

Peer Review Comments

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Responses for Version 1

Reviewer 1: Katherine Corker

Affiliation: Kenyone College, USA

Competing Interests Statement: None

Review Completed: 25 May 2015

Recommendation: Revisions Required

1) General comments and summary of recommendation:

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

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

This paper proposes that researchers publish one replication study for every four original studies they publish, resulting in approximately 20% of journal space being dedicated to replications. Dr. LeBel extensively reviews recent history in the reproducibility movement in psychology, and then he uses an existing norm of three journal reviews for each paper submitted/published as the basis for the proposal of the new replication norm.

This is a difficult paper to evaluate. It presents no new data or analyses and instead simply argues for this idea of a new replication norm. On the one hand, the norm that is proposed is novel. On the other hand, there does not seem to be much else that is new here. As you note, simply asserting a new norm will not make it so; some might argue that the 3:1 (reviews: published papers) norm is not well known either. I will add a few of the main thoughts I had while reading the paper.

1. You note that "systematic" replications (aka constructive replications, aka replicate-and-extend) are needed to both evaluate reproducibility and facilitate theoretical progress. I agree, and I wonder if there is a compelling argument for why the majority of research cannot be of this sort. That is, does the proposed replication norm perpetuate a false dichotomy between "replication" and "original" research? Of course, we don't need to replicate back to the very first study in a line, but what would be the harm in having every study replicate back k-1 studies? That is, Author A proposes novel hypothesis X, tests it in Study 1a, directly replicates in Study 1b, and publishes it. Author B seeks to extend the test by demonstrating generalizability to a new operationalization of the DV. She does the test constructively, as depicted in Table 1. The new operationalization more clearly shows the effect compared to the old operationalization, and so in Study 2, she both replicates the new DV operationalization and extends to a new IV, as depicted in Table 2. As you can see, direct replications are embedded throughout the process, both within and across labs.

2. If you agree that systematic replications are a critical path forward, then some possibilities for expanding on your contribution become apparent. Above, I asserted that the current paper does not provide much of a novel contribution, but to me the major way to expand the contribution would be to focus on the practical implications of recommending that many/most tests be replications and extensions. For instance, what are the recommended analytic techniques for this type of design? The addition of new DVs (Table 1) could lend itself well to a MANOVA based approach (for experimental tests) or SEM (for correlational studies). Researchers should pay attention to the correlation between DVs (if they are different operationalizations of the same construct, they should correlate positively). The addition of new IVs is tricky – would the planned analysis be a 2 x 2, or would you treat the conceptual replication as a new data point in a mini meta-analysis (together with the original study and the direct replication)? I do think that what is sorely needed currently are practical recommendations about how to integrate replication into our workflows more generally.

3. There is some evidence in the norms literature that norms can not only motivate socially desirable behaviors among in-compliant individuals, but they can also motivate socially undesirable behaviors among compliant individuals. In Schultz et al.'s (2007) study, they found that high energy users decreased their energy consumption after negative feedback (desired outcome), but also that low energy users increased their energy consumption after positive feedback (undesired side effect). I wonder if the replication norm you propose would have similar consequences. That is, some researchers might do more replications than they previously did, but some researchers (frequent replicators) might actually withdraw effort from their replication studies. It's hard to say what effect this might have on the overall quantity of replications, but it may be something to consider.

4. I'm not sure I buy the claim that this 4:1 ratio would incentivize researchers to publish fewer papers per year, but I do agree that a replicate and extend mentality is consistent with incentives for pre-registration. The push for increased sample sizes will surely slow the pace of research, but neither your 4:1 ratio, nor higher Ns, will likely motivate researchers to publish less. Only changes to the reward structure (e.g., rewarding quality and reproducibility over quantity) will have this incentivizing effect.

Additional Points

5. Page 4 – The Social Psychology special issue reported 11 out of 27 studies replicated (40%). The numbers you report are inconsistent.

6. Page 4 – I'm not sure what you mean by "quasi-randomly selected." I would describe the Reproducibility Project as an attempted census of 2008. Random selection would require a bounded population, which would be hard to define.

