In your letter, you mentioned concerns regarding the calculation of effect size estimates, attention to heterogeneity, and considerations of study quality. We address these broad concerns in this letter, with smaller, specific changes discussed later.

First, we have considered carefully the concerns about effect size calculations you and Reviewer 3 raise (McGrath & Meyer, 2006; Pustejovsky, 2014). Although both are interesting and thought-provoking reads, neither has much influence on our effect size estimates. McGrath and Meyer (2006) point out that differences in the base rates of a dichotomous outcome can inflict differences between *r* and *d*. However, the outcomes we synthesize in the current report are either continuous or modeled by original authors as roughly continuous (e.g. scales, quantities of hot sauce or noise, counts of aggressive word completions or reaction times to read aggressive words.) Thus, base rate concerns do not alter our effect sizes. Pustejovsky (2014) outlines a method to convert controlled experiments to effect size *r* (and later, *z*) by making assumptions about the levels of predictor *x* in the treatment and control groups. As we describe in our reply to Reviewer 3, the equations suggested by Pustejovsky do not seem relevant or tractable in our current research, which handles randomized experiments and bivariate sampling designs separately and does not seek to combine them. The current research could be presented just as easily in terms of Cohen’s *d*, but we prefer to stick with *r* to remain as faithful as possible to Anderson et al.’s original codings.

Pustejovsky’s point is interesting and leads us to consider the statistical assumptions inherent in the argument that “violent game effects on aggression are stronger than those of smoking on lung cancer.”

Regarding heterogeneity, we have made thorough additions to the manuscript to better handle and report heterogeneity. Models were changed to use random-effects estimators when possible, the method of random effects estimation is reported, and the I2 statistic and its 95% CI are reported for all models.

Regarding the quality of studies, we think it best not to make too many changes from the original Anderson et al. codings. Our analyses are intended as a direct reexamination of the analyses and arguments provided by Anderson et al. We are concerned that performing a new analysis based on our own assessment of quality would look as though we are making *post hoc* changes to the data to attempt to support a pet hypothesis. An earlier draft of the manuscript outlined inconsistencies in the original application of best-practices inclusion criteria, but we felt that such a discussion would lead to personal grievances and an intractable argument about the subjectivities of inclusion criteria. There is also little to consider in terms of quality that Anderson did not already measure. For example, we are not aware of any studies that blind the experimenter to the condition.

Editor:

We thank the editor for the reference to McGrath and Meyer, 2006. It’s our impression that most of the outcomes in this literature are either continuous or at least treated as such. Thus, with studies generally using equal cell sizes and continuous outcomes, the concerns about base rate mentioned by McGrath and Meyer have only minimal effect.

We went back to the literature and performed a spot check. Measures of aggressive behavior are chiefly continuous: counts or means of Competitive Reaction Time Task intensity and/or duration, quantities of hot sauce administered, duration of cold-water exposure, the Buss-Perry Aggression Questionnaire, [ETC ETC]. Measures of aggressive cognition are treated as continuous: counts of word stems completed with aggressive words, reaction times to read aggressive words, scales of attitudes towards violence, [ETC ETC]. Measures of aggressive affect are continuous, measured by scales of Likert-type questions, [ETC ETC].

[CHECK ON THIS FOR THE CROSS-SECTIONAL LITERATURE]

We recognize that some loss of fidelity is likely inflicted by the modeling of all such effect sizes in terms of *r* and *z*. For example, counts might best be modeled as negative binomials rather than as normal distributions. However, the detail of statistical report in the published record does not allow for greater precision, and it seems that such compromises are inherent to meta-analysis. At the least, we expect the loss of fidelity should not inflict systemic bias within our analyses of subgroups.

[SECTION ABOUT HETEROGENEITY GOES HERE. As requested, we have added I^2 and its 95% CI to the current report. We describe in the introduction the hazards posed by heterogeneity and mention appropriate cautions in the discussion.

