

Solving the Puzzle of Null Violent Media Effects

Brad J. Bushman¹ and Craig A. Anderson²

¹ School of Communication, The Ohio State University

² Department of Psychology, Iowa State University

Over the past half-century, many professional and scientific organizations have issued statements about the harmful effects of violent media on children and adolescents. Most pediatricians, media researchers, and parents also believe that violent media can be harmful. Although most studies report significant violent media effects, some report null effects. In this article, we address the puzzle of why some studies obtain null effects. Some possible factors include the use of inappropriate or excessive number of covariates, small samples, trait measures when state measures are more appropriate, poor predictor and outcome measures, and poor procedures to control for suspicion. We also discuss statistical, methodological, and theoretical issues that underlie some “failures” to replicate in experimental, cross-sectional, and longitudinal studies. The differences between studies that do and do not report significant violent media effects are explainable; sometimes they are intentional because different scholars have different objectives. We offer best practice suggestions for conducting violent media research, which we hope will be useful for editors, reviewers, researchers, pediatricians, politicians, parents, and others trying to evaluate violent media research. Finally, we also show the importance of our analyses and suggestions for other media research, for other research topics, and for the replication crisis in psychology.

Public Policy Relevance Statement

There are good theoretical and practical reasons to expect that exposure to violent media (e.g., TV programs, movies, video games) will increase aggression. This article proposes several solutions to the puzzle of why some studies find nonsignificant violent media effects, even though most studies find significant effects.

Keywords: media violence, aggression, null effects, partial effects, replication crisis

Another hallmark of truth is that it snaps things together. . . . it’s like the act of snapping the puzzle pieces together.

—Jordan Peterson, Canadian professor of psychology

The purpose of this article was to address the puzzle of why some scholars find significant violent media effects, whereas other scholars find nonsignificant effects. For example, a meta-analytic review of violent video game studies found that the results from Dr. Christopher Ferguson’s lab were consistently nonsignificant ($r = .03$, 95% confidence interval (CI) $[-.05, .10]$, $k = 8$ studies) compared with the results from our labs ($r = .20$, 95% CI $[.15, .25]$, $k = 19$ studies) and the results from other labs ($r = .18$, 95%

CI $[.16, .21]$, $k = 58$; Greitemeyer & Mügge, 2014, Table 3). This article attempts to make sense of conflicting findings.

Violence is a common theme in video games (Hartmann et al., 2014) as well as in other forms of entertainment media, including TV programs (Hetsroni, 2007; Krongard & Tsay-Vogel, 2020), films (Bushman et al., 2013), music videos (Martin & Collins, 2002), and even advertisements (Jones et al., 2014). Indeed, violence is a common theme even in media rated as appropriate for children, such as in E-rated video games for “Everyone” (Thompson & Haninger, 2001) and in G-rated films for “General Audiences” (Yokota & Thompson, 2000). The amount of violent content in the media also appears to be increasing over time. For example, the number of acts of gun violence in PG-13 films (for ages 13+) has more than tripled since the rating was introduced in 1985 (Bushman et al., 2013), a trend that continues in more recent years (Romer et al., 2017).

Many professional and scientific organizations have issued statements about the potentially harmful effects of exposure to violent media, including American Academy of Child and Adolescent Psychiatry, American Academy of Family Physicians, American Academy of Pediatrics, American Medical Association, American Psychiatric Association, American Psychological Association, Australian Psychological Society, Canada Action Agenda, International Society for Research on Aggression, Society for the

This article was published Online First September 2, 2021.

Brad J. Bushman  <https://orcid.org/0000-0002-1266-5101>

Craig A. Anderson  <https://orcid.org/0000-0001-6353-0023>

We thank Ariel Aloe for his help with the section on meta-analyzing partial effect sizes. We thank Morgan Wang for making Figure 2.

Correspondence concerning this article should be addressed to Brad J. Bushman, School of Communication, The Ohio State University, 3016 Derby Hall, 154 North Oval Mall, Columbus, OH 43210, United States. Email: bushman.20@osu.edu

Psychological Study of Social Issues, United Nations, and the United States (US) Surgeon General.¹ Some harmful effects include increased aggression against others, increased fear of becoming a violence victim, and desensitization to the pain and suffering of others. Similarly, surveys show that the overwhelming majority of pediatricians, media researchers, and parents believe that exposure to violent media can increase aggression in children (Bushman et al., 2015a, 2015b).

Yet, some scholars are publishing null results and are publicly proclaiming that there are no harmful effects from violent media exposure. Given the size and overall consistency of the research literature, we find it puzzling, both theoretically and practically, to deny violent media effects. As we summarize below, it makes theoretical and practical sense to conclude that exposure to violent media can increase aggression.

Numerous Theories Predict Violent Media Effects

Numerous theories predict that exposure to violent media should increase aggression, including social learning theory (Bandura, 1973), social-cognitive theory (Bandura, 2001), script theory (Huesmann, 1986), cognitive neoassociation theory (Berkowitz, 1990), excitation transfer theory (Zillmann, 1979), and social information processing theory (Wyer, 2012). Metatheories such as the general aggression model (Anderson & Bushman, 2002a, 2018) and I-cubed theory (Finkel & Hall, 2018) can also predict that violent media should generally increase aggression. The psychological processes that underlie the link between exposure to violent media and aggression can be divided into those that produce short-term violent media effects on aggression, and those that produce long-term violent media effects (Bushman & Huesmann, 2012). Short-term violent media effects are mainly due to three psychological processes: (a) increases in physiological arousal stimulated by media violence, (b) the priming or activation of aggressive concepts in memory, and (c) simple mimicking of aggressive behavior. Long-term violent media effects are mainly due to two psychological processes: (a) social learning that occurs by observing an aggressive model and (b) desensitization of emotional processes that occurs after repeated exposure to violent media. Other paths have also been identified (Anderson & Bushman, 2018). In summary, there is no shortage of theories that posit a link between exposure to violent media and aggression.

Solving the Puzzle of Null Violent Media Effects

Based on basic psychological theory, exposure to violent media should influence those who consume it. The bulk of studies show this to be the case (Anderson et al., 2010; Anderson & Bushman, 2002b; Bushman & Huesmann, 2006; Greitemeyer & Mügge, 2014; Paik & Comstock, 1994). Yet, some scholars find null effects; a few consistently find null effects.

