Overestimated Effects of Violent Games on Aggressive Outcomes in Anderson et al. (2010)

Evidence for Short-Term Violent-Game Effects on Affect and Behavior Are Overestimated in Anderson et al. (2010)

Overestimated Causal Effects of Violent Games on Aggressive Affect and Behavior in Anderson et al. (2010)

Abstract

Violent video games are theorized to be a significant cause of aggressive thoughts, feelings, and behaviors. Important evidence for this claim comes from a large meta-analysis by Anderson and colleagues (2010) that found effects of violent games in experimental, cross-sectional, and longitudinal research. In that meta-analysis, the authors argued that there is little publication or analytic bias in the literature, an argument supported by their use of the trim-and-fill procedure. In the present manuscript, we re-examine their meta-analysis using a wider array of techniques for detecting bias and adjusting effect sizes. Our conclusions differ from those of Anderson and colleagues in three salient ways. First, we detect significant publication bias in experimental research. Second, experiments meeting these authors’ criteria for methodological quality do not find larger adjusted effects than other experiments, but instead represent a subsample of experiments in which statistical significance was more frequently found. After adjusting for bias, there is often little difference between the two estimates. Finally, after accounting for publication bias, effects of violent games on aggressive behavior in experimental research are estimated as being very small, and estimates of effects on aggressive affect are much reduced. In contrast, the cross-sectional literature finds correlations that are relatively robust to adjustments for small-study effects, but these cannot demonstrate a causal role of violent video games. We outline future directions for stronger experimental research. The results indicate the need for an open, transparent, and pre-registered research process to test the existence of the basic phenomenon.

Overestimated Effects of Violent Games on Aggressive Outcomes in Anderson et al. (2010)

Do violent video games make their players more aggressive? Given the continued popularity of violent video games and their increasing technological sophistication, even modest effects of violent games could have serious implications for public health. Psychological research provides evidence of such a link, leading professional organizations to issue policy statements describing harmful effects of violent media (AAP, 2009; APA, 2015). In the view of the professional task forces reviewing the evidence and drafting these statements, the evidence is clear enough, and the hazards certain enough, that the public should be informed and educated of the harmful effects of violent video games.

Although purported effects of violent video games are interesting in their own right, the research of these effects has implications for theories of aggression and violence writ large. Thus, telationships between these theories of aggression and the hypothesized effects of violent video games are reciprocal. Researchers test their evidence using violent-game studies, and violent-game effects are interpreted and studied in the context of theories of aggression.

Social learning theory (Bandura, XXXX) suggests that behaviors can be reinforced or discouraged by watching others perform those behaviors. Script theory (Huesmann, 1998) suggests that exposure to violent media teaches and rehearses aggressive scripts, which are then activated and performed in aggression-eliciting contexts. Another account suggests that violent games contain aggression cues, exposure to which activates aggressive cognitive networks containing aggressive scripts and perceptual schemata, thereby automatically and unconsciously influencing behavior. That is, violent games contain aggressive primes, exposure to which activates aggressive thoughts, thereby influencing behavior. With regard to short-term violent game effects in experiments among college undergraduates, it is considered likely that participants already have well-learned aggression scripts and are familiar with the likelihood of reward associated with aggressive behavior; thus, “most video game violence researchers believe that the existing short-term effects are mainly the result of priming effects” (Anderson et al., 2010, p. 155). In the long term, repeated exposure to violent games is hypothesized to make aggressive thoughts and hostile knowledge structures “chronically accessible” (Anderson & Bushman, 2002), leading to persistent changes in personality and behavior.

It is also theorized that violent games make aggression seem more normative, leading participants to interpret ambiguous behavior as aggressive (Hostile attribution bias, CITATION NEEDED) and to think aggressive behaviors are normative and appropriate (norming, CITATION NEEDED). Exposure to violent games is also expected to desensitize the player to violence (Engelhardt et al., 2011), reducing empathy and making aggressive behavior more palatable. These mechanisms are often described in combination as the General Aggression Model (Anderson & Bushman, 2002), which describes aggressive behavior as the consequence of aggressive thoughts , angry affective states, and/or arousal. Despite decades of research and hundreds of studies, however, the basic phenomena remain debated. For proponents, the effects are obvious, robust, and nearly ubiquitous. For skeptics, the research is not as clean nor the effects as obvious as has been presented. Instead, skeptics point to a host of issues including construct validity, null findings, and publication bias as undermining the evidence for violent game effects.

The proponents’ argument is advanced by a meta-analysis from Anderson et al. (2010). This meta-analysis covers 381 effect-size estimates based on 130,296 participants. The covered studies were separated into “best-practices” and “not-best-practices” subsets according to whether they met a set of inclusion criteria. The authors emphasize the best-practices subset, but provide analyses of the full sample as a sensitivity analysis. They find that in best-practices experiments there are statistically and practically significant effects of video game violence on aggressive thoughts (*r* = *.*22), aggressive feelings (*r* = *.*29), and aggressive behaviors (*r* = *.*21). Moreover, these effects are not limited to experiments but are also found in cross-sectional comparisons and even in longitudinal research designs. Anderson et al. applied the trim-and-fill procedure (Duval & Tweedie, 2000) to detect and adjust for publication bias. This procedure recommended minimal adjustment, suggesting that the research literature was only minimally contaminated by publication bias. Bushman, Rothstein, and Anderson (2010) and Huesmann (2010) call the evidence in this corpus of studies “decisive.”

Despite this meta-analysis, there are still skeptics of causal effects of violent video games on aggressive outcomes. Ferguson and Kilburn (2010), for example, are concerned that the Anderson et al. (2010) meta-analysis may suffer from biases in the publication of studies, the entry of effect sizes into meta-analysis, and the application of the best-practices inclusion criteria. Other skeptics, such as Elson, Mohseni, Breuer, Scharkow, and Quandt (2014), are also concerned that the individual studies suffer from questionable research practices such as the selective report of dependent variables that yield statistical significance. Skeptics suspect that these meta-analytic biases and questionable research practices may overestimate the strength of evidence for, and magnitude of, violent video game effects, despite the results of trim-and-fill analyses.

At the same time, there remains residual heterogeneity in the original analyses performed by Anderson et al. (2010). That is, among some designs and outcomes, effect sizes differed by more than would be expected by sampling variance alone, suggesting differences in the true effect size across studies. [THESE STUDY GROUPS SHOW HETEROGENEITY]. Anderson et al. looked for factors that could explain these differences (e.g., whether games used a 1st or 3rd person perspective, whether participants were children or adults), and generally did not find any.

To address this continued skepticism, we re-analyze the database created and used by Anderson et al. (2010). We feel this re-analysis is necessary for several reasons: First, the topic is important and controversial. Effects of violent video games are hotly debated and have implications for public health and for freedom of expression alike. Second, the Anderson et al. (2010) meta-analysis is a tremendous volume of work encompassing many studies. We were drawn to the quality and quantity of data. Third, there are promising new techniques for addressing potential publication bias and questionable research practices. These new techniques include PET (Precision-Effect Test; Stanley & Doucouliagos, 2014), PEESE (Precision-Effect Estimate with Standard Error; Stanley & Doucouliagos, 2014), *p*-curve (Simonsohn, Nelson, & Simmons, 2014a, 2014b), and *p*-uniform (van Assen, van Aert, & Wicherts, 2015). The articles introducing these techniques each contain simulations indicating that they may provide better adjustments for these potential artifacts than the trim-and-fill method used in Anderson et al. (2010). Application of these techniques, then, may yield new insights regarding the magnitude of effects on certain outcomes in certain paradigms.

**Concerns about Bias**

We were concerned about three potential sources of bias in the Anderson et al. meta-analysis. The first, *publication bias*, is the phenomenon that studies with statistically significant (e.g., *p < .*05) findings are more likely to be submitted and accepted for publication than are studies with non-significant results. The second, *p-hacking*, is the possibility that researchers increase their Type I error rates in an attempt to find publishable, statistically significant results. The last, *selection bias*, is the application of flexibility in meta-analytic inclusion criteria. We discuss each in turn.

**Publication bias.** Publication bias is a problem that contributes to the overestimation of effect sizes and the propagation of Type I error. When studies that attain statistical significance are more likely to be published than those that are not, meta-analyses of the published literature are no longer representative of the full body of research. Note that publication bias is proportionate, not absolute. The presence of some published null results therefore does not rule out the possibility of any publication bias. Note also that the bias can be inflicted at both the level of journals, which may reject null results, and authors, who may not bother submitting null results. Meta-analyses of literatures suffering from publication bias are likely to overestimate effect sizes and may reach incorrect conclusions of statistically and practically significant effects.

The critical question is whether there is evidence for publication bias in the violent video-game literature as synthesized by Anderson et al. (2010). Here there is disagreement. Anderson et al. claim that there is little evidence for publication bias. Their claim follows from their attempts to account for such bias using both statistical methods and literature review.