We elaborate on our conclusions regarding publication bias. We consider publication bias to include not only journals’ preference for significant results, but the tendency of researchers to submit for consideration only the significant results. We expect that publication bias is probabilistic, rather than absolute; null results will therefore sometimes be published, albeit less frequently than are significant results. Furthermore, significant findings appear in prestigious journals (*Psychological Science, Journal of Personality and Social Psychology*) whereas null results are published in less prestigious journals (*Journal of Applied Social Psychology*, *Journal of Broadcasting and Electronic Media, Criminal Justice and Behavior,* or Japanese conference proceedings). Researchers may be incentivized to find significant results for a more prestigious publication. Finally, in the case that a student’s post-graduation job does not expect scholarly achievement, we suspect that significant results are more likely to be prepared for publication by advisors, post-docs, or other grad students, whereas null results are less likely to be passed on for preparation. For example, when Nicholas Carnagey left academia, his dissertation became the 2009 journal publication Anderson and Carnagey (2009). We note that in the process of publication, one experiment lost one outcome and a subgroup of approximately 50 subjects that failed to show the desired effect, while another experiment changed outcomes to one that showed a stronger effect.

We have revised the section describing each technique. We omit Figures 1 and 2 and have shortened the text.

Reviewer 1:

We ask Reviewer 1 to reconsider. The PEESE estimate Reviewer 1 considers is the single most optimistic estimator of the effect and is known to be upwardly biased when the null is true. Even so, Reviewer 1 may find it important that a decrease from *r* = .21 to *r* = .15 implies a twofold loss of explanatory power (*R*­2 falls from 4.4% to 2.3%) and the need for a twofold increase in sample size (80% one-tailed power at *n* = 136 vs. *n* = 270).

We recognize that the current findings would be inconsistent with contemporary theories of human aggression. However, to discard the evidence because it does not support the current theory would seem to be an unfortunate inversion of the scientific method.

It was not our intention to argue that the effects are necessarily minimal; rather, we meant to say that the adjusted effect size estimates are close to zero. We do not mean to argue that this is strong evidence that there is no effect. Instead, we interpret this as a sign of uncertainty and the need for a strong preregistered replication effort. We thank Reviewer 1 for pointing out the inconsistencies in our language and have made appropriate edits.

Reviewer 2:

Reviewer 2 made a number of valuable suggestions where our rhetoric grew sloppy, e.g., in our unqualified claim that PET, PEESE, and *p*-curve “provide better adjustments” for publication bias. We have softened such claims and used more precise rhetoric as advised.

We have elaborated a bit more on the problem of outcome-switching, as requested. Indeed, the flexible quantification of the Competitive Reaction Time Task (see Elson et al., 2014) may constitute a form of outcome switching.

TODO: STUFF ABOUT TRIM-AND-FILL

We have added a section cautioning the reader as to the potential influence of heterogeneity in meta-analysis in general and the degradation of trim-and-fill results in particular. We caution the reader that such heterogeneity can also influence conclusions drawn from meta-regression and p-curve techniques.

TODO: VEVEA & HEDGES 1995

TODO: USE RANDOM EFFECTS FOR EVERYTHING, EXPLICATE RE MODEL (E.G. “REML”), REPORT I2

Reviewer 3:

Reviewer 3 similarly indicated a number of places where our rhetoric was too strident. We thank Reviewer 3 for these constructive critiques and have amended our language as appropriate.

We have clarified the nature of the effect sizes being synthesized. First, we clarify that we ignored the partial effect sizes as collected by Anderson et al. We were most interested in the effects in experiments, for which the “best partials” estimates are the same as the “best raw” estimates in Anderson et al. (2010). The “best partials” in cross-sectional studies are difficult to interpret and the source of much controversy (see, e.g., Ferguson, 2015; Rothstein & Bushman, 2015). We thought it best not to touch these.

Similarly, we do not combine Pearson product-moment correlations and point-biserial correlations. We now make this clearer in the revised manuscript.

Also with attention to the computation and synthesis of effect size estimates, we thank Reviewer 3 for bringing the work of Pustejovsky (2014) to our attention. Because the original Anderson et al. meta-analysis was performed with Borenstein’s software, Comprehensive Meta-Analysis, we had attempted to mirror its approach as closely as possible. This lead us to adopt the Borenstein perspective that, as Pustejovsky says, frames conversion formulas as “applying across entire categories of effect size measure and as being reversible […] as if it were an algebraic identity.”

There are a number of reasons we are reluctant to implement the refinements suggested by Reviewer 3. First, it is our impression that the concerns raised by Pustejovsky apply chiefly when attempting to synthesize correlational designs with experimental designs. As we maintain separate meta-analyses for controlled experiments and cross-sectional designs, Pustejovsky’s suggestions would appear to be relevant only when trying to interpret the relative magnitude of the synthetized effects in experiments against those in cross-sections. We avoid making such interpretation in our manuscript, noting only that the cross-sectional research seems comparatively less compromised by small-study effects than does the experimental research.