Null effects are obtained when the explained variance is relatively small or (conversely) the unexplained (error) variance is relatively large. For example, the *F*-test is a ratio of explained to unexplained variances (i.e., *M* squares). To reduce false null effects (type II errors), researchers can increase explained variance, decrease unexplained variance, or both. Conversely, to increase false null effects, researchers can decrease explained variance, increase unexplained variance, or both. In Tables 1 and 2,

we list some factors under each category. We also indicate whether each factor primarily applies to experimental studies, cross-sectional studies, and longitudinal studies. This list of factors is meant to be illustrative, not exhaustive.

Of course, the factors listed in both tables might also be important to test various theoretical explanations. For example, one of the threats listed in Table 1 is "Excessive Delay Between Exposure to Violent Media and Measurement of Aggression." For those studying priming or arousal, the amount of time between exposure to violent media and measurement of aggression is a theoretically interesting variable rather than a threat.

Figure 1 illustrates some of the ways in which true violent media effects can be hidden or underestimated by inappropriate data analyses. Panel A, for instance, shows the inappropriateness of statistically controlling for trait aggression when testing the effect of media violence on bullying behavior, that is, testing whether Area C is statistically significant when in fact one should be testing whether Area B + C is statistically significant. This is inappropriate for at least three reasons. First, trait aggression measures often include aggressive behavior items (e.g., "I get into fights a little more than the average person"; Buss & Perry, 1992). Thus, controlling for trait aggression means that you have partialled out the dependent variable from itself. Second, trait aggression measures often include items assessing mediating variables (e.g., "I sometimes feel like a powder keg ready to explode"; Buss & Perry, 1992), so controlling for trait aggression means that you have partialled out the theoretically expected underlying process (e.g., angry feelings), which should reduce Area C to nonsignificance in Panel A (if all mediators are partialled out). Third, trait aggression is often viewed as a moderator variable rather than a nuisance control variable. Specifically, violent media are more likely to increase aggression-related outcomes in people high in trait aggression than in people low in trait aggression (Bushman, 1995).²

Panel B shows the inappropriateness of statistically controlling for TV violence when testing the effect of video game violence on aggression (or vice versa), that is, testing Area B (or D) when the media violence hypothesis says that one should test Area B + C + D. However, some researchers enter multiple types of media violence exposure measures as predictors, test only the smallest areas (e.g., B, then D) for significance, and finding them each to be nonsignificant, go on to claim that media violence does not affect aggression.

Some scholars control for variables other than trait aggression, mediators, and exposure to violent media. Colloquially, they

¹ See: <http://u.osu.edu/bushman.20/statements-on-media-violence-effects-by-major-scientific-groups/>.

² There are occasions in which it is appropriate to partial out correlated trait aggression measures. For example, when a study is focused on the effects of a brief experimental manipulation on immediate affective, cognitive, or behavioral reactions, it is entirely appropriate to control for trait aggression, if the measure is taken prior to the experimental manipulation. Another example is when the focus is on testing the relative strength of correlated predictors of aggression in cross-sectional and longitudinal studies, which we have previously labeled as a "destructive testing" strategy. But, when the focus is on whether there is a reliable association between media violence exposure and aggressive behavior, partialing out a measure that assesses mediating variables or the dependent variable is inappropriate. The worst case is when the trait aggression measure and the dependent variable are actually measuring the same thing, that is, when factor analyses fail to show that the two measures are measuring two different things. This same reasoning holds for partialing out the independent variable (Panel B).

Table 1*Some Factors That Can Decrease Variance Explained by Violent Media in Experimental, Cross-Sectional, and Longitudinal Studies*

Factor	Type of study		
	Experimental	Cross-sectional	Longitudinal
Inappropriate or Inadequate Cover Story	X	X	X
Lack of Suspicion Check	X	X	X
Inappropriately Matched Violent and Nonviolent Media	X		
Lack of a Manipulation Check (or Pilot Testing)	X		
Overcontrol of a Mediating Variable (see Figure 1, Panel A)	X	X	X
Overcontrol of Media Violence Variable (See Figure 1, Panel B)	X	X	X
Use of a Weak or Inappropriate Media Violence Variable	X	X	X
Insufficient Exposure to Violent Media	X		
Overcontrol of an Aggression Outcome (See Figure 1, Panel A)	X	X	X
Overcontrol by Having Too Many Covariates Relative to Sample Size	X	X	X
Use of a Weak or Inappropriate Aggression Outcome	X	X	X
Confusing Theoretically Distinct Concepts (e.g., Aggressive Thoughts, Angry Feelings, Aggressive Behaviors)	X	X	X
Excessive Delay Between Exposure to Violent Media and Measurement of Aggression	X		
Use of Extremely Rare Aggression Measures (e.g., School Shootings)		X	X
Jumping Levels of Analysis (Ecological Fallacy)		X	X

control for “everything but the kitchen sink.” This phrase originated in the early 1900s and became popular during World War II, where it was said that “everything but the kitchen sink” was thrown at the enemy (theidioms.com). Some examples include demographic variables (e.g., gender, race, ethnicity, income), individual differences other than trait aggression (e.g., attitudes toward violence, antisocial personality, trait empathy), parental variables (e.g., parental attachment, parental enforcement), domestic violence variables (e.g., yelling in the home, violence in the home, having been hit by an adult, Conflict Tactics Scales; Straus, 1979), and neighborhood variables (e.g., poverty neighborhood, exposure to delinquent peers). All of these variables are related to aggression and could therefore reduce the apparent magnitude of the relation between media violence and aggression.

Tables 1 and 2 and Figure 1 can help solve the puzzle of why some scholars obtain null violent media effects. Of course, there is never complete consensus in any scientific field, but it is important to try make sense of conflicting findings. We provide here some examples of studies that found null violent media effects that contain one or more of the problems listed in Tables 1 and 2 and Figure 1. The examples are meant to be illustrative rather than exhaustive.