With regard to statistical methods, the authors used a trim-and-fill procedure to estimate bias-adjusted effect size estimates. This procedure recommended only a small adjustment, thereby suggesting a minimal degree of publication bias. This claim has two weaknesses. First, although trim-and-fill was quite popular at the time of the Anderson et al. analysis, today we understand trim-and-fill to be at least somewhatflawed. It corrects for bias when bias is absent and does not correct enough when bias is strong (Simonsohn et al., 2014b; van Assen et al., 2015). It also has difficulty adjusting effect sizes to zero when the null is true and there is publication bias (Moreno et al., 2009; van Assen et al., 2015).

With regard to literature review, the authors made an attempt to collect unpublished literature. The authors found 18 dissertations that had gone unpublished, 16 of which failed to find statistical significance on one or more outcomes. Only one unpublished non-dissertation study was found. Given the difficulty of gathering unpublished results, we suspect that there may be more unpublished non-dissertation studies censored from report. On this basis, more detailed consideration of the possibility of bias in the Anderson et al. meta-analytic dataset is warranted.

***P*-hacking.** Because statistically significant results are easier to publish, particularly in prestigious journals, researchers often strive for statistical significance. Often, this striving leads to the desired statistical significance but also causes an inflated Type I error rate; the obtained result is more likely to be a false positive. Practices that lead to this inflation of Type I error include data-dependent stopping (i.e., deciding to end data collection when *p < .*05 or continue when *p > .*05), the strategic inclusion or exclusion of outliers depending on their influence on the results, or the analysis of subgroups when the full sample fails to detect an effect. Another form of *p*-hacking is outcome switching: If an experiment’s primary outcome does not find the desired result, other outcomes with more statistically significant changes might be presented instead and the primary outcome hidden from report.

It has been argued that such outcome-switching may exist in the quantification and report of certain measures of aggressive behavior. Some researchers measure aggressive behavior by allowing participants to administer a painful burst of noise to another participant (the Competitive Reaction Time Task). Both the volume and duration of such a noise burst are measured. There is considerable diversity in the way studies have combined these quantities, and Elson et al. (2014) suggest that this diversity reflects the fact that some studies find statistical significance under one combination while other studies find significance under a different combination. In general, when researchers collect several dependent measures, there exists the possibility that there is some strategic selection among them. Such selection of the larger, more statistically significant outcomes risks overestimation of the net effect size.

**Selection bias.** Selection bias may contaminate meta-analysis when the researchers include or exclude studies on the basis of the hypothesis they favor. In that regard, the application of the best-practices inclusion criteria applied by Anderson et al. was the subject of some controversy. Ferguson and Kilburn (2010) argued that the inclusion criteria were applied more liberally to studies with significant results than to studies with nonsignificant results. If this is the case, then the best-practices subset may find larger effects not due to stronger methodology, but because of greater overestimation through selection bias. If best-practices criteria recognize quality research regardless of their results, the application of these criteria should reduce signs of publication bias. On the other hand, if best-practices criteria cause greater selection bias, then the application of these criteria would strengthen signs of publication bias.

**Assessing Bias in Meta-Analysis**

There are several approaches to assessing the aforementioned biases in meta-analysis. Some of these are recent developments published only after the publication of Anderson et al. (2010). We used these tests and methods to provide further analysis of the Anderson et al. meta-analysis. Additionally, we looked at the corpus of dissertations not published in journals and considered how their estimates differed from other collected research.

**Statistical Procedures.** A common theme in many statistical tests for meta-analytic bias is the relationship between effect sizes and standard errors (or sample size) in reported studies. In an unbiased research literature, there should be no relationship between effect sizes and standard errors; sample size does not cause effect size. However, such a relationship will be observed if publication favors statistically-significant studies at the expense of nonsignificant studies. Small-sample studies need large observed effect sizes to reach statistical significance, whereas large-sample studies can reach statistical significance with smaller observed effect sizes. Thus, in the presence of publication bias, there is an inverse relationship between effect size and precision.

One critical issue in meta-analysis in general, and the relationship between effect size and precision in particular, is whether the combined studies are similar enough to each other that synthesizing them is reasonable. When studies are similar, they are said to be homogeneous; when they are dissimilar, they are said to be heterogeneous. Meta-analysts seek to minimize the extent of heterogeneity by dividing observed effect sizes into roughly homogeneous subgroups based on their methodologies, study populations, and other features. Despite these efforts, results can nevertheless be inconsistent across observations, and conclusions must consider the challenges of heterogeneity. Heterogeneity can be estimated statistically by examining whether the variance between observed effect sizes exceeds what would be expected by sampling error alone. Most of the bias-adjustment methods below assume homogeneity, and all will have difficulty in the face of substantial heterogeneity.

Sometimes heterogeneity can cause a correlation between effect sizes and standard errors that is not due to bias. For example, experimental studies tend to have smaller samples than cross-sectional studies, and each paradigm may reflect different underlying effect sizes. Alternatively, it may be possible that manipulations and measurements in small samples are more effective than in large samples. If effect sizes are heterogeneous and researchers are performing *a priori* power analyses, there will be a relationship between effect sizes and standard errors that does not represent bias in research, as studies of the large effect will have small sample sizes and studies of the small effect will have large sample sizes. To represent these possibilities, relationships between effect sizes and standard errors are often called “small-study effects” rather than “publication bias.” Some of these possibilities can be excluded through practice. For example, conducting separate bias tests for cross-sectional and experimental studies can rule out study design as a potential cause of small-study effects.

***Funnel plots.*** Funnel plots provide a useful graphical summary of potential small-study effects in meta-analysis. The relationship between effect sizes and standard errors is plotted, allowing for visual estimation of small-study effects. In a funnel plot, effect size is plotted on the *x*-axis and precision (that is, the inverse of the standard error) on the *y*-axis. In the absence of small-study effects and heterogeneity, study results will form a symmetrical funnel shape, displaying substantial variance when sampling error is large but narrowing to a precise estimate when sampling error is small. Because of this sampling error, some small-sample studies are expected to find null or even negative results even when the underlying effect is positive, so long as there is not bias.

Such symmetry is not found in funnel plots of research contaminated with publication bias or *p*-hacking. In the case of publication bias, studies are missing from the lower portion of the funnel where results would fail to reach statistical significance or would even suggest an effect of opposite sign. This asymmetry can also be caused by *p*-hacking. When samples are collected until a desired *p*-value is attained, published studies will increase in both precision and effect size, moving towards the upper-right edge of the funnel. When subgroups or experimental subgroups are censored to highlight only a subgroup in which statistical significance was found, studies will lose precision and increase in effect size, moving towards the lower-right edge of the funnel. When outcomes are censored highlight only the significant outcomes, the effect size increases, moving studies to the right of the funnel.

Under conditions of heterogeneity, funnel plots may overestimate the degree of asymmetry (Lau, Ioannidis, Terrin, Schmid, & Olkin, 2006; Terrin, Schmid, Lau, & Olkin, 2003). Variability among studies may cause some precisely estimated studies to have effect size estimates far from the overall mean, giving the false impression of small-study effects. For this reason, homogeneity is desired for tests and adjustments related to the funnel plot. However, it is often the case in meta-analysis that heterogeneity is present and cannot be explained through consideration of moderators (Higgins, 2008).

One of the critical issues in meta-analysis is what may be learned in the presence of bias. The most charitable position is that researchers may assess the degree of bias and provide needed corrections to recover accurate effect size estimates (e.g., Duval & Tweedie, 2000; Simonsohn et al., 2014b). We are less sanguine, as much is unknown about the statistical properties of corrections—their efficiency and bias in realistically-sized samples as well as their robustness to violations of assumptions. Still, they have some value in analysis. We provide a review of the bias-detection-and-correction methods used in this study, noting the strengths and weaknesses of each.

***Egger’s regression test.*** Egger’s weighted regression test (Egger, Smith, Schneider, & Minder, 1997) inspects the degree and statistical significance of the relationship between standard errors and effect sizes. A significant test statistic suggests that the observed funnel plot would be unusually asymmetrical if the collected literature were unbiased. This test is sometimes helpful in reducing the subjectivity in visually inspecting a funnel plot for asymmetry.

Egger’s regression test has some weaknesses. Although it can detect bias, it does not provide a bias-adjusted effect size. The test is also known to have poor statistical power when bias is moderate or studies are few, limiting the strength of conclusions that can be drawn through application of the test (Sterne, Gavaghan, & Egger, 2000). Performance is also likely to degrade under conditions of heterogeneity (e.g., Lau et al., 2006; Terrin et al., 2003). Skeptics have used Egger’s test to look for evidence of bias in the violent-game-effect literature (e.g., Ferguson, 2007; Ferguson & Kilburn, 2009), but Anderson et al. (2010) abstained from its use.

***Trim and fill.*** One popular bias-adjustment technique, trim and fill (Duval & Tweedie, 2000), is used to detect and adjust for bias through inspection of the number of studies with extreme effect size estimates on either side of the meta-analytic mean estimate. If the funnel plot is asymmetrical, the procedure “trims” off the most extreme study and imputes a hypothetical censored study reflected around the funnel plot’s axis of symmetry (e.g., an imputed study with a much smaller or even negative effect size estimate). Studies are trimmed and filled in this manner until the ranks of the absolute values of the observed effect sizes on each side of the mean effect size are roughly equal.

