advantageous to have

multiple items in case a few turn out to be problematic. It is useful to be able to test what

happens when misbehaving items are dropped from scale composites for analyses. The more

items one has, the more latitude one has. Thus, this is probably another reason to be

worried about short forms especially when tackling invariance issues.

We appreciate this insight and added a third recommendation in the Practical Impacts sections (pg. 43) suggesting researchers use measures with more items.

A few more minor comments/nit-picky issues…

1. There were a few places where Likert were used in ways that might be questionable. I think

this is especially true of the note on Table 2. I think the best descriptor is “ordered

categorical variable” or even just 1-5 scale. (I know the hair splitting over when something

is truly a Likert scale is tedious. I wonder if it is easier to often delete Likert from papers

and use response scale. RIP Rensis Likert, to be sure; however, I think avoiding the issue is

a safe pathway in 2021.

Thank you for pointing this out. For clarity, we have removed all references to Likert.

2. I think I missed this at Stage 1 but I think there is something cloudy in the discussion about

what is justified when only metric invariance is established versus strict invariance on page

6. I think an additional sentence telling readers what is so special about the invariance of

residual variances (and when it matters) might prove helpful.

We appreciate the comment. Measurement invariance is a complicated topic and not all readers may be familiar with these distinctions, even with the explanation and example we provided. Following guidance to only make changes to the introduction that correct factual errors, we decided to leave page 6 as-is. But to reinforce the point in the introduction, we added a sentence to the discussion (p. 43) reminding readers that when they analyze correlations with observed scale scores, they will need to test for full (strict) invariance.

Reviewer #1

1. Authors were very thoughtful/responsive to previous conceptual and analytic concerns. My

only additional thought is to consider some brief follow-up analyses in cases where

decisions were arbitrary. For example, if subgroups were created arbitrarily in the MIMIC

model, then authors might demonstrate that a different method of subgrouping yields similar

results. Or if the income/prestige composite is arbitrary, then show that analyzing one of

these elements on its own yields simlar results (with any reduction in reliability

notwithstanding). These are just two examples, but authors might review the manuscript for

others. The point is to make the findings as strong as possible by ruling out any skeptical

arguments about cherry-picking whenever arbitrary decisions were allowed. Good luck in

moving forward - I look forward to learning about what is found!

We appreciate the reviewer’s positive comments. A multiverse analysis of the kind the reviewer is suggesting, iterating a large number of analyses over many different ways of creating subgroups, would be a considerable undertaking. Moreover, it is not clear whether it would provide us with clear guidance for whether or how to revise our interpretation of the analyses that were reviewed and accepted at Stage 1, since such an analysis would be post hoc. We believe that our decisions are defensible (and thanks to the registered report process, protected from analyst bias), and we note that the data and code will be available for interested researchers.

Reviewer #2

1. The literature review contains some inaccurate statements about previous work. For

example, several claims about the Damian et al., 2015 are inaccurate. –on p. 4 the

authors wrote: "Similarly, an individual’s personality measured in high school can

better predict future socioeconomic success than their parent’s SES (Damian et al.,

2015)." I am pretty sure that paper never made such a claim. A quick look at the

tables in that paper shows much larger effects of parental SES than personality.

–on p. 7 the authors wrote: "Further, conscientiousness has been shown to moderate

individual SES over the life course, such that those high in conscientiousness from

low-SES backgrounds are more likely to experience socioeconomic mobility later in

life than low-SES, low-conscientiousness peers (Damian et al., 2015)." Despite the

authors’ claim that they are describing the interaction effect found in the cited paper,

the text quoted here does not describe an interaction but a main effect of

conscientiousness. A better description of the interaction found in the paper they cite

is that higher conscientiousness appeared to “help” people’s future SES more when

they stemmed from lower parental SES (resource substitution).

There might be other inaccurate statements in the literature review, the above are

just two examples–the authors reviewed several studies (many of which were longitudinal) and extracted a range of expected effect sizes for the link between personality and SES but then their sample, study design, and analyses did not seem comparable with those of the studies they reviewed. When the authors found smaller effects than those extracted from the cited studies, they concluded that previous studies might have overestimated effects due to analytic flexibility or publication bias. That’s certainly one possibility, but the authors should also highlight the ways in which their study design and analyses might have differed from those of the cited studies because a

difference in methodology could also be at the root of the differences in findings (see

further comments on this topic below).

Please see response to editor comment #1.