7. There are typos throughout the paper (e.g., p. 6 – "social normal;" p. 13 – "the quality of the researcher").

References:

Schultz, P. W., Nolan, J. M., Cialdini, R. B., Goldstein, N. J., & Griskevicius, V. (2007). The constructive, destructive, and reconstructive power of social norms. *Psychological Science*, 18, 429-434.

2) Figures/tables/data availability:

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

NA

3) Ethical approval:

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

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

NA

4) Language:

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

ment beyond the scope of this process.

Yes, the writing is fine.

Reviewer 2: This reviewer has chosen to remain anonymous

Affiliation:

Competing Interests Statement: None

Review Completed: 4 August 2015

Recommendation: Revisions Required

1) General comments and summary of recommendation:

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

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

Review of Replication Norm paper.

This is an interesting and worthwhile manuscript. I would recommend publication with revision.

I liked the summary of contemporary replicability and replication issues and debate. One question the authors might respond to in a revision: If every journal required a direct replication as Step 1 in any manuscript reporting a series of follow-up studies that build on some original finding, would there still be a need for the Replication Norm outlined here?

Another question: why are people even as willing as they are to review papers? Maybe economists have discussed this. Is it because it provides a little gate-keeping power? Or is it the academic's sheer interest in their own topic of interest?

pp. 14-15: Good discussion of how ideas from social psychology may help in suggesting how to promote acceptance of suggested new norms.

I would like to see more discussion of how different participants in the scientific enterprise are, or are not, incentivized to promote this idea. Seems complex to me. Departments probably are NOT incentivized to encourage their faculty to do more replications. On the other hand, they are not incentivized to get their faculty to review papers, either, but that happens. On a more positive note, it would seem to me that funders (NIH, NSF, etc.) ARE pretty well incentivized to want the investigators they fund to spend some time validating the work of other investigators.

2) Figures/tables/data availability:

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

Not relevant.

3) Ethical approval:

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

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

Not relevant

4) Language:

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

Reasonably well written.

Editor Decision for Version 1

Editor: Simine Vazire

Affiliation: University of California, Davis, USA

Editor decision: Revisions required

Decision date: 31 July 2015

Dear Dr Etienne Philippe LeBel,

Dear Etienne,

I want to first apologize for the delay in rendering this decision. I have no good excuse except the usual culprits. I apologize for any inconvenience this may have caused you.

I have now received two reviews of your manuscript from researchers with expertise in research methods and replication. I also independently read the manuscript before consulting these reviews. The reviewers had mostly positive reactions to your manuscript. 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.

Both reviewers provided clear reviews and you should address each of their concerns either in the text or in the response letter. I agree with Reviewer 1 that the major limitation of this article is the narrowness of its contribution. I think you could expand and further develop the central thesis of your manuscript. Thus focus of my recommendations below is on ways in which you could further elaborate on the central idea in your paper.

1. Reviewer 2 makes an excellent suggestion: delineate the ways in which each of the major constituencies in the research process can contribute to incentivizing replications, and address the obstacles that each of these constituencies faces. Specifically, you could have a section on each of the following categories, and explain what challenges the new replication norm would pose for each of them, and how we could incentivize them to contribute to or support this norm: Funding agencies, journals, professional societies, psychology departments (and their universities, hiring committees, tenure & promotion committees), established researchers, early career researchers. I think there is a lot more than can be said about the role of each of these groups, and very concrete recommendations you can make about what each group can and should do, and also what the current incentive structure means they are likely to do.

2. You compare the replication norm to the peer-reviewing norm, which I think is very useful. The parallels (and non-parallels), could be explored further. For example, reviewer 2 asks why people generally do a fair amount of peer-review. Can this tell us anything about how we can incentivize/normalize replications? Another important question is, should doing replications be seen as service or as research? If it is service, does this undermines your point about it being a basic part of the scientific process? If it is research, does the comparison to peer-reviewing fall apart?

