Main points:

**I. Where’s the nuance / What’s the take-home**

**II. Do your exclusions cause their own bias?**

III. Where’s the theory?

Reviewer 1:

1. Reviewer 1 suspects there is likely publication bias in most (social) psychological literatures. In that regard, Reviewer 1 seems to imply the current results are not particularly remarkable. We agree that most literatures in social psychology are probably influenced by publication bias to some degree. It is for this reason that analyses like ours are crucial in separating the wheat from the chaff and in directing future research. The literature should not endeavor to be “only as bad as the rest of social psychology;” it should instead endeavor to be *correct*. With this in mind, we must correct Anderson et al.’s conclusions regarding the absence of bias.

2. Reviewer 1 suggests that our conclusions are not credible given the effects of even more minimal manipulations in stimulating aggression (e.g., black-color primes, heat-word primes, pictures of guns). We have not been able to find the relevant citations for these claims. We have found the claim that that teams in black or red jerseys are charged with more fouls (Frank & Gilovich, 1988), but follow-up studies contest this claim (Caldwell & Burger, 2010). Heat-word primes have been linked not to aggressive behavior, but rather to hostile attribution bias (DeWall & Bushman, 2009). However, evidence suggests that this finding does not replicate (McCarthy, 2014). Actual temperature has been linked to aggressive behavior (Anderson, Anderson, Dorr, DeNeve, & Flanagan, 2000), but that is very different from mere lexical priming. Meier, Robinson, & Wilkowski (2006) report that aggressive lexical primes have no main effect on aggressive behavior, but that they interact with trait agreeableness. However, the evidence for even this interaction is slim: *p* = .044 under one CRTT quantification, and *p* = .262 under another.

It is our concern that the problems of p-hacking, HARKing, and selective report may be found in these other literatures on aggression. Of course, a detailed inspection of those literatures is beyond the scope of the current paper, but you get the idea. Another interesting similarity between this literature and those literatures is the conflation of cognitive outcomes (e.g., differences in reaction time to identify an aggressive stimulus) with actual aggressive behavior (e.g., blasting someone with noise). Perhaps the cognitive effects are more plausible and more replicable than the behavioral effects.

3. Reviewer 1 suggests that our results would benefit from a more thorough theoretical treatment. See our response to **I** above.

Reviewer 3:

Reviewer 3’s comments lead to a number of further revisions. We apologize that our simulation was not available as we thought it was, and appreciate Reviewer 3’s recreation. We appreciate Reviewer 3’s prudence in allowing us to leave closed this particular can of worms. We have added an appropriate footnote on page 18, as requested. We prefer to cite the Borenstein (2009) citation used by Pustejovsky rather than citing Pustejovsky directly, as this reviewer’s comment initially caused us a great amount of confusion, as we thought the complaint regarded the need for an assumed value of *w*.

[We have redone the moderator analyses as suggested. Meta-regression is PET and PEESE. Weights removed in moderator\_analyses.R]

Other comments lead to refinements in language and further support of arguments through citation.

We thank Reviewers 2 and 4 for their kind words.