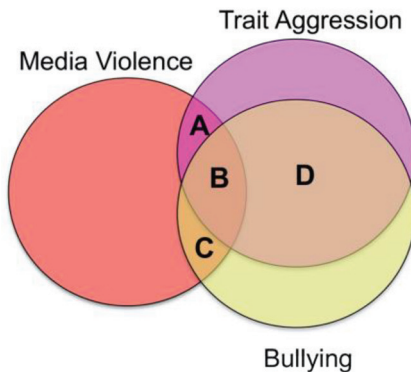
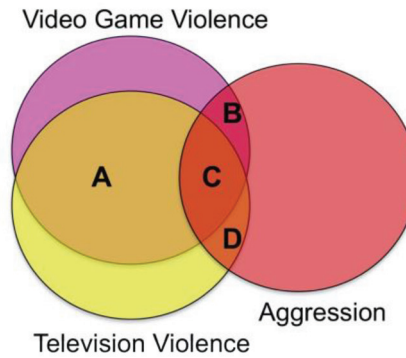
Perhaps the simplest explanation for null results in any study is low statistical power. Statistical power is determined by effect size, sample size, and significance level. Meta-analytic reviews

indicate that violent video game experiments typically have an effect size of about $d = .40$ (Anderson et al., 2010). The recommended power for studies is .80 (i.e., there is an 80% chance of rejecting a false null hypothesis). Using a .05 (two-sided) significance level, it requires 99 participants in each video game group to obtain .80 power, or 198 total participants if there are two groups (e.g., violent video game vs. nonviolent video game). There are several examples of studies reporting null violent media effects with low statistical power, ranging from .25 to .51 (Dowsett & Jackson, 2019; Ferguson et al., 2008, Study 1; Ivory & Kalyanaraman, 2007; Jerabeck & Ferguson, 2013).

One problem for psychological studies in general and violent media studies in particular is that participants might become suspicious about the purpose of the study, and act in an unnatural way. In other words, the results might be due to demand characteristics (participants attempting to confirm or disconfirm the guessed hypothesis) rather than exposure to violent media (Orne, 1962). Thus, it is important to have compelling cover stories and suspicion checks. Suspicion-based effects are especially likely to occur in pretest/posttest designs, where researchers measure aggression-related outcomes (e.g., aggressive thoughts, angry feelings, aggressive behavior) both before and after exposure to violent media. Participants probably quickly figure out that the study is about violent media. If participants are gamers, and the study is about video game violence, they might

Table 2*Some Factors That Can Increase Unexplained (Error) Variance in Experimental, Cross-Sectional, and Longitudinal Studies*

Factor	Type of study		
	Experimental	Cross-sectional	Longitudinal
Insufficient Statistical Power (e.g., Due to Small Sample Sizes)	X	X	X
Undercontrol by Lack of Appropriate Covariates	X	X	X
Failure to Check Statistical Assumptions (e.g., Normality)	X	X	X
Failure to Check for Outliers	X	X	X
Use of a Theoretically Inappropriate Population	X	X	X
Use of a Weak or Inappropriate Media Violence Variable	X	X	X
Use of a Weak or Inappropriate Aggression Outcome	X	X	X
Jumping Levels of Analysis (Ecological Fallacy)		X	X

Figure 1*Inappropriate Use of Covariates in Violent Media Research.***Panel A: Partialing out the Dependent Variable****Panel B: Partialing out the Independent Variable**

Note. See the online article for the color version of this figure.

even try to sabotage the research study (Bender et al., 2013). However, some studies that use pretest/posttest designs do not measure suspicion levels in participants (Unsworth et al., 2007; Williams & Skoric, 2005).

Some studies have used weak or inappropriate media violence variables (Przybylski & Weinstein, 2019; Williams & Skoric, 2005). One occasional problem in experimental studies is that the supposedly nonviolent “control” conditions sometimes use video games that involve characters trying to harm one another or have guns or other weapons present even if they are not to be used. Research has shown that the mere presence of a weapon can increase aggression, called the “weapons effect” (Berkowitz & LePage, 1967; for a meta-analytic review, see Benjamin et al., 2018). For instance, playing a normally violent game in stealth mode (e.g., with instructions to rescue hostages with as little violence as possible) still primes aggressive thoughts and angry feelings, and thus should not show much difference from the violent game in experimental studies. Similarly, replacing normal bullet-shooting guns with less harmful tennis-ball shooting guns still should prime “gun” and “shooting” schemas, and therefore make poor nonviolent control conditions. In short, violent media do not need to contain death and blood and gore to prime existing aggressive schemata and scripts in memory. In fact, many violent children’s video games involve hitting and killing other game characters but without blood and gore. But some researchers have used such games as the control or the nonviolent video game, to which a gorier game is compared. For example, one study used *Grand Theft Auto III* as the violent game, and *The Simpsons: Hit & Run* as the nonviolent game (Brady & Matthews, 2006). However, the latter game contains characters trying to harm other characters, and therefore should also be considered violent. Other studies suffer from this same problem (Elson et al., 2015). Research has shown that even children’s nongory violent games with happy music and cartoonish characters can increase aggression in children and even college students (Anderson et al., 2007).

Other studies use weak or inappropriate aggression outcome variables (Przybylski & Weinstein, 2019), such as the Strengths and Difficulties Questionnaire (Goodman, 1997), which was never

intended to measure aggressive behaviors. Other studies have used outcome variables that are not directly related to aggression and violence, such as rule-breaking and nonviolent criminal behavior (Ferguson, 2011); this is acceptable only if one does not use any resulting null effects to claim that media violence does not affect aggression or violence. Some violent media studies use trait measures for outcome variables when state measures are theoretically more appropriate. Personality traits are considered to be stable over time and across situations, whereas states are more temporary and are often determined by situations (Gaudry et al., 1975). It is unreasonable to expect a few minutes or even hours of violent game play to influence stable personality traits or attitudes. Sometimes researchers even use trait measures for outcome variables that are not directly relevant to aggression and violence (i.e., delinquency, venting when angry, seeking catharsis through violent gameplay; Ferguson et al., 2014). Dichotomous outcome variables have been used in some studies (DeCamp & Ferguson, 2017; Williams & Skoric, 2005), which are weaker than continuous variables (DeCoster et al., 2009; MacCallum et al., 2002).