Trim-and-fill has its critics. Moreno et al. (2009), Simonsohn et al. (2014b), and van Assen et al. (2015) argue it is not useful: when there is no bias, there is too much adjustment, and when there is strong bias, there is too little adjustment. Higgins and Green (2011) express concern about the imputation of studies, which adds purely hypothetical data to the meta-analysis. Finally, as with most adjustments for bias, trim-and-fill performs best when studies are homogeneous. Terrin et al. (2003) point out that heterogeneity may degrade trim-and-fill’s performance considerably.

For these reasons, trim-and-fill is most commonly suggested as a form of sensitivity analysis rather than a serious estimate of the unbiased effect size. When the naïve meta-analytic estimate and the trim-and-fill-adjusted estimate differ only slightly, it is suggested that the research is largely unbiased; when the difference is large, it suggests potential research bias. Anderson et al. (2010) applied the trim-and-fill procedure in their meta-analysis. The procedure yielded only slightly-adjusted effect sizes, and so the authors concluded minimal research bias. Again, the development of novel adjustments for small-study effects allows for further testing of this conclusion.

***PET and PEESE meta-regression.*** Meta-regression is a promising new tool in bias detection and adjustment. Meta-regression estimates a bias-adjusted effect size by considering the relationship between effect sizes and standard errors, then estimating the hypothetical underlying effect size that would be found if the standard error were zero. Two meta-regression estimators are the Precision-Effect Test (PET) and Precision-Effect Estimate with Standard Error (PEESE) (Stanley & Doucouliagos, 2014).

In PET, a weighted *linear* regression is fit to describe the relationship between effect sizes and standard errors, as in the Egger regression test. Unlike Egger’s test, which considers the slope of this regression, PET considers the intercept of this regression. This extrapolates from the available data to estimate what the effect would be in a hypothetical study with perfect precision. When there is minimal bias, there is minimal adjustment. When there is no underlying effect, published studies tend to lie on the boundary between statistical significance and nonsignificance, forming a linear relationship between sample size and precision. Thus, PET performs well at estimating effects when the underlying effect is approximately zero. However, PET performs less well when there is some effect. When there is an underlying effect, small studies will be censored by publication bias, but most large studies will find statistical significance and be unaffected by bias. PET will fail to model this nuance and risks underestimating the size of nonzero effects (Stanley & Doucouliagos, 2014).

A second meta-regression estimator, PEESE, is intended to address this problem. PEESE fits a weighted *quadratic* relationship between effect sizes and standard errors. This prevents the underestimation of nonzero effects by modeling bias as being stronger among low-precision studies than among high-precision studies; thus, high-precision studies are treated as relatively unbiased, as they are assumed to reliably detect the effect. Again, in the absence of bias, adjustment is minimal. PEESE is less likely than PET to underestimate nonzero effects, but risks overestimating the size of null effects (Stanley & Doucouliagos, 2014). Because PET underestimates nonzero effects and PEESE overestimates null effects, sometimes PET and PEESE are combined as a two-step conditional PET-PEESE procedure. If PET detects a significant effect, the PEESE estimate is used; if PET does not detect a significant effect, the PET estimate is used. Although this approach would seem to make use of the estimators’ complementary strengths and weaknesses, this approach may be exceedingly conservative, as PET has questionable statistical power for the detection of effects (Gervais, 2015). When PET’s power is poor, conditional PET-PEESE tends to underestimate effects, as only PET is ever applied. For this reason, we report both PET and PEESE. When the PET estimate is significant, the PEESE estimate should be favored, but when it is not significant, one should not necessarily favor PET over PEESE, as non-significant results do not guarantee the truth of the null hypothesis. Caution is also necessary in conditions of heterogeneity, under which the performance of PET and PEESE will degrade.

As one case study, these meta-regression techniques have been previously applied by Carter and McCullough (2014) to inspect the amount of evidence for “ego depletion,” the phenomenon of fatigue in self-control. They found that after adjusting for small-study effects, PET-PEESE suggested an absence of evidence for the phenomenon. The authors therefore recommended a large-sample pre-registered replication effort, which found no evidence of ego depletion (Hagger et al., in press).

***P-Curve.*** Another novel technique for accounting for small-study effects is *p*-curve (Simonsohn et al., 2014a, 2014b), which estimates the underlying effect size by inspecting the distribution of significant *p*-values. When the null hypothesis is true (i.e. δ= 0), the *p*-curve is flat: significant *p*-values are as likely to be less than .01 as they are between .04 and .05. When the null hypothesis is false, the *p*-curve becomes right-skewed such that *p*-values less than .01 are more common than are *p*-values between .04 and .05. The degree of right skew is proportionate to the power of studies to detect an effect; larger sample sizes or effects will yield greater degrees of right skew. By considering the *p*-values and sample sizes of significant studies, *p*-curve can be used to generate a maximum-likelihood estimate of the true effect size.

One weakness of *p*-curve estimation is that questionable research practices may introduce left skew into the *p*-curve. The combination of right skew from fairly reported studies and left skew from questionable studies results in a flatter *p*-curve than would be found if all studies had been fairly reported. Thus *p*-curve will underestimate the true effect size in these circumstances. Aside from this weakness, simulation work suggests that *p*-curve is quite effective at estimating true effect sizes (Simonsohn et al., 2014a, 2014b).

Another weakness of *p*-curve is that studies with *p*-values above the .05 criterion are not considered, resulting in a substantial loss of information. Consequently, the approach can be inefficient, especially when effect sizes are small or statistical significance is rare.

In contrast to the funnel-plot-based tests and estimators, which are derived from the relationship between sample size and effect size, *p*-curve is a function of statistical power. Thus, these estimators will perform differently given heterogeneous effect sizes and *a priori* power analysis. Whereas funnel plots will detect small-study effects in this scenario, spuriously suggesting research bias, *p*-curve will find that the significant studies are appropriately powered.[[1]](#footnote-1)

Heterogeneity can nonetheless influence effect size estimation using *p*-curve. *P*-curve considers only the statistically significant results, and studies with greater underlying effect sizes are more likely to attain statistical significance. This can cause *p*-curve to estimate a larger effect size than does naïve meta-analysis, as the naïve analysis considers all studies, whereas *p*-curve considers only the statistically-significant studies. Van Aert, Wicherts, and van Assen (in press) caution that this will lead *p*-curve to overestimate effect sizes under moderate to large heterogeneity.

***P-uniform.*** *P*-uniform is another power-based test and adjustment for bias (van Assen et al., 2015). Like *p*-curve, it considers only the statistically-significant results in meta-analysis. It attempts to find an underlying effect size for which the conditional *p*-value distribution would be as close to uniform as possible. That is, it looks for an effect size δ0 for which the null hypothesis *H*0: δ= δ0 would generate an approximately uniform distribution of *p*-values. It also provides a test for publication bias by considering whether the adjusted effect size is statistically significantly smaller than the naïve meta-analytic estimate. Like *p*-curve, it only considers studies with *p < .*05, and so may lose substantial information. It is also likely to overestimate effect sizes given heterogeneity (van Aert et al., in press).

**Unpublished Dissertations.** Yet another approach is to eschew statistical adjustments and attempt to inspect the unpublished literature directly. When unpublished work provides smaller effect size estimates than published work, publication bias may be present. Nonsignificant results can be difficult to retrieve for meta-analysis as they often go unpublished and forgotten.

However, one publication format is largely immune to these publication pressures: the doctoral dissertation. Department requirements generally dictate that dissertations be submitted and published in a dissertation database regardless of whether or not that dissertation is later published as a peer-reviewed journal article. Another advantage of dissertations is that they are typically thorough, reporting all outcomes and manipulations, whereas published journal articles may instead highlight only the significant results (O’Boyle, Banks, & Gonzalez-Mule, 2014). Dissertations, then, provide us with a sample of reported studies relatively uncontaminated by publication biases favoring significant results. In our analyses, we examine these unpublished dissertations and the statistical significance of their results.

**Summary of Methods**

Given this state of the field, our analysis will consist of two main questions. First, is there evidence of small-study effects in the dataset? The presence or absence of these effects will be assessed informally by inspection of funnel plots and more formally by the Egger test. Supplementary tests will be provided by *p*-uniform and the Test for Excess Significance. Second, what might be appropriate bias-adjusted estimates? We will apply PET, PEESE, *p*-curve, and *p*-uniform to estimate bias-corrected effect sizes. The answer to this second question is necessarily tentative because the statistical properties of these adjustments are only coarsely known. Finally, we will consider whether there are differences between the results of published articles and unpublished dissertations that might suggest bias.

**Method**

We perform a reanalysis of the Anderson et al. (2010) meta-analysis using the data as provided by the study’s first author. We augment the trim-and-fill approach with funnel plots, PET and PEESE meta-regression, *p*-curve and *p*-uniform analyses, and the Test for Excess Significance. We use the original authors’ separation of studies by study design (experimental, cross-sectional, longitudinal), by study outcome (affect, behavior, cognition, arousal), and by study quality (all studies, best-practices subset) in our presentation. Thus, point-biserial correlations from experiments and product-moment correlations from cross-sections are treated separately, as is generally preferred. Finally, we perform *χ*2 tests to see whether unpublished dissertations are more or less likely to yield statistical significance than other published work.

