

# Peer Review Comments

**Article:** Heyman, T., et al. (2017). Does a Working Memory Load *Really* Influence Semantic Priming? A Self-replication Attempt. *Collabra: Psychology*, 3(1): 18, DOI: <https://doi.org/10.1525/collabra.96>

**Article type:** Original Research Report

**Editor:** Rolf Zwaan

**Article submitted:** 07 June 2017

**Editor decision:** Accept Submission

**Revision submitted:** 28 June 2017

**Article accepted:** 29 June 2017

**Article published:** 07 August 2017

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## Ported Reviews

**XLM-2017-3268**

Does a working memory load really influence semantic priming? The story of a self-replication attempt *Journal of Experimental Psychology: Learning, Memory, and Cognition*

Dear Mr. Heyman,

I received reviews from three anonymous reviewers. While Reviewer 1 is generally supportive of the work, the other two reviewers argue convincingly that this paper does not meet the standard for publication in JEP:LMC. First, as Reviewer 2 notes, the results do not allow any firm conclusion about whether the originally reported results are real or not. In addition, Reviewer 2 makes a quite convincing argument that this type of paper is not appropriate for JEP:LMC. I was persuaded by his/her argument and am rejecting this paper.

For your guidance, I append the reviewers' comments below and hope they will be useful to you as you prepare this work for another outlet.

Thank you for giving us the opportunity to consider your submission.

Sincerely,

Michael Cortese, Ph.D.

Associate Editor

*Journal of Experimental Psychology: Learning, Memory, and Cognition*

Reviewers' comments:

Reviewer #1: This paper is not the typical submission to JEP:LMC, but I found myself appreciating its content for several reasons. Personally, I thought the quotes at the beginning were a bit over the top. However, I appreciated the authors' sincere attempt to understand their initial failure to replicate earlier findings. I thought the approach was

sound and thorough, especially in attempting to conduct the identical experiment in the original language and setting. I also found the Bayesian analyses and their interpretations to be helpful in understanding the results across experiments. I hoped for more compelling hypotheses regarding factors responsible for the different findings, but I thought their discussion was reasonable given the evidence. This is clearly an important and timely issue, and I thought this was a laudable attempt to provide readers with an example of failure to replicate theoretically relevant evidence, and to demonstrate approaches for evaluating and interpreting the evidence across multiple attempts to replicate.

Reviewer #2: Manuscript 2017-3268 reports two experiments that fail to replicate the authors' own finding that was published in this journal two years ago. The first replication attempt used the same task as the original, but in a different language, a different location, and with a smaller sample. The second study is an exact replication of the original, but with a larger sample. I have strongly ambivalent feelings about this manuscript. Below I'll first provide my evaluation of this research, and then I'll provide my more general beliefs about the appropriateness of publishing this type of manuscript.

## RESEARCH EVALUATION

The two studies are competently conducted and the analyses are generally appropriate. The methods and analyses are clearly up to the standards of JEPLMC, as they are identical to those previously published in this journal. In my opinion, however, the results are extremely mixed, and consequently the theoretical contribution of these studies is very unclear. To put it simply, I expected in such a paper for the authors to conclude very clearly whether the effect is real (reliable) or not, but unfortunately the data do not provide any clear conclusion.

The results are highly variable between experiments. Each of the 3 studies has 12 potential priming effects across the 2 (SOA) X 2 (load) X 3 (pair-type) conditions (see Tables 1-3). Of those 12 possible priming effects, only 5 produced consistent effects across all three studies. For example, considering only the BA pairs, none of the four conditions (SOA X load) produced the same effect across all three studies. For the SYM pairs, only two of four conditions produced consistent results. For the FA pairs, three of four conditions were consistent across studies. With these extremely variable results, I am very reluctant to conclude anything from these studies. The only firm conclusion from these new experiments is that the conclusion of the original paper *might* be incorrect.

Such highly variable results often indicate the presence of some "hidden moderator". I would encourage the authors to seek and demonstrate one or more moderators of these results (e.g., language proficiency?). If the authors can predict and explain when the key result does and does not occur, that would be a worthy contribution to the literature. Alternatively, if the authors were to run yet another replication study, they might consider a stronger manipulation of cognitive load, and/or dropping the 1200 SOA condition (which produced even less reliable results than the 200 SOA condition). The high variability might also be due partially to the somewhat small sample of items, which could be increased to hopefully decrease the variability in priming effects across studies.

## APPROPRIATENESS

Please note that what I write below is not about the authors of this particular manuscript. My comments below are aimed more at a sociological level, in terms of our field as a whole, because I think this submission raises a much larger issue than this one particular effect.

First I'd like to acknowledge that it's commendable to replicate one's own work, and more importantly, to correct the public record of scientific knowledge. This is a step in the right direction. However, I am opposed to publishing two papers by the same authors reporting what may or may not be an effect. To state it bluntly, I do not believe that we should continue to reward the publication of unreliable results by subsequently awarding those same authors with additional publications to correct their mistakes. This implicitly encourages, rather than discourages, the publication of irreproducible effects. I also believe it is unjust for erroneous authors to appear virtuous by publicly admitting their errors, while collecting two publications in the process of committing and then correcting those errors.