Overcontrol is a potential problem in many studies that report null violent media effects. Some studies have controlled for trait aggression (Ferguson et al., 2014; Przybylski & Weinstein, 2019; Teng et al., 2011). Other studies have controlled for aggression-related mediators, (Przybylski & Weinstein, 2019). Several studies have controlled for the “kitchen sink.” For example, one study controlled for violent media propensity, parental attachment, youth disclosure, parental enforcement, yelling in the home, violence in the home, having been hit by an adult, gender, race and ethnicity, and poverty (DeCamp & Ferguson, 2017). Another study controlled for gender, parental involvement, stress, family/peer support, extracurricular activity level, and trait aggression (Ferguson et al., 2014). A one-year prospective study controlled for gender, depressive symptoms, pretest scores, neighborhood, negative adult relationships, antisocial personality, family attachment and delinquent peers, the Family Environment Scale (Moos & Moos, 2002), the Conflict Tactics Scale (Straus, 1979), psychological aggression, and physical assault, prior to adding TV and video

game violence exposure in the final step of a hierarchical regression analysis (Ferguson, 2011).

Of course, some of these factors we have discussed can be used to bias findings in the other direction as well—by inflating violent media effects. The important point is to identify the factors and consider whether and/or how they might influence the results in inappropriate ways.

Zero-Order, Partial, Semipartial Correlations, and Standardized Multiple Regression Coefficients

One possible solution to the puzzle of null media violence effects is the type of correlation authors report. Specifically, authors can report zero-order, partial, or semipartial correlations in their studies (Cohen et al., 2003). A *zero-order correlation*, also called a *Pearson correlation* (r), is the correlation between the independent variable (e.g., violent media) and the dependent variable (e.g., aggression) without controlling for the influence of any other variables. Because it only examines the correlation between two variables, it is also called a *bivariate correlation*. It is assumed that both variables are normally distributed (i.e., bivariate normal). A *partial correlation* is the correlation between the independent variable and the dependent variable after controlling or adjusting for the influence of other variables on both the independent and dependent variables. The *semipartial correlation*, also called the *part correlation*, is the correlation between the independent variable and the dependent variable after controlling for the influence of other variables on the independent variable. The semipartial correlation does not control for the influence of other variables on the dependent variable. When squared, the semipartial correlation indicates how much unique variance the independent variable explains in the dependent variable. In other words, this is the change in R^2 when a variable is entered last in a regression model.

Although some meta-analysts argue that only zero-order correlations should be combined in a meta-analysis (Rothstein & Bushman, 2015), other meta-analysts argue that partial and semipartial correlations can also be combined in a meta-analysis using metaregression (Aloe & Becker, 2012; Aloe & Thompson, 2013). Metaregression is a tool used in meta-analysis to examine the impact of moderator variables (the X variables) on effect sizes (the Y variable) using regression-based techniques (Thompson & Higgins, 2002; Tipton et al., 2019).

Other researchers use the standardized regression coefficient (beta or β) from a multiple regression model, which indicates the expected change in the dependent variable (in standard deviation units) due to an increase of 1 SD unit increase in the independent variable (holding all other variables in the regression model constant). In simple linear regression, if the independent variable and dependent variable are both standardized, the standardized regression coefficient equals the correlation coefficient. Some scholars have suggested that standardized regression coefficients can be substituted for missing correlations (Peterson & Brown, 2005). Specifically, in this approach $r = \beta + .05$ if β is positive, and $r = \beta$ if β is negative. This substitution is very attractive to practicing meta-analysts because many research reports do not include correlations but do include betas from multiple regression models. However, analyses of real and simulated data have found that when β s are used to estimate correlations, they are associated with large biases in estimating the population correlation and even larger biases in estimating its standard deviation (Aloe, 2015; Roth et al., 2018). Roth et

al. (2018) urged a “return to the standard practice of using only existing correlations in meta-analysis” (p. 644).

In summary, partial correlations, semipartial correlations, and standardized regression should not be used as replacements for zero-order correlations in a meta-analytic review because they are inaccurate (Aloe, 2015; Aloe & Thompson, 2013). A compromise would involve reporting meta-analysis results on both types of input data because they are answering different questions (Anderson et al., 2010, “best raw” vs. “best partials”). Bivariate effects only examine the relationship between the independent and dependent variables, whereas partial effects control or adjust for other variables.

Different Researchers Might Have Different Objectives

It is likely that the “debate” about violent media effects can be partially resolved by understanding the objectives of the two “camps.” In his own primary studies and also in his meta-analytic reviews (Ferguson, 2015), Dr. Ferguson and colleagues have consistently used partial effects that control for variables other than violent media exposure. In any given primary study, this approach is appropriate under four conditions: (a) one is interested in the unique effects of violent media on aggression after controlling for other factors related to consumption of violent media and/or aggression; (b) inappropriate covariates—such as proxies for the independent, dependent, theoretically consistent mediating variables—are excluded; (c) the “kitchen sink” approach described earlier is avoided, and (d) the sample size is sufficiently large relative to the number of covariates. Both of us have published numerous primary studies that do this, both within the media violence domain as well as in other research domains (Anderson et al., 2017; Burnay et al., 2019). But many of the primary study replication failures in the media violence domain violate these important restrictions, as noted in an earlier section.

Similarly, treating a child’s aggression level at Time 1 as a covariate in longitudinal studies is appropriate if one wants to test whether exposure to violent media at Time 1 increases their level of aggression at Time 2 above their initial aggression level at Time 1 (Anderson et al., 2007). But again, adding inappropriate covariates (i.e., overcontrolling) invalidates the conclusions of some “null effect” studies.

In contrast, most scholars (including us) are primarily interested in the relation between violent media and aggression without overcontrolling for other variables. Furthermore, the statistical problems of using partial correlations in meta-analytic reviews lead most scholars to focus on bivariate correlations in their meta-analytic reviews, thereby avoiding both the statistical and the overcontrol issues. As noted earlier, a compromise position that seems useful (especially in the longitudinal context) is to present both the zero-order and the “best partials” results, excluding all overcontrol variables, of course.

Rather than detailing the “failed to replicate” media violence and aggression studies that are fatally flawed by the use of inappropriate controls, we invite interested readers to apply Tables 1 and 2 whenever evaluating media violence research.