In the original dataset, Anderson et al. (2010) coded all effect sizes in terms of Pearson *r*, then converted these to Fisher’s *z*-scores with standard error equal to .[[2]](#footnote-2) This approach is appropriate given that most outcome measures are either continuous or at least modeled as continuous by study authors. We use their estimated *z*-scores and standard errors in this analysis. This approach has the benefit of providing standard errors that are not a function of effect size. Standard errors that are a function of their corresponding effect sizes can lead to the spurious detection of small-study effects.

Our inspection focuses on the raw effect sizes contained in that report. Anderson and colleagues report partial correlation coefficients for cross-sectional studies; we abstain from analysis of these. Re-analysis of the partial effect sizes is likely to be challenging due to the particularities of partial correlations (see, e.g., Aloe, 2014) and as such is omitted from the current manuscript.

All data and code have been made available online at https://osf.io/r76j2/?view\_only=0cbfaef76d0142c0864de9f28a4324e1.

**Aggregation within Studies**

As we apply them, the meta-analytic procedures assume that entire studies are censored or re-analyzed per their statistical significance. However, the original data have some studies divided into subsets to test for moderators. For example, one study might be entered as two records: one for the simple effect among males, and another for the simple effect among females. Where multiple effects were entered for a single study, we aggregated these to form a single effect size estimate by summing the sample sizes and making a weighted average of the subsample effect sizes. This parallels the behavior of the software used in the original analysis.

**Calculation of** *p***-values**

Although the original data entry performed by Anderson and colleagues is admirably thorough, the data set given us does not have the necessary statistics for *p*-curve meta-analysis. We calculated *t*-values by the equation *r* × , then used the *t*-value to calculate a two-tailed *p*-value. We do not report a *p*-value disclosure table as recommended by Simonsohn et al. (2014a), as the meta-analyzed *p*-values are a function of the data as entered by Anderson et al. and not a direct entry of *p*-values from manuscripts. Note that the *p*-values we enter thereby correspond to the main effect of violent video game exposure as entered by Anderson et al. and not the specific hypothesis tests conducted or reported by the studies’ original authors.

**Adjusted Estimates**

PET was performed by fitting a weighted-least-squares regression model predicting effect size as a linear function of the standard error with weights inversely proportional to the square of the standard error. PEESE was also performed, predicting effect size as a quadratic function of the standard error and using similar weights. Egger tests, PET, and PEESE were performed using the metafor package for **R** (Viechtbauer, 2010), using the rma() function to fit a weighted random-effects model with an additive error term.[[3]](#footnote-3) Models were fitted via restricted maximum-likelihood (REML) estimation, per package defaults. Effect sizes are converted from Fisher’s *z* to Pearson *r* for tables and discussion.

For *p*-curve, we used the R code behind version 3.0 of the online *p*-curve app (Simonsohn et al., 2014a), entering a *t*-value and degrees of freedom parameter for each relevant study. This code provides estimates in terms of Cohen’s *d*. We converted these to Pearson *r* for consistency of presentation, using the formula *r* = . Full *p*-curve output from the online p-curve.com application is available in the supplementary materials

For *p*-uniform, we use the puniform package provided by van Aert at https://github.com/RobbievanAert/puniform. Analysis was performed using the correlations and sample sizes as entered by Anderson et al. The package’s default method for the aggregation of *p*-values was used.

PET, PEESE, and *p*-curve are likely to perform poorly when there are few datapoints. Therefore, our analyses are restricted to effects and experimental paradigms with at least ten independent effect sizes. Readers wanting to generate estimates for more sparse datasets or explore the impact of our inclusion and exclusion decisions are invited to download the data and code.

**Sensitivity analysis.** In addition to our analysis of the full dataset as provided by Anderson and colleagues, we perform leave-one-out sensitivity analyses, removing each datapoint one at a time and making all adjusted estimates. A supplementary spreadsheet is attached that lists the individual studies and the estimates when they are left out.[[4]](#footnote-4)

**Studies Excluded**

We removed two studies from the meta-analytic database due to concerns over relevance and accuracy. Panee and Ballard (2002) was removed because the study tested the effects of a violent or nonviolent training level on subsequent in-game behaviors in a violent video game, not the effects of violent gameplay on aggressive outcomes. All participants played the same violent game; therefore, it does not provide a relevant test of the hypothesis. Finally, Graybill, Kirsch, and Esselman (1985) was removed from analysis. This study measured not the amount of aggressive cognition, but the direction and type of it. Because each subject was categorized into one directional and one typological category, the results do not estimate differences in aggressive cognition. As entered in the Anderson et al. dataset, the study’s manipulation checks were entered as though they were primary study outcomes on aggressive cognitions. Neither of these are hypothesis-relevant tests. As entered by Anderson et al., the effect size of the non-manipulation-check outcomes are *r* = -.02 and *r* = .02, so we are arguably being conservative by excluding this study.[[5]](#footnote-5)

**Subsets Re-analyzed**

We reproduce estimates from Anderson et al. (2010) and apply PET, PEESE, *p*-curve, and *p*-uniform to detect and adjust for small-study effects. Sufficient datapoints were available to re-analyze experimental studies of aggressive affect, aggressive behavior, aggressive cognition, and physiological arousal, as well as cross-sectional studies of aggressive affect, aggressive behavior, and aggressive cognition. As much as sample sizes permitted, studies were further divided to create separate best-practices-only and all-studies estimates per Anderson et al. (2010) as sample sizes permit.

The numbers of studies, overall numbers of participants, and naïve fixed- and random-effects estimates are provided for each subset in Table 1.

**Results**

There were sufficient experiments to re-analyze the evidence for causal effects of violent game exposure on aggressive affect (k = 34, N = 2879; best, k = 18, N = 1318), behavior (k = 39, N = 3328; best, k = 23, N = 2413), cognitions (k = 40, N = 4074; best, k = 24, N = 2887), and physiological arousal(k = 24, N = 1770; best, k = 11, N = 833), both as full samples and as a best-practices only subset. Additionally, there were enough studies to re-analyze the correlations between violent game play and aggressive affect (k = 14, N = 9811; best, k = 7, N = 9811), behavior (k = 37, N = 29113; best, k = 22, N = 12391), and cognitions (k = 22, N = 13012; best, k = 17, N = 7997) in non-experimental, cross-sectional research. These too were analyzed as both full samples and as best-practices subsamples, with the exception of cross-sectional research of aggressive affect, in which case too few studies were available to justify our tests.

Again, we wished to know whether there are small-study effects indicative of bias, and if so, what would be appropriate bias-adjusted effect size estimates. We present each in turn.

**Detection of Bias**

The first question is addressed by inspection of the funnel plots in Figures 1, 2, 3, and 4. We find dramatic funnel-plot asymmetry among experiments of aggressive affect. Application of best-practices criteria did not ameliorate this asymmetry. In fact, application of best-practices criteria seems to have exaggerated the asymmetry among experiments of aggressive affect and of aggressive behavior.

This funnel-plot asymmetry was tested by Egger’s regression. Results are provided in Table 2. The regression test for funnel-plot asymmetry was statistically significant in several subsets of the data. Egger’s test detected significant asymmetry in both the full set and best-practices subset of experiments studying aggressive affect. Notably, the Egger test was not significant in the full sample of experiments of aggressive behavior, but it was in the best-practices subsample, suggesting that the application of best-practices inclusion criteria may have exacerbated funnel-plot asymmetry. *P*-uniform also suggested significant bias only for experiments of aggressive behavior, both for the full sample and for the best-practices subsample.

In total, small-study effects are likely present in studies of violent game effects. This result is indicates that the collected meta-analytic data may be contaminated by publication, analytic, or selection biases, and may therefore yield biased overestimates of effect sizes.

**Adjusted Effect Sizes**

Table 3 summarizes the results of the *p*-curve, *p*-uniform, PET, and PEESE analyses. We caution the reader that we do not know the small-sample properties of the adjusted estimators and so do not valorize one in particular as being likely to provide the most accurate estimate of the underlying effect. Instead, we consider all estimators and look for convergence among adjusted estimates. Like Anderson et al., we will emphasize in this text the results of the analysis of the best-practices subsample.

Among experimental studies, the estimators yield larger adjustments for bias than did Anderson et al.’s trim-and-fill estimators. In general, PET and PEESE suggested substantially lower effect sizes in experimental research, whereas *p*-curve and *p*-uniform suggested smaller adjustments. The notable exception is experiments of aggressive behavior, in which *p*-curve and *p*-uniform indicated an effect size as small as did PET.