3. I agree with Reviewer 1 that you exaggerate the extent to which there is a clear distinction between replications and original research, and I would add that the distinction between direct and conceptual replications is also less clear than you present it as being. This is in some ways beyond the scope of your paper, but I think acknowledging that there is a continuum from direct replications to conceptual replications to original research is important, and it could also help bolster some of your recommendations. I agree with you that replications, for the purposes you describe here, should be as direct as possible, and it is important to draw a line between pretty-direct replications and clearly conceptual replications. MORE HERE.

4. I also agree with Reviewer 1 that the more you can give specific practical guidelines, the better. The example you give, including a systematic replication and an extension in the same study, is very useful. Can you give other models for how to incorporate replications into one's research at relatively low cost? How do other labs who regularly conduct replications do it? You could also describe coordinated efforts like the Registered Replication Reports, which make it quite easy for a lab to jump

into an already existing replication effort. What about adversarial collaborations? Or Many Labs? The more concrete examples and practical tips you can give, the better. Another area where practical guidance would be useful is in how to select which studies to replicate.

5. In some places, you focus mainly on how failed replications can be useful (e.g., the paragraph that begins at the bottom of p.11) – it would be good to demonstrate how both failed and successful (and even inconclusive) replications can be useful (which you do in other places).

6. In several places you mention that one benefit of the replication norm would be that researchers would conduct fewer studies. However, fewer studies is not a better situation for its own sake, but only because there is necessarily a tradeoff between quantity and quality of studies. You make this pretty clear, but I think it could be more explicit – right now it sounds like you are saying that fewer studies should be a goal in its own right.

7. Related to point #1 above, I think it is worth thinking more about the role of early career vs. established researchers. On page 14 you describe the possibility of teaching the replication norm to graduate students and early career researchers, but it seems to me that established researchers are the ones who should bear the brunt of the burden at the beginning, for several reasons. They have less to lose, but also, their behavior is likely to have more influence on others' behavior. Indeed, it seems that one way to get the replication norm off the ground would be to get not only professional societies to endorse it (as you say on page 15), but also getting visible, established researchers to publicly commit to devoting a certain proportion of their research time to replication studies.

Minor points:

-You could mention the work by Westfall and colleagues on treating stimuli as random factors when discussing the idea of adding new vignettes/stimuli in systematic replication & extension studies.

-A shameless plug: SPPS now also has reporting standards about excluded measures, how sample size was determined etc., and it also now invites replication papers, so if you wanted more examples of journals that are making these changes, you could add it (pp. 4 and 5).

-p. 6: "... also may reduce the tendency for researchers to published unexpected, exploratory, and/or tenuous results" – this is not a bad practice in and of itself, it's only bad if these results are presented as expected/confirmatory/conclusive.

-pp. 7 through 8 – you can cut the equations (" $2/2+8$ "; "given that 1 divided by 5 is 20" etc.)

-Table 2 – the column heading for the left column should be something other than "control condition" – this is confusing because it makes it sound like there was more than one level of this variable in the original study.

-The list of direct replication studies that journals refused to publish at the end of the paragraph at the top of page 12 reads a little bit like you have a chip on your shoulder – are there other examples that are not of your own work? I understand that of course it's much easier to know of rejected papers that you were an author on, but if there was a way to make this part less personal that would be good.

The full review information should be included at the bottom of this email. There may also be a copy of the manuscript file with reviewer comments available once you have accessed the submission account. Please consider these points along with the points raised above and revise the file accordingly:

To access your submission account, follow the below instructions:

- 1) login to the journal webpage with username and password
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- 3) click 'Review' menu option
- 4) download Reviewed file and make revisions based on review feedback
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ments to the journal.

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

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

Please could you have the revisions submitted by [GIVE 2 WEEK DEADLINE]. If you cannot make this deadline, please let us know as early as possible.

Kind regards,

Simine Vazire

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Author's Response to Review Comments for Version 1

Author: Etienne Philippe LeBel

Affiliation: Western University, Canada

Revision submitted: 26 August 2015

Please find attached a cover letter summarizing all of the changes that I have made in response to the editorial feedback (following the order in your action letter).