Interpreting the Magnitude of Violent Media Effects

Of course, there is more to an effect than whether it is statistically significant at the .05 significance level. For decades, statisticians have issued warnings about the misuse of p values (Baker, 2016). It is perhaps more important to consider the magnitude of media violence effects. Most meta-analytic reviews find average correlations of about $r = .20$ for violent media-related aggression (Anderson et al., 2010); very few meta-analytic reviews of violent video game effects find correlations less than .10 or greater than .20 (Mathur & VanderWeele, 2019).

Scholars have offered different ways to interpret the magnitude of effects. Conventional values have been offered for “small” ($r = .10$), “medium” ($r = .30$), and “large” ($r = .50$) effects (Cohen, 1988). Empirical values have been reported for the 25th ($r = .12$), 50th ($r = .19$), and 75th ($r = .30$) percentiles in the social sciences (Lipsey & Wilson, 1993). According to the Promising Practices Network (2014), terms such as *significant*, *important*, *notable*, and *consequential* are typically reserved for effects at least $d = .25$, which corresponds to $r = .12$. It is interesting to note that the average effect obtained from 322 meta-analyses of more than 25,000 social psychology studies involving over 8 million participants conducted over the past 100 years was $r = .21$ (Richard et al., 2003), which is about what most meta-analytic studies of violent media effects find. Of course, exposure to violent media is not the only risk factor for aggression, but it

is not a trivial risk factor either (Gentile & Bushman, 2012). As Götz et al. (2021) note, psychological phenomena are most likely determined by a multitude of causes, with any individual cause likely having only a small effect.

It is difficult to interpret the magnitude of any effect size in the absence of theory. Fortunately, there are many theoretical reasons why exposure to violent media should increase aggression, as noted previously.

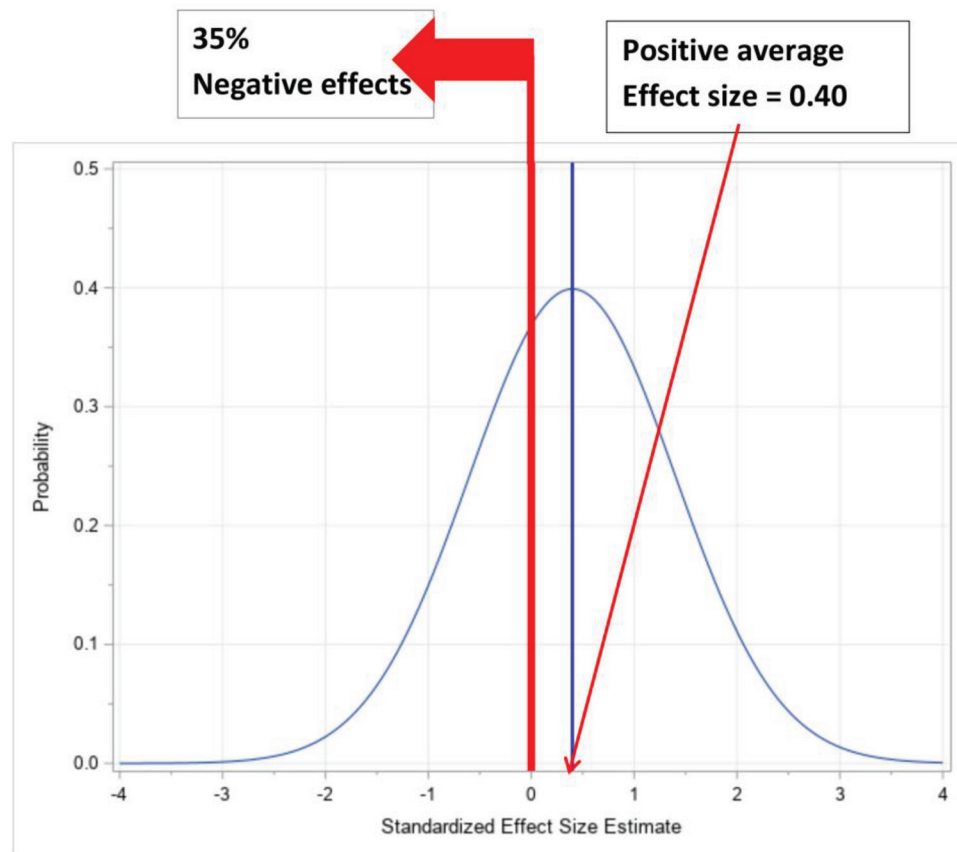
Some Null Effects Should Not Be Surprising

Like all effects, violent media effects have a sampling distribution. A portion of this distribution will be null and negative, even if the true effect is positive and nontrivial in size. For example, if the true standardized mean difference is $d = .40$, which corresponds to a correlation of .20 (i.e., the typical effect size for violent media studies), 35% of studies will produce negative results (see Figure 2). Based on this distribution of studies, one should not conclude there is no violent media effect in the population of studies (even though over one-third of studies produce negative or null results).

Generalizing Beyond Violent Media Effects

Although we have focused on violent media effects, many of the concepts we discuss are relevant to other types of media

Figure 2
Percent of Negative Results if the Average Effect Is $d = 0.40$ ($r = .20$).



Note. See the online article for the color version of this figure.

(e.g., prosocial media, sexually explicit media, advertisements, Public Service Announcements). For example, a recent article discussed the problem of overcontrol in pornography effects research (Wright, 2021).

Similarly, although we have focused on the effects of violent media on aggression, many of the concepts that we have discussed are relevant to the effects of other variables on aggression (e.g., weapons, frustration, provocation, alcohol, social exclusion, hot temperatures, pain, stress). For example, as noted earlier, the “weapons effect” is the finding that the mere presence of weapons can increase aggressive behavior (Berkowitz & LePage, 1967). When it comes to guns as weapons, they can be used for different purposes by different people. For example, rural youth and their families are more likely to acquire guns for recreational purposes such as hunting and target practice, whereas urban youth and their families mainly acquire guns for self-protection (Branas et al., 2004; Herrin et al., 2018). Previous research has shown that hunting rifles do not as strongly prime aggressive thoughts in hunters compared with nonhunters, although assault rifles prime aggressive thoughts in both hunters and nonhunters (Bartholow et al., 2005). Hunters know that hunting rifles are intended to shoot non-human targets, whereas nonhunters might not make this distinction. Likewise, exposure to photos of Olympic athletes holding guns does not prime aggressive thoughts in people (Bushman, 2018). People know that Olympians use guns to shoot inanimate targets rather than people. However, when people are exposed to photos of guns that are used to shoot people, it does not matter whether the person holding the gun is a “good guy” or a “bad guy” (Bushman, 2018).