In experiments of aggressive affect, the original report suggested no adjustment was necessary for the best-practices subset. By contrast, our analyses suggested downward adjustments. Relative to the fixed-effects estimate, *p*-uniform suggested an adjustment of -.05 to *r* = .24, and *p*-curve suggested an adjustment of -.08 to *r* = .21. PEESE adjusted by -.15 to *r* = .14, and PET adjusted the effect into nonexistence (*r* = -.12). Exclusion of an outlier (Ballard & Wiest, 1996) reduced the naïve estimates (*r* = .27, fixed- and random-effects, *I2* = 0.01, [0.00, 62.8]), the *p*-uniform estimate (*r* = .20), and the *p*-curve estimate (*r* = .19). This exclusion also increased the PET (*r* = -.01, *I2* = 0.00, [NA, NA]) and PEESE (*r* = .17, *I2* = 0.00, [NA, NA]) estimates. (These unusual confidence intervals on *I2* indicate homogeneity of residuals after adjusting for small-study effects – more on this below.) Thus, exclusion of the outlier seems to have brought the adjustments into greater agreement.

In experiments of aggressive behavior, the original report suggested an adjustment of -.03 to *r* = .18. By contrast, our analyses recommended larger downward adjustments ranging from -.06–.19, reducing *r* to .15 (PEESE) or as little as .02 (*p*-uniform). Methods were conflicted as to whether the estimate was statistically significant: PEESE and *p*-curve indicated statistical significance, whereas PET and *p*-uniform did not. Our analyses also contest Anderson et al.’s conclusion that studies in the best-practices subsample find larger effects than do the not-best-practices studies. PEESE, *p*-uniform, and *p*-curve suggested identical estimates for the full sample and the best-practices subsample, whereas PET suggested that the effect was larger in the full sample than in the best-practices subsample.

In experiments of aggressive cognition, the original report suggested an adjustment of -.02 to *r* = .20. Our adjustments are divergent, perhaps due to the moderate heterogeneity among studies of this outcome. *P*-uniform suggested increasing the estimate by .02 to *r* = .24, *p*-curve suggested an adjustment of -.03 to *r* = .19, PEESE suggested adjusting by -.04 to *r* = *.*18, and PET suggested adjusting by -.12 to *r* = *.*10.

Estimates of the effects on physiological arousal seemed robust to adjustments for small-study effects. Among the best-practices subset of experiments, PEESE, *p*-curve, and *p*-uniform suggested effects as large as, or larger than, the naïve estimate. Again, the presence of moderate heterogeneity may limit the strength of the conclusions possible given the data.

Among cross-sectional studies, our estimators suggested minimal need for adjustment. PEESE, *p*-curve, and *p*-uniform all estimated effect sizes very close to the naïve random-effects estimate. However, the considerable heterogeneity in these subsets may limit the efficacy of these adjustments and may indicate the need for further consideration of differences in study methodology.

Modeling the relationship between standard errors and effect sizes also substantially reduced the heterogeneity in some subsets of the data. Among best-practices experiments of aggressive affect, no heterogeneity remained in the PET and PEESE models. Similar homogeneity was attained among experiments of aggressive behavior in both the best-practices and full samples. This suggests that there is little residual variance in study results to explain by study attributes. In the case of best-practices experiments of aggressive behavior, there was so little residual variance that a confidence interval on *I*2 consisted of the null/empty set. The documentation for metafor suggests that this indicates “highly (or overly) homogeneous data,” (Viechtbauer, 2010, helpfile for confint.rma.uni) an unusual absence of residual sampling variance. This would be consistent with the presence of bias: Effect sizes in this subset seem to reach statistical significance with improbably high precision. A similar phenomenon is apparent among the best-practices experiments of aggressive affect when one removes the Ballard & Weist (1998) outlier.

By comparison, modest heterogeneity remained among experiments of aggressive cognition and among the full sample of experiments of aggressive affect. Heterogeneity was also present among nonexperimental work, particularly in studies of aggressive affect. More work will be necessary to determine what distinguishes those studies finding larger effects from those finding smaller effects.

There are some instances of convergence in our presented estimates. When inspecting effects on aggressive behavior in experiments, *p*-curve, *p*-uniform, and PET estimated that the underlying effects were so small as to be possibly undetectable in typical sample sizes. Notably, these estimates are highly consistent with some recent reports (Engelhardt, Mazurek, Hilgard, Rouder, & Bartholow, 2015; Kneer, Elson, & Knapp, 2016; Przybylski, Deci, Rigby, & Ryan, 2014; Tear & Nielsen, 2014). For effects on aggressive affect and cognitions in experiments, *p*-curve and PEESE yielded similar estimates, suggesting that there may be detectable, nonzero effects despite overestimation.

**Unpublished Dissertations**

The funnel plots previously presented suggest the presence of substantial bias in publication or analysis. If so, then unpublished dissertations may be less likely to have found statistical significance. Figure 5 highlights the unpublished dissertation experiments with funnel plots. As one might expect given publication bias, the unpublished dissertations generally populate the left side of the funnel plot.

We applied χ2 tests to examine two relationships: First, the relationship between statistical significance and publication status, and second, the relationship between publication status and selection as meeting best-practices criteria. Table 4 includes these frequencies. The liberal counts assume independence of each entered effect size, while the conservative counts aggregate all effect sizes within each study. The aggregation in this latter counting strategy lead to three categories of studies: those that found significance on all outcomes, those that found significance on some outcomes, and those that found significance on no outcomes.

All tests were statistically significant. Across all paradigms, unpublished dissertations were much less likely to have found statistical significance than published studies (liberal and conservative tests, *p < .*001). Similarly, unpublished dissertations of all paradigms were far less likely to be included as best-practices than published studies (liberal test, *p < .*001; conservative test, *p* = *.*003). To the extent that these unpublished dissertations may reflect competent research less influenced by publication pressure, these results may be cause for concern. Similar results are also obtained when restricting these analyses to experiments: statistical significance, liberal test, *p* < .001, conservative test, *p* = .001; best-practices coding, liberal test, *p* < .001, conservative test, *p* = .001.

Meta-analytic effect size estimates were also drastically reduced within the set of experiments reported in unpublished dissertations. For aggressive affect, the random-effects estimate fell from *r* = *.*22 [.15, .29] in the full sample to *r* = *.*02 [-.10, .15] in unpublished dissertations; for aggressive behavior, the estimate fell from *r* = *.*17 [.14, .20] in the full sample to *r* = *.*01 [-.11, .12] in unpublished dissertations; and for aggressive cognitions, the estimate fell from *r* = *.*20 [.16, .24] in the full sample to *r* = *.*13 [.02, .24] in unpublished dissertations. These estimates should cause pause—they indicate that studies failing to find significant evidence for violent-game effects are more likely to go unpublished.

**Discussion**

Our findings differ from those of Anderson et al. (2010) in three important ways. First, whereas the original analysis concluded there was minimal bias, we find strong evidence of publication bias among experiments, especially for outcomes of aggressive affect and aggressive behavior. Second, the original meta-analysis claimed that methodologically strong experiments found larger effects than did methodologically weak experiments. Instead, we find that best-practices experiments may not find larger effects, but instead may represent a subset of experiments in which statistical significance was more commonly found. . Third, the original meta-analysis argued that all outcomes were statistically and practically significant. In our analysis, we find instead that the effect of violent video games on aggressive behavior in experiments is likely smaller than anticipated, and may be so small as to be very challenging to study (*r* = .02–.15). Together, these analyses indicate that the evidence for causal effects of violent video games on aggressive outcomes, particularly aggressive affect and aggressive behavior, is much weaker than once thought.

In contrast, the cross-sectional literature seems relatively unbiased, and provides clear evidence of an association between violent video game use and aggressive thoughts, feelings, and behaviors. These correlations, however, cannot demonstrate causality, and may reflect a selection process (in that aggressive people may prefer violent games) or confounding by third variables (in that some other trait or process causes people to play violent video games and to behave aggressively). The longitudinal literature appears conflicted as to whether violent games cause aggressive behavior or aggressive behavior causes violent games (e.g., Willoughby, Adachi, & Good, 2012; Breuer, Vogelgesang, Quandt, & Festl, 2015). Additionally, attempting to adjust for confounding variables such as gender appears to reduce effect sizes substantially (Anderson et al., 2010; Ferguson, 2015; Furuya-Kanamori & Doi, 2016). Furthermore, we find considerable heterogeneity in effect sizes among cross-sectional studies; future research should determine why certain cross-sections find substantially larger or smaller effect sizes.

Finally, we note that experiments do seem to provide evidence that violent games increase aggressive cognitions in experiments. However, these effect sizes are heterogeneous across studies, and the heterogeneity could not be explained by moderators of player perspective, player role, target type, gameplay duration, or the type of measure (see Anderson et al., 2010), nor could it be explained as publication bias. Future research could help by programmatically testing the possible sources of heterogeneity in primary studies.

**Limitations**

There are important limitations to the analyses we present. Although we are confident in the ability of funnel plots to detect small-study effects, we are less sure about the ability of our adjustments to provide accurate effect size estimates. We expect, at least, that they are reasonable estimates and may be closer to the truth than is the naïve estimate. Nonetheless, the statistical properties of these adjustments are not well understood, and the bias, efficiency, and robustness of these estimators are not known in any systematic or formal fashion. Moreover, they are each understood to perform poorly under certain conditions: PET underestimates non-null effects, PEESE overestimates null effects, and *p*-curve and *p*-uniform may under- or over-estimate effects in the context of *p*-hacking or heterogeneity.