Dear Dr. Vazire:

Thank you very much for your feedback on my manuscript A New Replication Norm for Psychology. I was glad to hear that you believe a revised version of my manuscript may be suitable for publication at Collabra. I took your editorial feedback seriously and have attempted to address all issues raised by you and the two reviewers in my revised manuscript.

Here is a summary of the changes that I have made in response to the feedback (following the order in your action letter).

Action editor issues:

(1) To address the main issue of narrowness of the contribution, I have significantly expanded the "Promoting the Norm" section of the manuscript by delineating how each major constituency of the research process (i.e., funders, journals, professional societies, departments, and individual researchers) can incentivize replications and promote the new norm and any obstacles each constituency faces in supporting the new norm (pp. 17-21). As per your suggestion, for the subsection on

individual researchers, I separately considered early vs. established researchers (p. 21). I have also updated the abstract by adding a sentence to this regard (p. 2).

I have also broadened the scope of the contribution in several other ways in response to other issues raised by you and the reviewers. I summarize these below in the order they appear in your action letter.

(2) Regarding parallels vs. non-parallels between extant peer review norm and new replication norm, I have now added two paragraphs after the first paragraph introducing the new replication norm idea within the "New Replication Norm" section (pp. 8-9).

(3) To address the issue that I exaggerate the difference between original and replication research, I have now added a footnote and new reference (FN3, p. 8) that states:

It is important to mention that the difference between original and replication research should not be exaggerated given that in actuality a continuum exists between original and replication research (Aarts, 2015).

Regarding the concern that I also exaggerate the difference between direct and conceptual replications, I have added the following sentences (p. 11):

A direct replication aims to duplicate as closely as possible the conditions and procedures that existing theory and evidence anticipate as necessary for obtaining the effect (Nosek & Lakens, 2014; Open Science Collaboration, 2012). A conceptual replication, on the other hand, intentionally alters some (or several) aspects of the methodology to test whether a finding generalizes to different experimental manipulations, measures, or contexts (Asendorpf et al., 2013; Schmidt, 2009). Though in practice the difference between direct and conceptual replications lie on a continuum, it is crucial that independent replications duplicate the methodology of the original study as closely as possible because if different methodology is used and discrepant results emerge, then it is ambiguous whether the discrepant results are due to the different methodology or because the original finding was false (LeBel & Peters, 2011; Pashler & Harris, 2012).

(4) With regard to the suggestion of providing more specific and/or practical guidelines for researchers to incorporate replications into their workflow, I have added a new paragraph that mentions two additional strategies for incorporating replications at relatively low costs (i.e., multisite Registered Replication reports @ Perspectives on Psychological Science and collaborative replication efforts organized by the Center for Open Science, p. 14).

To address the suggestion that it would be useful to offer practical guidance on how to select which studies to replicate, I have added a new subsection (entitled: "What studies should be replicated?") within the "New Replication Norm" section which mentions that studies that have had substantial theoretical impact and/or that have important applied implications are good candidates. The new subsection reads (p. 10-11):

In general, researchers should seek to replicate studies that have had substantial theoretical impact within one's own research area and/or studies that have important applied implications for society and/or public policy. High-theoretical impact studies include classic or seminal studies that have spurred on voluminous amounts of follow-up research (e.g., Bargh et al.'s, 1996 automaticity of social behavior studies) or studies that have been highly-cited relative to citation metrics for a particular field. Other studies that are good candidates for replication include findings that potentially have important applied implications (e.g., Birtel & Crisp's, 2012 imagined-contact effects for treating prejudice, which subsequent replication efforts were unable to corroborate: McDonald, Donnellan, Lang, & Nikolajuk, 2014; see also Klein et al., 2014).

(5) To address the suggestion that it would be good to mention how both failed and successful replications can be useful, I improved the following sentence within the "Direct and Indirect Benefits of New Replication Norm" section (p. 15). This sentence now reads:

This is to be contrasted with the present reality where the results of successful and unsuccessful independent replications are simply publicly unknown or are privately known in an unsystematic matter (e.g., water cooler discussions at conferences).