These various methodological and statistical concepts also apply widely, well beyond the aggression domain. The next section illustrates one way that they do.

Null Findings and the So-Called Replication Crisis in Psychological Science

Many scientists believe there is a replication crisis in science (Baker, 2016). A large-scale collaborative project attempted to determine the extent of the replication crisis within the field of psychology (Open Science Collaboration, 2015). Researchers attempted to replicate 100 previously published psychological studies. The results found that the average effect in the replicated studies was about half the size ($r = .20$) of the average effect obtained in the original studies ($r = .40$), which was a considerable decline. Though media effects were not part of that study, null findings for violent media effects (or any other effects) likely contribute to the field’s perception of a replication crisis. Many “failed replications” in psychology may be due to the problems we note in Tables 1 and 2 and in Figure 1. Other scholars have found numerous flaws in the methods, statistics, and conclusions drawn in the original article (Bench et al., 2017; Gilbert et al., 2016, n.d.; Luttrell et al., 2017; Strack, 2016; see also Wilson, 2016). We hope that our tables and figure will help scholars conduct appropriate replication studies.

Conclusion

We have tackled an important problem in violent media research—why some studies produce null results. We believe that

this is a timely and important topic that will benefit science, policymakers, pediatricians, parents, and children. We hope this article will lead to an improvement in the quality of methods used in future violent media studies, an improvement in the quality of the statistical analyses of those studies, and an improvement in the use and development of theory in this domain and others. We also believe that these hard lessons in the media violence domain can lead to better research in all domains of psychology.

References

- Aloe, A. M. (2015). Inaccuracy of regression results in replacing bivariate correlations. *Research Synthesis Methods*, 6(1), 21–27. <https://doi.org/10.1002/jrsm.1126>
- Aloe, A. M., & Becker, B. J. (2012). An effect size for regression predictors in meta-analysis. *Journal of Educational and Behavioral Statistics*, 37(2), 278–297. <https://doi.org/10.3102/1076998610396901>
- Aloe, A. M., & Thompson, C. G. (2013). The synthesis of partial effect sizes. *Journal of the Society for Social Work and Research*, 4(4), 390–405. <https://doi.org/10.5243/jsswr.2013.24>
- Anderson, C. A., & Bushman, B. J. (2002a). Human aggression. *Annual Review of Psychology*, 53, 27–51. <https://doi.org/10.1146/annurev.psych.53.100901.135231>
- Anderson, C. A., & Bushman, B. J. (2002b). Media violence and societal violence. *Science*, 295(5564), 2377–2378. <https://doi.org/10.1126/science.1070765>
- Anderson, C. A., & Bushman, B. J. (2018). Media violence and the general aggression model. *Journal of Social Issues*, 74(2), 386–413. <https://doi.org/10.1111/josi.12275>
- Anderson, C. A., Gentile, D. A., & Buckley, K. E. (2007). *Violent video game effects on children and adolescents: Theory, research, and public policy*. Oxford University Press. <https://doi.org/10.1093/acprof:oso/9780195309836.001.0001>
- Anderson, C. A., Shibuya, A., Ihori, N., Swing, E. L., Bushman, B. J., Sakamoto, A., Rothstein, H. R., & Saleem, M. (2010). Violent video game effects on aggression, empathy, and prosocial behavior in eastern and western countries: A meta-analytic review. *Psychological Bulletin*, 136(2), 151–173. <https://doi.org/10.1037/a0018251>
- Anderson, C. A., Suzuki, K., Swing, E. L., Groves, C. L., Gentile, D. A., Prot, S., Lam, C. P., Sakamoto, A., Horiuchi, Y., Krahé, B., Jelic, M., Liuqing, W., Toma, R., Warburton, W. A., Zhang, X. M., Tajima, S., Qing, F., & Petrescu, P. (2017). Media violence and other aggression risk factors in seven nations. *Personality and Social Psychology Bulletin*, 43(7), 986–998. <https://doi.org/10.1177/0146167217703064>
- Baker, M. (2016, March 10). Statisticians issue warning over misuse of *P* values. *Nature*, 531(7593), Article 151. <https://doi.org/10.1038/nature.2016.19503>
- Bandura, A. (1973). *Aggression: A social learning theory analysis*. Prentice Hall.
- Bandura, A. (2001). Social cognitive theory of mass communication. *Media Psychology*, 3(3), 265–299. https://doi.org/10.1207/S1532785XMEP0303_03
- Bartholow, B. D., Anderson, C. A., Carnagey, N. L., & Benjamin, A. J., Jr. (2005). Individual differences in knowledge structures and priming: The weapons priming effect in hunters and nonhunters. *Journal of Experimental Social Psychology*, 41(1), 48–60. <https://doi.org/10.1016/j.jesp.2004.05.005>
- Bench, S. W., Rivera, G. N., Schlegel, R. J., Hicks, J. A., & Lench, H. C. (2017). Does expertise matter in replication? An examination of the reproducibility project: Psychology. *Journal of Experimental Social Psychology*, 68, 181–184. <https://doi.org/10.1016/j.jesp.2016.07.003>
- Bender, J., Rothmund, T., & Gollwitzer, M. (2013). Biased estimation of violent video game effects on aggression: Contributing factors and