These limitations of *p*-curve/*p*-uniform are particularly salient given concerns about the flexible analysis of the Competitive Reaction Time Task (Elson et al., 2014) and the presence of heterogeneity in certain analyses. It is possible that the underlying effect is substantial but our estimates are biased in some direction by *p*-hacking in one or more studies, and it is possible that some *p*-curve/*p*-uniform estimates are too high due to heterogeneity. *P*-curve and *p*-uniform also discard all non-significant results, causing a considerable loss of information.

Perhaps selection models (Vevea & Hedges, 1995) could provide a more effective and nuanced adjustment. We are particularly excited by the possibility of Bayesian selection methods (Guan & Vandekerckhove, 2016) that draw strength from reasonable prior information. The presented adjustments, in concert with our funnel plots, nevertheless have value in indicating biases and difficulties in this research literature.

Another limitation of meta-regression is that small-study effects may be caused by phenomena besides publication bias or *p*-hacking. For example, a small survey might measure aggressive behavior thoroughly, with many questions, whereas a large survey can only afford to spare one or two questions. Similarly, sample sizes in experiments may be smaller, and effect sizes larger, than in cross-sectional surveys. The current report is able to partly address this concern by following the original authors’ decision to analyze experimental and cross-sectional research separately. Still, there may be genuine theoretical and methodological reasons that larger studies find smaller effects than do smaller studies. We must insist, however, that a combination of heterogeneity and *a priori* power analysis is not likely to be one of them. Anderson and colleagues’ (2010) meta-analysis found no significant moderators of effect sizes in experiments.[[6]](#footnote-6) Thus, researchers could not have known enough about sources of heterogeneity among these studies to power experiments accordingly.

There are also substantive limitations. We abstained from inspection of the partial effect sizes from the cross-sectional studies, as these can be challenging to synthesize properly. We have also abstained from inspection of longitudinal studies as there are not enough data points to permit a good estimate. It is likely that there are small but detectable longitudinal effects of many hours of gameplay over time (e.g., Willoughby, Adachi, & Good, 2012; but see Etchells, Gage, Rutherford, & Munafo, 2016) even if the effects of a brief 15-minute exposure in an experiment are undetectably small. All the same, researchers conducting longitudinal studies should be careful to maintain a transparent research process and to publish results regardless of their significance lest the longitudinal research literature be found to suffer from similar weaknesses. Our point is chiefly that our understanding of the phenomenon as studied through experimental paradigms is likely overstated. Researchers believe they have well-controlled manipulations (Anderson et al., 2004) yielding robust, unbiased effects (Anderson et al., 2010; Bushman, Rothstein, & Anderson, 2010). We are concerned that, instead, researchers have poorly-controlled manipulations (Hilgard, Engelhardt, & Rouder, 2016) yielding uncertain effects overstated through research bias.

Finally, although the Anderson et al. (2010) meta-analysis is the most-cited meta-analysis finding evidence of effects of violent video games, it is not the only such meta-analysis. A meta-analysis by Greitemeyer and Mügge (2014) finds evidence of violent-game effects by summarizing the research literature published since the Anderson et al. (2010) meta-analysis. Our preliminary inspection of their dataset reveals less pronounced funnel plot asymmetry, although a correction has withdrawn the claim that trim-and-fill suggested the effect on aggressive outcomes had been *underestimated* by bias. The corrected manuscript now reports no adjustment suggested by trim-and-fill. We hope to re-analyze this meta-analysis in the future as well.

**Implications**

The present results have important implications for theory and research practice. Practically speaking, sample sizes may need to be much larger. Theoretically speaking, researchers may have a poorer understanding of the antecedents and causes of aggression than anticipated. We elaborate below.

**Power.** The results suggest that individual experiments studying the effects of violent video games are often badly underpowered. If the effects are indeed so small as we estimate, then researchers will be hard-pressed to detect them. For example, our largest estimate of the effect size recommended an adjustment from *r* = .21 to *r* = .15 for aggressive behavior in a well-designed experiment. Although this would seem to be a small adjustment, it is of substantial practical importance. Whereas the naïve estimate suggests a sample size of 136 is sufficient for 80% power in a one-tailed test, the PEESE estimate suggests that *n* = 270 is needed—a doubling of the sample size. The other adjustments all suggest that incredible sample sizes would be needed: *p*-curve, *r* = .09, *n* = 759; PET, *r* = .07, *n* = 1,250; *p*-uniform, *r* = .02, *n* = 15,400. These effects are smaller than those of, say, neglectful or hostile parenting (*r* = .28, Hoeve et al., 2009).

**Moderators and boundary conditions.** This poor power would have serious implications for the field’s understanding of moderators and boundary conditions of violent game effects on aggressive outcomes. Many studies report significant interactions of violent game content by individual differences such as trait anger or gender. We are concerned that the understanding of such nuance is overstated. If the main effects are so small, tests of moderators are likely to be dramatically underpowered. If power is poor, the positive predictive value of significant interactions is minimal; such significant interactions would be more likely to be Type I errors than to reflect correctly-rejected null hypotheses.

Furthermore, we suspect that significant moderators are tested and discovered *post hoc*. We expect that it is not unusual to collect a battery of brief personality measures alongside an experimental manipulation. How these measures are to be applied in analysis may be flexible — perhaps they are applied as possible moderators when a significant main effect is not found. When many moderators are tested, Type I error rates will rise substantially due to the number of tests conducted. Post-hoc exploratory analyses of moderators are valuable, but they should be reported as such, as the presentation of exploratory results as confirmatory results is misleading. One of us has published such an interaction, trait anger × violent game exposure *(1 citation removed for masked review)*, and has experienced difficulty in replicating it *(1 citation removed for masked review)*. Another exploratory analysis of ours, claiming to find effects on cognitive control *(1 citation removed for masked review)*, was likely mistaken, as such “ego-depletion” effects could not be detected in a large-scale replication effort (Hagger et al., in press). The diversity of reported moderators and the infrequency of their replication suggest possible weaknesses in the literature of violent game effects.

Finally, we are concerned that publication bias may conceal possible moderators of the effects, insofar as they exist. Imagine that a study measures the effect of games on aggressive behavior among two subpopulations, for which one has a moderate “true effect” and the other has a null “true effect”. If the null result is censored, violent-game effects appear stronger, but the moderation goes undetected.

**Unfalsifiable predictions of aggressive affect.** Of the outcomes we tested, aggressive affect had the most dramatically asymmetrical funnel plot. We suspect that this asymmetry is caused in part by inferential practices that may serve to remove these null results from meta-analysis.

Consider a hypothetical experiment comparing feelings of frustration caused by a violent and a non-violent game. If the result is significant, this is interpreted as evidence that violent video games cause aggressive feelings. However, if the test is not significant, this is not always interpreted as evidence that violent games do not cause aggressive feelings. Rather, it is sometimes taken as evidence that the games are matched stimuli, differing only in violent content and not in other confounding dimensions. The hypothesis can be changed after analyses to support the theory (Kerr, 1998).

If authors reported their null results as demonstrations of stimulus equivalence, they were excluded from meta-analysis. Anderson and colleagues (2010) are explicit about this, saying “Studies based on violent and nonviolent video games that have been preselected to be equally arousing obviously are not appropriate tests of the short-term arousal- and affect-inducing effects of violent video games. Thus, they should be excluded from the analyses designed to test this specific hypothesis. The same is true when comparison games have been preselected to create equivalent affective states” (page 156). Ambiguities in whether stimuli were truly *pre*selected threaten the validity of these meta-analytic results.

Finally, researchers may need to reconsider the role of aggressive affect in mediating putative effects of violent games on aggressive behavior. For those who enjoy video games, video game play is a highly reinforcing, intrinsically-motivating activity (Przybylski, Rigby, & Ryan, 2010). We suggest that future work critically and carefully test the proposition that aggressive behaviors performed in a video game cause players to feel angry or aggressive.

**Theories of Aggression.** It may be tempting to conclude that violent video games do not cause aggressive behavior or that the General Aggression Model is falsified. However, we feel such statements would be premature for two reasons. First, the literature synthesized by Anderson et al. (2010) is too contaminated by bias to tell with confidence whether there is or is not an effect on aggressive behavior. Because bias adjustment techniques are imperfect and disagree on the adjusted effect size, we leave this question for future research. Second, it is possible that current theories of aggression provide useful ways for thinking about aggression, but violent-game manipulations are not substantial enough to perturb the underlying causal mechanisms of aggression. That is to say, it seems trivial to suggest that people with aggressive feelings and aggressive thoughts are more likely to aggress, as predicted by the General Aggression Model. It may instead be the case that a brief exposure to violent video games does not cause aggressive feelings or inspire the kind of aggressive thoughts that lead to aggressive behavior.