(6) Concerning the issue about how the new replication norm may lead researchers to conduct fewer studies, I've added a new sentence in the Benefits section that explicitly mentions the trade-off between quantity and quality of studies (p. 16). The sentence is:

This is the case because of the inherent trade-off between quantity and quality of studies whereby running and publishing a higher number of studies will typically lead to lower quality studies.

(7) I agree it is important to distinguish between early vs. established researchers when discussing how individual researchers can promote the new replication norm. In the new sub-section entitled "Individual researchers." (p. 21), I discuss the relevant issues separately for early vs. established researchers.

(8) I've now mentioned the work by Judd, Westfall, and Kenny (2012) when discussing the systematic replication example which seeks to generalize the finding to new stimuli (p. 12). The sentence reads:

Completely independent participants would be randomly assigned to either the cleanliness-related versus neutral priming condition and then judge a series of new moral actions (e.g., more ecologically-valid moral actions), to see if the original finding generalizes to these arguably more valid stimuli (in which case a mixed-effects approach advanced by Judd, Westfall, & Kenny, 2012 could be used whereby stimuli are considered as random factors.)

(9) I now mention *Social Psychological & Personality Science* as another example journal with higher reporting standards (p. 4) and as another example journal that now publishes replications (p. 5).

(10) I have now revised the sentence about journals publishing replications potentially reducing the tendency for researchers to report exploratory findings as confirmatory (p. 6). The sentence reads:

The new development of journals publishing replications may also reduce the tendency for researchers to report unexpected, exploratory, and/or tenuous results as confirmatory or conclusive findings (Schimmack, 2012).

(11) As suggested, I have now removed the simple equations (" $2/2+8$ ", originally on p. 7) and (given that 1 divided by 5 is 20%, originally on p. 9).

(12) In Table 2, I have now improved the column heading of the left column as to avoid confusion (p. 13). The column heading now reads: "Low cognitive load (control)"

(13) Regarding examples of journals that refused to publish replications of their own studies, I have now swapped one of my own examples with one by another researcher (Dijkstra et al., in press, see p. 15).

Reviewer 1 issues:

(14) As previously mentioned, I've addressed the issue that I exaggerate the difference between original and replication research by adding a footnote and new reference (FN3, p. 8, see above).

Regarding Reviewer 1's suggestion that it may be useful to have every study replicate back k-1 studies, I have added a footnote stating that though this would dramatically increase the reliability of findings it may not be the wisest use of resources (FN8, p. 14):

A more radical strategy could involve always executing systematic replications for studies testing novel hypotheses that extend a published effect (i.e., always replicate back k-1 studies where k = current study). Though this approach in theory would dramatically increase the reliability of findings, it would arguably be an inefficient use of resources in terms of maximizing scientific discovery given that researchers have finite resources (McElreath & Smaldino, 2015).

(15) Concerning expanding the contribution by providing more concrete/practical suggestions, I have added a footnote and a new paragraph that describe concrete recommendations on how researchers can integrate replications into their workflows more generally.

The footnote (FN, pp. 12-13) reads:

An alternative strategy would be to present the new-and-improved moral actions after the original dependent variables (DVs), in which case a MANOVA approach could be used to analyze the resulting data. Note, however, that in cases where individual DV items are time-consuming, this could cause

fatigue effects which may interfere with finding an effect supporting the generalizability of the original finding.

The new paragraph (p. 13) reads:

In this case, data from such systematic replication could be analyzed via a mini meta-analysis, whereby the conceptual replication (e.g., the elderly vs. control contrast under the new high cognitive load situation) is treated as a new meta-analytic data point to be considered in conjunction with the direct replication result and original result. Indeed, there are some user-friendly R packages that can be used to execute such mini meta-analyses (e.g., metafor package, Viechtbauer, 2010; meta package, Schwarzer, 2015).