If the journal cares about truth, then it should accept to publish *any* competently conducted failure to replicate effects published in the journal. While we may all agree that would be scientific progress, we mustn't forget that the replicability of JEPLMC effects is only about 50% (according to the Reproducibility Project). Obviously this means that for every two original articles published in JEPLMC, the journal would become liable to publish one additional article effectively retracting one of those two original articles. So, in my opinion, JEPLMC needs to either (a) start dedicating 33% of its publication space to replication failures, or else (b) find some other solution for correcting its mistakes. Assuming that the editors prefer strategy (b), then what exactly are the options? These are just off the top of my head, and presumably a more thoughtful consideration would yield more and probably better options:

1. Retraction. I agree with the authors that this seems overly harsh. If there's evidence of fabrication or intentional p-hacking, then it should be retracted. But the run-of-the-mill replication failure (like this one) shouldn't be retracted (for several reasons that I won't elaborate here).
2. Commentary. The journal publishes brief commentaries on published articles. This category could be expanded to include self-commentaries, including a short statement about the failure to replicate, with a link to a full replication report on the journal website.
3. Correction. APA journals publish brief "Corrections" in cases of minor but important typos or omitted information and such. These could be expanded to include a short statement about the failure to replicate, with a link to a full replication report on the journal website.
4. Self-Publication. The journal could somehow support authors' self-publication of replication failures via a "Correction" with a link to the authors' replication failure, an official mention on the journal website, or some other means like this. My concern with this is that self-publication is not as permanent as journal publication, so the record could become lost.

Of these four options - and again, you may think of better ones - I prefer the Correction strategy. It's permanent, it's transparent and honest, and it doesn't require JEPLMC to dedicate 33% of its space to essentially retracting its own irreproducible effects.

Finally, I'd like to conclude with a note to the JEPLMC editors: Of course, the best solution is to minimize the publication of irreproducible results. So long as JEPLMC continues to publish single-experiment papers, the journal will continue to promote irreproducible results. Either require from authors more reliable evidence for the results that you publish (e.g., self-replication \*as a pre-condition for publication\*), or else accept that you are publishing irreproducible results at a high rate.

Reviewer #3: The authors report data from two self-replications on semantic priming. In contrast to their recently (2015) published study, forward associates can elicit priming even when participants' working memory was taxed.

#### Evaluation

This is definitively a hard one. Why? Because I do think that the AE or the whole editorial board have to make a general decision here on whether self-replications are worthwhile. To me, although I applaud the authors for their honesty (although my personal default attitude is that all researchers are honest until proven otherwise), the theoretical and methodical gain of this article is definitively not what a JEPLMC article typically offers. So, the experiments and analyses seem fine to me - but the theoretical gain is mediocre at best.

Let me add first some more general points:

- 1) Self-replications are not nearly as convincing than truly independent replications
- 2) Is the JEPLMC the right outlet for self-replications?
- 3) Should instead of publishing this as an article the original article just be withdrawn?
- 4) Is psychology really in a crisis? If so, researchers and journals should be very rational how to handle this (at the moment many are nervous/aroused). It is for sure a good thing to be skeptical, always - also concerning your own research. But what happens if all people start to publish their failed experiments? What happens if some 'bad guys' run say 5 exp, and two produce an effect - ok then first publish the 2 significant ones, wait a year and then publish the failed replication...and so on...I am not convinced that this is a good solution for the problem (holds true for registered reports by the way...I have at least 100 failed exp on my hard drive of which I of course think that they were theoretically justified exp but they just did not work...I am sure many of them would be acceptable for preregistered reports...you see where this is going).

So, after many discussions with many colleagues and some key notes of the 'leading' critics on psychological science at the moment, I am not convinced that especially cognitive psychology is in a crisis (I think no other area of psychology runs so many cheap-to-replicate [in terms of effort] exps and as a consequence replications are a standard -typically researchers replicate their own surprising findings before submitting them to a journal - in my 'default view' at least)

And some more specific issues

1) Semantic priming a phenomenon closely linked to processes in memory and knowledge representation with a meticulous procedure, established methodical standards and so on is - to me - definitely NOT a close cousin to what social psychologist label 'priming'. The authors do themselves and their previous research really not a favor in suggesting that these two types of priming are somehow related.

2) I do not see a merit in EXP1. A replication with different procedures, a smaller sample and so on does not makes sense to me.

3) The paper actually has two stories. First, the one concerning semantic priming - the failed replications and the point that forward associates are not so different from backward associates after all. Ok. Second, the general aspect of self-replications. For an empirical article I always think that ONE story is sufficient...

If I am allowed I would like to suggest the following to the authors - if the AE thinks that I presume here to express an opinion that should not be expressed in a review - please feel free to delete this part of my review!

So, what about withdrawing your original paper or submit an ERRATA or short note just saying that replications failed. Simultaneously use this case as an example/motivation for a theoretical article on the replication crisis in a method journal.