Second, the controlled-experiment correlation estimator proposed by Pustejovsky does not seem appropriate to the current research. In correlational research, researchers inspect the correlation between some aggressive outcome (e.g., behavior) and some measure of chronic violent game exposure (e.g., hours of violent video games played in the past 5-6 years, Anderson & Dill, 2000). In experiments, researchers inspect the relationship between an aggressive outcome and a brief measure of violent game exposure (e.g., fifteen minutes with a violent game). The two paradigms consider very different mechanisms and make very different assumptions about the underlying relationship.

But if one were to use Pustejovsky’s equation 4 to synthesize these results, we submit that the results would be insensible. Recall that Pustejovsky’s equation suggests the assumption that observations in the control group have *xi* = *q* for *i* = 1, …, *n1*, whereas the observations in the treatment group all have *xi* = *q + w* for *i* = 1, …, *n2*. Here *w* indicates the displacement in *x* from the control mean *q*. If we were to consider *x* some measure of months or even years of violent game exposure, as used in correlational research, the displacement *w* inflicted by 15 minutes of play must be perishingly small. As *w* tends towards zero, the equation *rce* = tends towards 1. That is, under these assumptions, if fifteen minutes of violent game exposure can inflict any change in aggressive behavior, then the effect of fifty hours or more over some years would be nothing short of titanic. Similar concerns apply to the formula for large-sample variance of *zce*.These would seem to reflect a fundamental “apples and oranges” problem that forbids direct comparison of the experimental and cross-sectional phenomena synthesized in this literature. We think that this is a valuable and interesting insight, but feel that it is beyond the scope of the current manuscript.

[Note that *w*2 does not represent as Reviewer 3 suggests, but rather the assumed change in the predictor *X* inflicted by the experimental assignment.]

We changed our method of t-value and p-value calculation as requested by Reviewer 3. All analyses are updated in light of these new t-values.

We wish to point out some reasons why we think it unlikely that the funnel plot asymmetry is caused by a combination of heterogeneity and *a priori* power analysis. First, we understand the use of power analysis to be quite uncommon in 2010 and earlier. Reforms that placed an emphasis on power calculation did not happen until some years later, e.g. Simmons, Nelson, & Simonsohn’s (2012) 21-word solution. Second, we doubt researchers could have known the causes of heterogeneity in effect size. The Anderson et al. (2010) meta-analysis looked for many suspected moderators of the effect (e.g., sex, age, game perspective) and found none. Additionally, if Anderson knew what would permit acceptable statistical power at both N = 515 (Anderson, Gentile, & Buckley, 2007) and N = 39 (Bartholow & Anderson, 2002), then we expect he would have mentioned such moderators in the 2010 meta-analysis. Such precision in a priori power analysis would have required knowledge yet unavailable to researchers. Finally, whereas this combination of heterogeneity and power analysis will create an asymmetrical funnel plot, it will still lead to a right-skewed *p*-curve, as *p*-curve is a function of statistical power alone. It is therefore sobering that the *p*-curve for effects on behavior in experiments is essentially flat. We have uploaded a simulation demonstrating this to the OSF repo at <https://osf.io/y2jc6/>.

Reviewer 4:

We were previously using the weighted regression, unaware of the distinction between the Sterne & Egger (2005) method and the Egger 1997 method. Thus, the results have not changed, but the citation has.

Per the reviewer’s suggestions, we have added *p*-uniform and the Test for Excess Significance. We have also switched to zero-centered, contour-enhanced funnel plots to better illustrate the p = .05 threshold.

[STUFF ABOUT HETEROGENEITY AND MODERATORS GOES HERE]

We are deeply sympathetic to the reviewer’s request for data. We feel that it is very important that the data be publicly available so that readers and reviewers can better scrutinize our analyses and results. However, Dr. Anderson made it clear when he sent us his data that we did not have his permission to share it with anybody else. Prior to our original submission, we emailed him to request permission to add the data to the online repository, but he has yet to reply. We are reluctant to hector Dr. Anderson any further ourselves, but suggest Reviewer 4 might contact him directly.