(16) To address the issue that some evidence in the norms literature exists that norms can sometimes motivate socially undesirable behaviors among already compliant individuals, I've added a footnote stating that this would be unlikely to occur for individual researchers within the context of the new replication norm. The footnote (FN10, p. 22) reads:

There is some evidence that under some conditions norms can also motivate socially undesirable behaviors among already compliant individuals (Schultz, Nolan, Cialdini, Goldstein, & Griskevicius, 2007). This should be unlikely for the new replication norm, however, given that researchers are intrinsically motivated to execute and publish replications because they care about the theoretical progress of their own research area.

(17) Regarding the issue that Reviewer 1 didn't buy the claim that the proposed 4:1 ratio would incentivize researchers to publish fewer papers, I've now added a sentence (on p. 16, as mentioned above) that explicitly mentions the inherent trade-off between quantity and quality of studies.

(18) Concerning the non-replicability rate of the Social Psychology special issue, I've now corrected the values, which are now: "about 70% (16 out of 23)" (p. 4).

(19) To address Reviewer 1's confusion regarding the expression "quasi-randomly selected" in describing the Reproducibility Project, I have now simply removed that expression (p. 4). I didn't follow the suggestion of describing the Reproducibility Project as "an attempted census of 2008" because I wasn't sufficiently convinced this would have clarified the nature of the sampling of studies.

(20) I have corrected the two typos identified by Reviewer 1 ("social norm[al] on p. 6 and "the quality of the research[er] originally on p. 13, now on p. 16).

Reviewer 2 issues:

(21) Regarding Reviewer 2's hypothetical question of whether if every journal required a direct replication as Step 1 in any manuscript reporting a series of follow-up studies would obviate the new replication norm, I have added a footnote stating that this would indeed obviate the need for the replication norm, but that this is not realistically feasible. The footnote reads (FN9, p. 19):

If every journal required a direct replication as Step 1 in any manuscript reporting a series of follow-up studies that build upon an original finding, we would not need the new replication norm. However, this is extremely unlikely to ever occur because such strategy is (1) simply not feasible except in particular areas of experimental psychology and (2) arguably an unwise use of resources given that not all findings need to be replicated (McElreath & Smaldino, 2015).

(22) To address Reviewer 2's issue about understanding why people are even willing to spend time reviewing papers, I have added several sentences elaborating on possible factors including (1) intrinsically caring about the theoretical progress in their own research area and (2) getting credit for doing so (p. 8; these sentences are part of the expanded section on the parallels vs. non-parallels between the extant peer review and new replication norm, as mentioned above in point #2).

(23) Regarding wanting more discussion about how different participants in the scientific enterprise are – or are not – incentivized to promote the new replication norm, I have now significantly expanded the "Promoting the Norm" section as described above in point #1. That is, I now delineate how funders, journals, professional societies, departments, and individual researchers can incentivize replications and promote the new norm and any obstacles each constituency faces in supporting the new norm (pp. 17-21).

I feel that the revised manuscript is a stronger paper that now makes a more important contribution to the literature given its broader scope. I look forward to hearing your reaction to the revised manuscript.

Please do not hesitate to contact me if you have any question.

Best regards,

Etienne P. LeBel

Editor Decision for Version 2

Editor: Simine Vazire

Affiliation: University of California, Davis, USA

Editor decision: Revisions required

Decision date: 10 September 2015

Dear Dr Etienne Philippe LeBel,

I am happy to provisionally accept your revised manuscript, "A New Replication Norm for Psychology", for publication in *Collabra*. I appreciate your responsiveness to the comments raised by me and the reviewers. I have a few small suggestions for you which I list below, but it is up to you which of these suggestions to incorporate in the final draft of your manuscript. I will be happy to accept the manuscript regardless of what you decide.