With regard to this latter point of aggressive thought, we point out that there is considerable skepticism and difficulty replicating social priming phenomenon in other fields (see, e.g., money priming, Rohrer, Pashler, & Harris, 2015; intelligence priming, Shanks et al., 2013; cleanliness priming, Johnson, Cheung, & Donnellan, 2014). As in the violent games literature, social priming assumes that brief exposure to a stimulus activates related thoughts, and that the transient activation of these thoughts leads to powerful changes in behavior. We suggest careful reconsideration of this idea. What is meant by the “activation” of “thoughts”? A neuron can be excited such that it is easier to provoke an action potential from it. A word can be primed such that it is easier to identify a related word some hundreds of milliseconds later. But this does not imply that an abstract concept such as “aggression” or “smart” can be activated such that it will influence complex behaviors minutes later – these processes differ in physical and spatial size by orders of magnitude. We propose careful consideration of the magnitude of manipulations, as incidental presentations of minimal stimuli are unlikely to have substantial effects on behavior.

Another possibility is that there are important qualitative differences between (1) the kind of aggressive thought accessibility that leads a person to complete KI\_\_ as KILL instead of KISS and (2) the kind of aggressive thought accessibility that leads a person to attempt to inflict physical harm. This could explain why there is evidence violent games cause increases in aggressive cognitions, whereas the evidence for changes in aggressive behavior is weaker. **Ways Forward**

Meta-analysis, while exciting and informative, is fraught with difficult limitations. One productive way of avoiding these limitations is to conduct large-scale, collaborative, registered replication reports. In a registered replication report, collaborators review and edit the proposed methods and measures until all agree that the experiment provides a fair and effective test of the hypothesis. A sample of predetermined size is collected, and the results are published regardless of their statistical significance. This approach protects against biases caused by conditional stopping, flexible analysis, and publication pressures (see, e.g., Hagger et al., in press; Matzke et al., 2015).

We suggest that those planning such a registered report consider the use of a modified-game paradigm (Elson, Bruer, Van Looy, Kneer, & Quandt, 2013; Elson & Quandt, 2014; Engelhardt, Hilgard, & Bartholow, 2015; Engelhardt, Mazurek, et al., 2015; Kneer et al., in press). In such a paradigm, the researchers take a single video game and edit its code. This allows researchers to manipulate violent content while preserving the content of gameplay (rules, controls, level design, etc.). This would minimize concerns that observed effects of violent games are instead due to confounding differences between stimuli. By comparison, usage of commercially-available games does not allow for such control, and differences in violence are likely to be confounded with other differences in gameplay, difficulty, or competition.

Outside of a registered replication effort, there are many other ways to enhance the quality of violent games research. Researchers should consider conducting and publishing direct replications of each others’ studies. Larger sample sizes would increase the evidentiary value of individual studies. Preregistration of sample size, measures, manipulations, and analyses would reduce opportunities for conditional stopping (i.e., collecting more data if *p* > .05), censorship of studies or subgroups that fail to find an effect, and flexibility in the quantification of aggressive outcomes. Finally, the open sharing of data would allow for cross-validation: an interaction found in one experiment could then be tested in another researcher’s experiment. This would also allow meta-analyses of individual participant data, a particularly powerful and precise form of meta-analysis (see, e.g., Riley, Lambert, & Abo-Zaid, 2010), which may help to find explanations for the remaining heterogeneity.

Such data-sharing is doubly important in meta-analysis. We commend Anderson and colleagues for sharing the data and for responding to questions as to how best reproduce their analyses. We suggest that future meta-analyses routinely include the data, funnel plots, and other supplementary materials in the published record (Lakens, Hilgard, & Staaks, 2016). Other researchers should be encouraged to inspect and reproduce meta-analyses. Meta-analyses that cannot be inspected or reproduced should be regarded with concern.

**Summary**

The research literature as analyzed by Anderson et al. (2010) seems to contain greater publication bias than their initial trim-and-fill analyses and conclusions indicated. This is especially true of those studies which were selected as using best practices, as the application of best-practices criteria seemed to favor statistically-significant results. Effects in experiments seem to be overestimated, particularly those of violent video game effects on aggressive behavior, which were estimated as being very close to zero. The insights into the causes and mechanisms of human aggression purportedly gained through this research program may enjoy less empirical support than originally reported.

Rather than accept these adjusted estimates as the true effect sizes, we recommend instead a preregistered collaborative research effort and prospective meta-analysis. In this research effort, preregistration and collaboration will both be indispensable. In the absence of preregistration and collaboration, the two well-defined camps of proponents and skeptics may each find results that support their conclusions and refuse to believe the results of the other camp. We cannot bear the thought of another thirty years’ stalemate. Our best hope for an accurate and informative hypothesis test rests upon an international, collaborative, and transparent research effort including proponents, skeptics, and disinterested third parties.

**Acknowledgments**

We thank Craig A. Anderson for sharing with us the dataset from Anderson et al. (2010) and inviting us to host it publicly in our GitHub repository.

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Table 1

*Naïve effect-size estimates*

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| Setting | Group | *k* | *N* | Fixed | Random | I2 |
| Aggressive Affect | | | | | | |
| Experiment | Best | 18 | 1318 | .29 [.24, .34] | .34 [.24, .42] | 66 [45, 91] |
| Experiment | Full | 34 | 2879 | .17 [.14, .21] | .22 [.15, .29] | 72 [61, 89] |
| Cross-Section | Best | 7 | 4348 | .10 [.07, .13] | .10 [.05, .16] | 65 [12, 96] |
| Cross-Section | Full | 14 | 9811 | .15 [.13, .17] | .16 [.08, .24] | 93 [87, 98] |
| Aggressive Behavior | | | | | | |
| Experiment | Best | 23 | 2413 | .21 [.17, .25] | .21 [.17, .25] | 4 [0, 17] |
| Experiment | Full | 39 | 3328 | .17 [.14, .20] | .17 [.14, .20] | 0 [0, 7] |
| Cross-Section | Best | 22 | 12391 | .29 [.27, .30] | .30 [.25, .35] | 88 [77, 93] |
| Cross-Section | Full | 37 | 29113 | .21 [.20, .22] | .24 [.21, .28] | 91 [84, 94] |
| Aggressive Cognition | | | | | | |
| Experiment | Best | 24 | 2887 | .22 [.18, .25] | .22 [.18, .27] | 35 [0, 70] |
| Experiment | Full | 40 | 4073.5 | .20 [.17, .23] | .20 [.16, .24] | 27 [0, 67] |
| Cross-Section | Best | 17 | 7997 | .21 [.19, .23] | .21 [.15, .28] | 87 [75, 94] |
| Cross-Section | Full | 22 | 13012 | .18 [.17, .20] | .21 [.15, .27] | 91 [83, 95] |
| Physiological Arousal | | | | | | |
| Experiment | Best | 11 | 833 | .20 [.13, .26] | .21 [.11, .31] | 50 [0, 80] |
| Experiment | Full | 24 | 1770 | .14 [.09, .18] | .15 [.09, .21] | 35 [0, 71] |

*Note: K =* number of studies; *N =* total N across studies. All effect sizes in Pearson *r w*ith 95% confidence intervals.

Table 2

*Tests for bias and small-study effects.*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Setting | Group | *b*Egger | SE(*b*Egger) | pEgger | pp-uniform |
| Aggressive Affect | | | | | |
| Experiment | Best | 3.667 | 0.78 | **< .001** | 0.201 |
| Experiment | Full | 2.635 | 0.737 | **< .001** | 0.861 |
| Cross-Section | Best | - | - | - | - |
| Cross-Section | Full | 0.123 | 1.883 | 0.948 | 0.661 |
| Aggressive Behavior | | | | | |
| Experiment | Best | 1.537 | 0.549 | **0.005** | **0.002** |
| Experiment | Full | 0.451 | 0.39 | 0.248 | **0.009** |
| Cross-Section | Best | 1.163 | 0.789 | 0.140 | 0.752 |
| Cross-Section | Full | 1.326 | 0.589 | **0.024** | 0.900 |
| Aggressive Cognition | | | | | |
| Experiment | Best | 1.372 | 0.761 | 0.071 | 0.684 |
| Experiment | Full | 0.883 | 0.544 | 0.104 | 0.814 |
| Cross-Section | Best | 1.061 | 0.941 | 0.259 | 0.628 |
| Cross-Section | Full | 1.241 | 1.064 | 0.243 | 0.544 |
| Physiological Arousal | | | | | |
| Experiment | Best | 0.137 | 1.22 | 0.911 | 0.797 |
| Experiment | Full | 1.295 | 0.714 | 0.070 | 0.930 |

*Note:* One analysis omitted for insufficient number of studies. Bold text highlights tests significant at the .05 level. Tests for Excess Significance (Ioannidis & Trikalinos, 2007) are reported in a supplement.