- boundary conditions. *Societies*, 3(4), 383–398. <https://doi.org/10.3390/soc3040383>
- Benjamin, A. J., Jr., Kepes, S., & Bushman, B. J. (2018). Effects of weapons on aggressive thoughts, angry feelings, hostile appraisals, and aggressive behavior: A meta-analytic review of the weapons effect literature. *Personality and Social Psychology Review*, 22(4), 347–377. <https://doi.org/10.1177/1088868317725419>
- Berkowitz, L. (1990). On the formation and regulation of anger and aggression. A cognitive-neoassociationistic analysis. *The American Psychologist*, 45(4), 494–503. <https://doi.org/10.1037/0003-066X.45.4.494>
- Berkowitz, L., & LePage, A. (1967). Weapons as aggression-eliciting stimuli. *Journal of Personality and Social Psychology*, 7(2, Pt.1), 202–207. <https://doi.org/10.1037/h0025008>
- Brady, S. S., & Matthews, K. A. (2006). Effects of media violence on health-related outcomes among young men. *Archives of Pediatrics and Adolescent Medicine*, 160(4), 341–347. <https://doi.org/10.1001/archpedi.160.4.341>
- Branas, C. C., Nance, M. L., Elliott, M. R., Richmond, T. S., & Schwab, C. W. (2004). Urban-rural shifts in intentional firearm death: Different causes, same results. *American Journal of Public Health*, 94(10), 1750–1755. <https://doi.org/10.2105/ajph.94.10.1750>
- Burnay, J., Bushman, B. J., & Larøi, F. (2019). Effects of sexualized video games on online sexual harassment. *Aggressive Behavior*, 45(2), 214–223. <https://doi.org/10.1002/ab.21811>
- Bushman, B. J. (1995). Moderating role of trait aggressiveness in the effects of violent media on aggression. *Journal of Personality and Social Psychology*, 69(5), 950–960. <https://doi.org/10.1037/0022-3514.69.5.950>
- Bushman, B. J. (2018). Guns automatically prime aggressive thoughts, regardless of whether a ‘good guy’ or ‘bad guy’ holds the gun. *Social Psychological and Personality Science*, 9(6), 727–733. <https://doi.org/10.1177/1948550617722202>
- Bushman, B. J., Gollwitzer, M., & Cruz, C. (2015a). Agreement across stakeholders is consensus: Response to Ivory et al. (2015). *Psychology of Popular Media Culture*, 4(3), 230–235. <https://doi.org/10.1037/ppm0000061>
- Bushman, B. J., Gollwitzer, M., & Cruz, C. (2015b). There is broad consensus: Media researchers agree that violent media increase aggression in children, and pediatricians and parents concur. *Psychology of Popular Media Culture*, 4(3), 200–214. <https://doi.org/10.1037/ppm0000046>
- Bushman, B. J., Jamieson, P. E., Weitz, I., & Romer, D. (2013). Gun violence trends in movies. *Pediatrics*, 132(6), 1014–1018. <https://doi.org/10.1542/peds.2013-1600>
- Bushman, B. J., & Huesmann, L. R. (2006). Short-term and long-term effects of violent media on aggression in children and adults. *Archives of Pediatrics and Adolescent Medicine*, 160(4), 348–352. <https://doi.org/10.1001/archpedi.160.4.348>
- Bushman, B. J., & Huesmann, L. R. (2012). Effects of violent media on aggression. In D. G. Singer & J. L. Singer (Eds.), *Handbook of children and the media* (pp. 231–248). Sage, Inc.
- Buss, A. H., & Perry, M. (1992). The aggression questionnaire. *Journal of Personality and Social Psychology*, 63(3), 452–459. <https://doi.org/10.1037/0022-3514.63.3.452>
- Cohen, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). Erlbaum.
- Cohen, J., Cohen, P., West, S. G., & Aiken, L. S. (2003). *Applied multiple regression/correlation analysis for the behavioral sciences* (3rd ed.). Erlbaum Publishers.
- DeCamp, W., & Ferguson, C. J. (2017). The impact of degree of exposure to violent video games, family background, and other factors on youth violence. *Journal of Youth and Adolescence*, 46(2), 388–400. <https://doi.org/10.1007/s10964-016-0561-8>
- DeCoster, J., Iselin, A.-M. R., & Gallucci, M. (2009). A conceptual and empirical examination of justifications for dichotomization. *Psychological Methods*, 14(4), 349–366. <https://doi.org/10.1037/a0016956>
- Dowsett, A., & Jackson, M. (2019). The effect of violence and competition within video games on aggression. *Computers in Human Behavior*, 99, 22–27. <https://doi.org/10.1016/j.chb.2019.05.002>
- Elson, M., Breuer, J., Van Looy, J., Kneer, J., & Quandt, T. (2015). Comparing apples and oranges? Evidence for pace of action as a confound in research on digital games and aggression. *Psychology of Popular Media Culture*, 4(2), 112–125. <https://doi.org/10.1037/ppm0000010>
- Ferguson, C. J. (2011). Video games and youth violence: A prospective analysis in adolescents. *Journal of Youth and Adolescence*, 40(4), 377–391. <https://doi.org/10.1007/s10964-010-9610-x>
- Ferguson, C. J. (2015). Do angry birds make for angry children? A meta-analysis of video game influences on children’s and adolescents’ aggression, mental health, prosocial behavior, and academic performance. *Perspectives on Psychological Science*, 10(5), 646–666. <https://doi.org/10.1177/1745691615592234>
- Ferguson, C. J., Olson, C. K., Kutner, L. A., & Warner, D. E. (2014). Violent video games, catharsis seeking, bullying, and delinquency: A multivariate analysis of effects. *Crime and Delinquency*, 60(5), 764–784. <https://doi.org/10.1177/0011128710362201>
- Ferguson, C. J., Rueda, S. M., Cruz, A. M., Ferguson, D. E., Fritz, S., & Smith, S. M. (2008). Violent video games and aggression: Causal relationship or byproduct of family violence and intrinsic violence motivation. *Criminal Justice and Behavior*, 35(3), 311–332. <https://doi.org/10.1177/0093854807311719>
- Finkel, E. J., & Hall, A. N. (2018). The I³ Model: A metatheoretical framework for understanding aggression. *Current Opinion in Psychology*, 19, 125–130. <https://doi.org/10.1016/j.copsyc.2017.03.013>
- Gaudry, E., Vagg, P., & Spielberger, C. D. (1975). Validation of the state-trait distinction in anxiety research. *Multivariate Behavioral Research*, 10(3), 331–341. https://doi.org/10.1207/s15327906mbr1003_6
- Gentile, D. G., & Bushman, B. J. (2012). Reassessing media violence effects using a risk and resilience approach to understanding aggression. *Psychology of Popular Media Culture*, 1(3), 138–151. <https://doi.org/10.1037/a0028481>
- Gilbert, D. T., King, G., Pettigrew, S., & Wilson, T. D. (2016). Comment on “Estimating the reproducibility of psychological science.” *Science*, 351(6277), Article 1037. <https://doi.org/10.1126/science.aad7243>
- Gilbert, D. T., King, G., Pettigrew, S., & Wilson, T. D. (n.d). Response to the reply to our technical comment on “Estimating the reproducibility of psychological science.” https://gking.harvard.edu/files/gking/files/gkpw_response_to_osc_rebutal.pdf
- Goodman, R. (1997). The strengths and difficulties questionnaire: A research note. *Journal of Child Psychology and Psychiatry, and Allied Disciplines*, 38(5), 581–586. <https://doi.org/10.1111/j.1469-7610.1997.tb01545.x>
- Götz, F. M., Gosling, S. D., & Rentfrow, P. J. (2021). Small effects: The indispensable foundation for a cumulative psychological science. *Perspectives on Psychological Science*. Advance online publication. <https://doi.org/10.1177/1745691620984483>
- Greitemeyer, T., & Mügge, D. O. (2014). Video games do affect social outcomes: A meta-analytic review of the effects of violent and prosocial video game play. *Personality and Social Psychology Bulletin*, 41(8), 1164–1589. <https://doi.org/10.1177/0146167213520459>
- Hartmann, T., Krakowiak, K. M., & Tsay-Vogel, M. (2014). How violent video games communicate violence: A literature review and content analysis of moral disengagement factors. *Communication Monographs*, 81(3), 310–332. <https://doi.org/10.1080/03637751.2014.922206>
- Herrin, B. R., Gaither, J. R., Leventhal, J. M., & Dodington, J. (2018). Rural versus urban hospitalizations for firearm mortality rates in children and adolescents. *Pediatrics*, 142(2), Article e20173318. <https://doi.org/10.1542/peds.2017-3318>