1. In my opinion, footnotes make the paper harder to read because I want to flip to the end and read them as I am going through the paper. Some of your footnotes probably need to remain footnotes, but I think some could be incorporated into the main text. If you agree, I think moving at least some of them into the main text would improve the readability of the manuscript.
2. In reading your suggestions about how to select which studies to replicate, I had a thought based on a recent experience with a replication submission. Do you think that one factor that should influence researchers' decisions about which studies to replicate should be the strength of the original finding (and any replications or other relevant evidence)? That is, imagine that a researcher could replicate Study A or Study B. Study A was very high-powered, used high-quality methods, and found very strong evidence for the effect (e.g., a large Bayes Factor or a very small p-value, etc.). Study B was underpowered and found weak evidence. Do you think our time would be better spent doing a high-powered replication of Study B (assuming the two studies are equivalent on the other criteria you discuss)? Given that there will be way more original studies than we can possibly replicate, even when we limit it to those that meet the other criteria you mention, it would be useful to name as many factors as possible that you think should guide researchers in selecting which study to replicate. Again, feel free to ignore this suggested criterion (I think in this scenario the replicator should choose to replicate Study B, but I can see counterarguments), just wanted to mention this in case you think it would make a good addition.
3. p. 11 - "It many instances" should be "In many instances"
4. p. 16 - "This is the case because of the inherent trade-off between quantity and quality of studies whereby running and publishing a higher number of studies will typically lead to lower quality studies." This sentence does not quite make sense given the sentence right before it. I think you mean that the reason that publishing fewer papers/studies is a benefit is because there is a tradeoff between quantity and quality, but right now it sounds like you are saying that the reason that the new replication norm would lead people to publish fewer studies is because there's a tradeoff between quantity and quality. Maybe that is what you mean to say, but in that case I don't follow the logic.
5. p. 17 - "This is no better demonstrated by" should be "This is illustrated by..." or something like that (the current wording is grammatically incorrect).
6. footnote 9 - There are also areas of non-experimental psychology (e.g., longitudinal studies) where requiring a direct replication as step 1 would be a huge burden.
7. p. 21 - I am ambivalent about naming specific people as trailblazers, but I'm not an author on the paper, so it's up to you! If you do keep them in, I would encourage you to consider naming individuals who

might be more likely to go unnoticed because of status or demographic variables that make them less likely to get attention. For example, Katie Corker is a researcher at a non-R1 institution who has done important replication work.

8. p. 21 - I am trying to anticipate what a critic might say about what the costs are to a later-career researcher of doing replication studies. One could argue that since more established researchers have more expertise/skill, taking their time away from original research is more costly to the field than it is to take young researchers' time, because young researchers are still learning the ropes. I'm not saying I endorse this view, or that you have to add this. Just a thought that you are free to ignore if you wish!

Thanks again for submitting your work to Collabra, and for your careful revision. I am looking forward to seeing this paper published!

To access your submission account, follow the below instructions:

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

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

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

Please could you have the revisions submitted by [GIVE 2 WEEK DEADLINE]. If you cannot make this deadline, please let us know as early as possible.

Kind regards,

Simine Vazire

University of California, Davis, USA

simine@gmail.com

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Author's Response to Review Comments for Version 2

Author: Etienne Philippe LeBel

Affiliation: Western University, Canada

Revision submitted: 10 September 2015

Thanks for the final feedback. I have moved some of the footnotes to the main text, revised the quantity/quality of studies trade-off issue, and made a few other minor changes to address most of the final concerns you brought up.

Thanks again, and let me know if you need anything else.

Cheers,

Etienne.

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Editor Decision for Version 3

Editor: Simine Vazire

Affiliation: University of California, Davis, USA

Editor decision: Accept Submission

Decision date: 10 September 2015

Dear Dr Etienne Philippe LeBel,

I am happy to accept your submission, "A New Replication Norm for Psychology", for publication in Collabra, pending the completion of copyediting and formatting processes.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point. The accepted submission will now undergo final copyediting. You will be contacted once this is complete to answer any queries that may have arisen during copyediting and to allow a final chance to edit the files prior to typesetting. If you wish to view your submission during this time, you can log in via the journal website.

Kind regards,

Simine Vazire

University of California, Davis, USA

simine@gmail.com

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