Table 3

*Adjusted effect-size estimates.*

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | PET | I2PET | PEESE | I2PEESE | *p*-uniform | *p-*curve |
| Aggressive Affect | | | | | | | |
| Experiment | Best | -.12 [-.29, .06] | 0 [0, 83] | .14 [.06, .23] | 0 [0, 86] | .24 [.08, .36] | .21 |
| Experiment | Full | -.10 [-.27, .08] | 58 [44, 85] | .08 [-.02, .18] | 60 [47, 86] | .24 [.11, .35] | .20 |
| Cross-Section | Best | - | - | - | - | - | - |
| Cross-Section | Full | .16 [-.04, .35] | 94 [88, 98] | .17 [.04, .29] | 94 [88, 98] | .16 [.12, .24] | .16 |
| Aggressive Behavior | | | | | | | |
| Experiment | Best | .07 [-.04, .18] | 0 [\*] | .15 [.09, .21] | 0 [\*] | .02 [-.23, .15] | .09 |
| Experiment | Full | **.13 [.04, .21]** | 0 [0, 7] | .15 [.10, .20] | 0 [0, 7] | .02 [-.23, .15] | .08 |
| Cross-Section | Best | **.29 [ .16, .41]** | 87 [77, 94] | .30 [ .23, .37] | 88 [78, 94] | .28 [ .25, .32] | .28 |
| Cross-Section | Full | **.21 [ .12, .28]** | 90 [84, 94] | .24 [ .19, .28] | 91 [85, 94] | .23 [ .20, .26] | .23 |
| Aggressive Cognition | | | | | | | |
| Experiment | Best | .10 [-.05, .24] | 33 [0, 65] | .18 [.11, .24] | 32 [0, 65] | .24 [.15, .31] | .19 |
| Experiment | Full | **.11 [.00, .22]** | 29 [0, 64] | .16 [.10, .21] | 27 [0, 62] | .24 [.14, .32] | .19 |
| Cross-Section | Best | **.24 [ .07, .39]** | 88 [76, 95] | .23 [ .14, .32] | 88 [77, 95] | .19 [ .14, .24] | .19 |
| Cross-Section | Full | **.20 [ .05, .33]** | 91 [84, 96] | .22 [ .13, .30] | 91 [84, 96] | .17 [ .14, .21] | .18 |
| Physiological Arousal | | | | | | | |
| Experiment | Best | .19 [-.12, .47] | 53 [0, 83] | .21 [.04, .37] | 54 [0, 84] | .26 [.08, .37] | .28 |
| Experiment | Full | -.01 [-.18, .17] | 31 [0, 66] | .09 [.00, .17] | 32 [0, 65] | .26 [.08, .37] | .28 |

*Note: K =* number of studies; *N =* total N across studies. \* Confidence interval on *I*2 consists of the null/empty set due to highly homogeneous data. One analysis omitted for insufficient number of studies. Bold text indicates where the 95% CI of the PET estimate excludes zero, suggesting that the underlying effect is nonzero and that PEESE should be favored over PET. All effect sizes in Pearson *r*.

Table 4

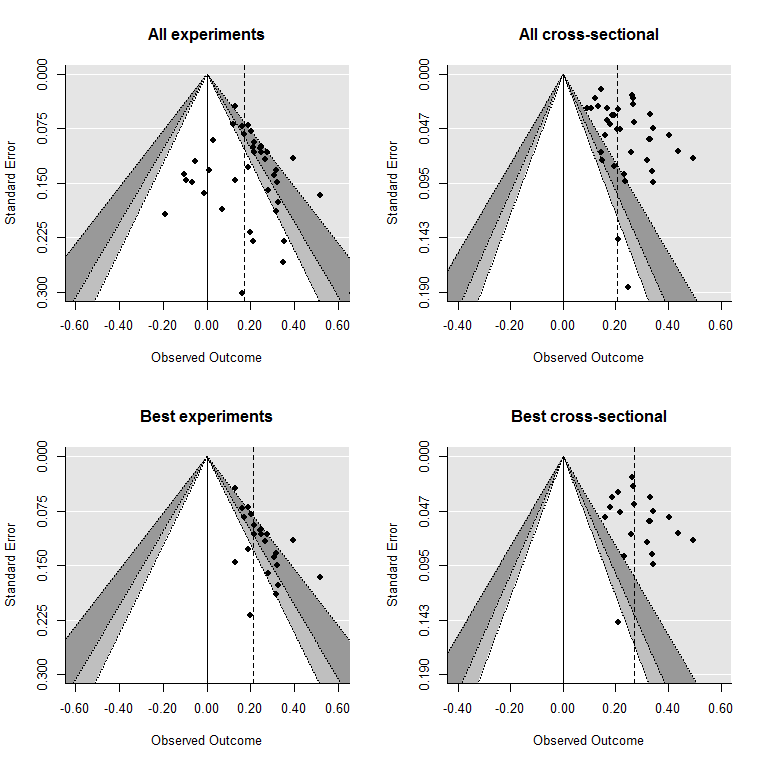
*The statistical significance and best-practices coding of effect sizes in unpublished dissertations.*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Liberal coding scheme | | | | |
|  | Statistical significance | | |  |
| Publication format | Yes | No | |  |
| Unpublished Dissertation | 4 | 30 | |  |
| Other | 201 | 125 | |  |
|  |  |  | |  |
|  | Labeled Best Practices | | |  |
| Publication format | Yes | No | |  |
| Unpublished Dissertation | 4 | 30 | |  |
| Other | 208 | 118 | |  |
|  |  |  | |  |
| Conservative coding scheme | | | | |
|  | Statistical significance | | | |
| Publication format | All outcomes | Some outcomes | | No outcomes |
| Unpublished Dissertation | 2 | 2 | | 14 |
| Other | 73 | 34 | | 29 |
|  |  |  | |  |
|  | Labeled Best Practices | |  | |
| Publication format | Yes | No | |  |
| Unpublished Dissertation | 3 | 15 | |  |
| Other | 83 | 62 | |  |



*Figure 1*. Funnel plot of studies of aggressive affect with shaded contours for *.*05 *< p < .*10 (light grey) and *.*01 *< p < .*05 (dark grey). Application of best-practices criteria seems to emphasize statistical significance, and a knot of experiments just reach statistical significance. One best-practices experiment (Ballard & Wiest, 1996) finds an implausibly large effect (*z* = 1*.*33), as does one not-best-practices cross-sectional study (Urashima & Suzuki, 2003, *z* = 0*.*60)

*Figure 2*. Funnel plot of studies of aggressive behavior with shaded contours for *.*05 *< p < .*10 (light grey) and *.*01 *< p < .*05 (dark grey). Application of best-practices criteria seems to emphasize statistical significance, and a knot of experiments just reach statistical significance. Again, application of best-practices criteria favors experiments finding statistical significance.



*Figure 3*. Funnel plot of studies of aggressive cognition with shaded contours for *.*05 *< p < .*10 (light grey) and *.*01 *< p < .*05 (dark grey). Results appear moderately heterogeneous, but somewhat less contaminated by bias. One not-best-practices cross-sectional study may be an outlier (Sigurdsson, Gudjonsson, Bragason, Kristjansdottir, & Sigfusdottir, 2006, *z* = 0*.*53).



*Figure 4*. Funnel plot of studies of physiological arousal with shaded contours for *.*05 *< p < .*10 (light grey) and *.*01 *< p < .*05 (dark grey). Results do not appear to be systematically contaminated by bias.



*Figure 5*. Funnel plots of all experiments of aggressive affect, behavior, and cognition. Dissertations not presented in any further publication format are indicated with Xs, while all other publication styles (e.g., journal articles, book chapters, conference proceedings) are indicated with filled dots. Shaded contours represent two-tailed *p*-values between .10 and .05 (light grey) and between .05 and .01 (dark grey). Nonsignificant results are less likely to be published, and in the case of experimental studies of affect and of behavior, dissertations suggest substantially smaller effects.

1. See supplementary file p\_curve\_power\_analysis.R for a simulation. [↑](#footnote-ref-1)
2. As a reviewer points out, this approximation is technically only correct when the effect size is zero; as the effect size increases, the standard error becomes smaller than . Still, we prefer this estimator because it eliminates an inherent correlation between effect size and standard error, thereby avoiding potential bias in meta-regression tests. Additionally, the approximation is good when effects are not too large, as here. See, e.g., Borenstein, 2009, p. 226. [↑](#footnote-ref-2)
3. We also fit fixed-effects models with a multiplicative error term. These are available in supplementary file [XXXX NEEDED] [↑](#footnote-ref-3)
4. Initially, we had attempted a different sensitivity analysis in which we removed datapoints with a Cook’s distance of more than 0.5 on the PET regression. In the case that several observations were excessively influential, we performed an iterative procedure, deleting the single most influential observation and checking again for influence until no observations had excessive influence. In practice, this tended to delete all datapoints that did not fit the PET regression well. This seemed to inappropriately favor the PET model over the available data, so we abandoned this approach. [↑](#footnote-ref-4)
5. In their original report, Anderson et al. (2010) report trim-and-fill analyses only for the “best practices” experiments and “best partials” cross-sections. Of these exclusions, only Panee and Ballard (2002) has any effect sizes entered as best-practices experiments (one, aggressive affect). We tested the degree to which this exclusion changed the results of naïve and trim-and-fill analysis. Even without this exclusion we were unable to reproduce their trim-and-fill result for aggressive affect: they report *r+* = .294, with zero imputed studies, whereas we get *r+* = .247, with six studies imputed to the left side of the funnel plot. See the supplement for details. [↑](#footnote-ref-5)
6. Moderators confounded with effect size could give the false impression of publication bias. We looked for such a confounding, and found none: see supplementary file moderator\_inspection.R. In general, we agree with Anderson et al. (2010) that there is little evidence of moderation between studies. [↑](#footnote-ref-6)