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### **Editor Decision for Version 1**

**Editor:** Rolf Zwaan

**Affiliation:** Erasmus University, Netherlands

**Editor decision:** Revisions Required

**Decision date:** 13 June 2017

Dear Mr. Heyman,

Thank you for submitting your manuscript for a streamlined review to Collabra. I have read the original action letter and reviews and am happy that the original editor and reviewers have made their contributions available for open review. I have also carefully read your manuscript.

I find the reviews thoughtful and respect the viewpoints expressed in them. Reasonable people can disagree, as appears to be the case here.

Your cover letter captures well the issue that is at stake. On the one hand, there is the view that allowing authors to publish non-replications of their own work seems unfair, allows them to "double-dip," as you put it. The underlying idea here is that publications are a reward and that people should not be rewarded twice for the same thing. It is of course true that our incentive system is still such that people are rewarded for publications, so I can understand that some might find it unfair to allow researchers to publish self-nonreplications.

However, I agree with you that the overriding concern should be the scientific record. We should regard scientific publications as contributions to the accumulation of knowledge rather than as vehicles for career advancement. From this perspective, the presence of well-executed nonreplications in the scientific record is crucial. It is important for future meta-analyses as well as for researchers who might want to build on others' work.

In other words, the fact that this is a self-nonreplication to me is not a reason not to publish a

paper. That said, some of the reviewers' comments did resonate with me.

First, I agree with the reviewers that the work is well-executed. It is solid work in the area of semantic priming. Your use of Bayesian analyses is informative. This part of the work merits publication.

Second, I wholeheartedly agree with reviewer 3 when this reviewer says: "The paper actually has two stories. First, the one concerning semantic priming - the failed replications and the point that forward associates are not so different from backward associates after all. Ok. Second, the general aspect of self-replications. For an empirical article I always think that ONE story is sufficient..."

While informative in and of itself, the meta-scientific self-analysis does detract from the paper, which is about semantic priming, after all. I also note that reviewer 1 seems uneasy with the introduction. The switches from meta-science to the actual paper and back are jarring. I suggest you extract the meta-scientific part from the paper and maybe turn it into a blog post. What remains then is a solid attempt to self-replicate, something that might become more common in the literature anyway if people start caring more about the scientific record.

Third, I couldn't agree more with reviewer 3's comment: "Semantic priming a phenomenon closely linked to processes in memory and knowledge representation with a meticulous procedure, established methodical standards and so on is - to me - definitely NOT a close cousin to what social psychologist label 'priming'. The authors do themselves and their previous research really not a favor in suggesting that these two types of priming are somehow related."

The reviewer is right that these two forms of priming are faux cousins. If you cut the meta-scientific discussion from the paper, this concern about the comparison with social priming will be addressed in one fell swoop as well, of course.

To summarize, I will be happy to accept a paper that reports on the self-replication itself and that omits the meta-scientific considerations, not because these are not interesting but because these do not cohere with the main part of the paper. I appreciate the intellectual honesty and meticulous experimentation that has gone into this replication work. The scientific record will be the better for it.

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

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

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

Kind regards,

Rolf Zwaan

Erasmus University, Netherlands

rolfzwaan@gmail.com

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## Author's Response to Review Comments for Version 1

**Author:** Tom Heyman

**Affiliation:** University of Leuven, Department of Experimental Psychology

**Revision submitted:** 26 June 2017

Dear Dr. Zwaan,

Please find enclosed the revision of our original research report titled "Does a working memory load really influence semantic priming? The story of a self-replication attempt". The manuscript includes five tables and two figures. It is authored by Tom Heyman, Kirsten Goossens, Keith Hutchison, and Gert Storms. The first author serves as the corresponding author.

As requested, we removed the parts about meta-scientific issues. More concretely, we restructured and partly rewrote the introduction section. As a result, we no longer compare semantic priming with social/behavioral priming. In addition, the second half of the original general discussion was omitted. The last paragraph of the revised general discussion does touch upon some meta-scientific issues, but really in the context of the present replication attempt and not so much as a separate, more general "storyline". That said, the paragraph can simply be removed if deemed necessary. We also made the project and pre-registration publically available on Open Science Framework and changed the links in the manuscript accordingly.

Thank you for your consideration.

Sincerely,

Tom Heyman

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### Attached document:

<https://s3-eu-west-1.amazonaws.com/ubiquity-partner-network/ucp/journal/collabra/96-1076-1-ED.docx>

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## Editor Decision for Version 2

**Editor:** Rolf Zwaan

**Affiliation:** Erasmus University, Netherlands

**Editor decision:** Accept Submission

**Decision date:** 29 June 2017

Dear Dr Tom Heyman,

After review, we have reached a decision regarding your submission to Collabra: Psychology, "Does a working memory load really influence semantic priming? The story of a self-replication attempt", and are happy to accept your submission for publication, 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.

I look forward to seeing your paper online.

Kind regards,

Rolf Zwaan

Erasmus University, Netherlands

rolfzwaan@gmail.com