2. on p. 10, the language describing correlational patterns in predictions is not always

clear, here’s an example: "Education is associated with later normative age of

marriage (Parker & Stepler, 2017; Wang, 2018) and family socioeconomic status is

associated with greater teen pregnancy (Penman-Aguilar et al., 2013). Thus,

applying the theoretical framework and findings of Bleidorn et al. (2013), we can

generate a prediction that higher socioeconomic status will be positively associated

with the age slope for neuroticism…" Specifically, when you use words such as

positively/negatively associated, you don’t also need to say “higher SES”. However,

when you write “SES is associated with greater teen pregnancy”, you DO need a

qualifier for SES, such as “lower (?) SES”

We edited these sentences about the correlations for clarity and accuracy. These changes did not impact the rationale for the study or the hypotheses tested.

3. Table 6 is difficult to read and understand. More description is needed in the text.

For example, on p. 33, the authors write “to assess the distinctive contribution of

each SES indicator we estimated multiple linear regression models, in which each

Big Five scale was regressed on education, self-reported income, and occupational

prestige in a single model; these are reported in Table 6.” But then, when I look at

Table 6, I see indicators of SES categorized as “dependent variables”.

Thanks to the reviewer for pointing this out. In table 6, the SES indicators are predictors in the regression model not dependent variables. The table has been modified to show this.

4. Given the present study design and available measures, I thought that the conclusion

regarding the findings not supporting social investment theory was too strong.

Specifically, the “tests” of social investment theory were indirect and based on few

citations and logical arguments for what kinds of social investment timing

lower/higher SES might lead to, but we don’t know that’s the case in this sample and

no empirical data was provided, unlike in the cited Bleidorn paper where some

national-level data was available regarding the age of first job and age of marriage,

for instance. I think more limitations need to be added and the conclusions need to be

toned down.

We appreciate the reviewer’s concern that this is not a strong refutation of social investment theory. We are somewhat confused by the reviewer’s statement that “we don’t know that’s the case in this sample and no empirical data was provided, unlike in the cited Bleidorn paper…” The Bleidorn et al. (paper did not have data on role timing in its sample either).

Nevertheless, we are in agreement with the reviewer that this is not a clear falsification of

SIT. We say so in the discussion, and suggest that the measurement and analysis – not

necessarily the underlying theory – may be responsible for the null results: “These results

are far from a definitive falsification of social investment theory writ large, which should be evaluated in light of an overall body of findings rather than a single study. However, they do raise questions about whether there are robust role-timing effects on personality that can be measured and analyzed this way.”

5. One of the conclusions of the present paper seems to be that previous studies may

have overestimated the link between personality and SES. That’s certainly a

possibility, but it’s also possible that differences are due to differences in study

design, sample, measurement, and analyses. For example, it’s not clear to me that

the studies reviewed (from which expected effect sizes were drawn) had similar

designs. In fact, many were longitudinal. Perhaps effects of personality on status

attainment cumulate with time and that’s why they appear larger in longitudinal

studies. Perhaps measurements had more sensitivity in past studies (e.g., the present

study had a more restricted range of educational attainment categories than previous

studies and occupational prestige was derived using a new unpublished method,

which we cannot properly evaluate without the cited paper). Indeed, the authors

write “Directionally, the signs of the correlations were mostly consistent with

predictions for self-reported income and education, but not for the two occupation derived indices (prestige and income)”. Perhaps this has something to do with the

novel measurement of occupation-derived indices. Anyway, none of this is meant to

reflect badly on the present study, but I think that some of these possibilities should be

mentioned in limitations and the conclusions regarding past literature might be toned

down a bit given the lack of comparability across studies.

We appreciate the reviewer’s comment. See our response to editor’s point #1 where we

address prior cross-sectional and longitudinal studies, and editor’s point #2 where we add

further caveats to the discussion. In addition, the reviewer’s comment prompted us to look again at the results referred to in the quoted passage. We noted that the only place the correlations differ in sign for the occupational measures is with agreeableness. We have therefore revised the discussion to reflect this.

We are happy to answer any questions you have about our responses.

Sincerely,

Bradley T. Hughes

Department of Psychology

1227 University of Oregon

Eugene, OR 97403

bhughes7@uoregon.edu

Cory K. Costello

ccostell@uoregon.edu

Josh Pearman

jpearman@uoregon.edu

Pooya Razavi

pooyar@uoregon.edu

Cianna Bedford-Peterson

cbedford@uoregon.edu

Rita Ludwig

rludwig@uoregon.edu

Sanjay Srivastava

sanjay@uoregon.edu

**Editor Final Decision­—Accept**

May 25, 2021

Dear Bradley T Hughes,

I have now had a chance to read over your manuscript “The Big Five Across Socioeconomic Status: Measurement Invariance, Relationships, and Age Trends”, along with the letter describing the changes you made. Thank you for your patience and responsiveness to the remaining concerns. 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. Our managing editor will contact you in case there are any pre-prodution file related questions. You will have an opportunity to check the page proofs before we publish your article. Thank you again for publishing in Collabra: Psychology.

Sincerely, Brent Donnellan