- Hetsroni, A. (2007). Four decades of violent content on prime-time network programming: A longitudinal meta-analytic review. *Journal of Communication*, 57(4), 759–784. <https://doi.org/10.1111/j.1460-2466.2007.00367.x>
- Huesmann, L. R. (1986). Psychological processes promoting the relation between exposure to media violence and aggressive behavior by the viewer. *Journal of Social Issues*, 42(3), 125–139. <https://doi.org/10.1111/j.1540-4560.1986.tb00246.x>
- Ivory, J. D., & Kalyanaraman, S. (2007). The effects of technological advancement and violent content in video games on players' feelings of presence, involvement, physiological arousal, and aggression. *Journal of Communication*, 57(3), 532–555. <https://doi.org/10.1111/j.1460-2466.2007.00356.x>
- Jerabeck, J. M., & Ferguson, C. J. (2013). The influence of solitary and co-operative violent video game play on aggressive and prosocial behavior. *Computers in Human Behavior*, 29(6), 2573–2578. <https://doi.org/10.1016/j.chb.2013.06.034>
- Jones, T., Cunningham, P. H., & Gallagher, K. (2014). Violence in advertising: A multilayered content analysis. In N. J. Rifon, M. B. Roynce, L. Carlson, N. J. Rifon, M. B. Roynce, & L. Carlson (Eds.), *Advertising and violence: Concepts and perspectives* (pp. 255–289). M. E. Sharpe, Inc.
- Krongard, S., & Tsay-Vogel, M. (2020). Online original TV series: Examining portrayals of violence in popular binge-watched programs and social reality perceptions. *Psychology of Popular Media*, 9(2), 155–164. <https://doi.org/10.1037/ppm0000224>
- Lipsey, M. W., & Wilson, D. B. (1993). The efficacy of psychological, educational, and behavioral treatment. Confirmation from meta-analysis. *The American Psychologist*, 48(12), 1181–1209. <https://doi.org/10.1037/0003-066X.48.12.1181>
- Luttrell, A., Petty, R. E., & Xu, M. (2017). Replicating and fixing failed replications: The case of need for cognition and argument quality. *Journal of Experimental Social Psychology*, 69, 178–183. <https://doi.org/10.1016/j.jesp.2016.09.006>
- MacCallum, R. C., Zhang, S., Preacher, K. J., & Rucker, D. D. (2002). On the practice of dichotomization of quantitative variables. *Psychological Methods*, 7(1), 19–40. <https://doi.org/10.1037/1082-989X.7.1.19>
- Martin, B. S., & Collins, B. A. (2002). Violence and consumption imagery in music videos. *European Journal of Marketing*, 36(7/8), 855–873. <https://doi.org/10.1108/03090560210430845>
- Mathur, M. B., & VanderWeele, T. J. (2019). Finding common ground in meta-analysis “wars” on violent video games. *Perspectives on Psychological Science*, 14(4), 705–708. <https://doi.org/10.1177/1745691619850104>
- Moos, R., & Moos, B. (2002). *Family environment scale manual*. Mindgarden.
- Open Science Collaboration. (2015). PSYCHOLOGY. Estimating the reproducibility of psychological science. *Science*, 349(6251), Article aac4716. <https://doi.org/10.1126/science.aac4716>
- Orne, M. T. (1962). On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications. *American Psychologist*, 17(11), 776–783. <https://doi.org/10.1037/h0043424>
- Paik, H., & Comstock, G. (1994). The effects of television violence on antisocial behavior: A meta-analysis. *Communication Research*, 12(11), 516–546. <https://doi.org/10.1177/009365094021004004>
- Peterson, R. A., & Brown, S. P. (2005). On the use of beta coefficients in meta-analysis. *The Journal of Applied Psychology*, 90(1), 175–181. <https://doi.org/10.1037/0021-9010.90.1.175>
- Promising Practices Network. (2014). How programs are considered. Retrieved November 12, 2019, from <http://www.promisingpractices.net/criteria.asp>
- Przybylski, A. K., & Weinstein, N. (2019). Violent video game engagement is not associated with adolescents' aggressive behaviour: Evidence from a registered report. *Royal Society Open Science*, 6(2), Article 171474. <https://doi.org/10.1098/rsos.171474>
- Richard, F. D., Bond, C. F., Jr., & Stokes-Zoota, J. J. (2003). One hundred years of social psychology quantitatively described. *Review of General Psychology*, 7(4), 331–363. <https://doi.org/10.1037/1089-2680.7.4.331>
- Romer, D., Jamieson, P. E., & Jamieson, K. H. (2017). The continuing rise of gun violence in PG-13 movies, 1985–2015. *Pediatrics*, 139(2), Article e20162891. <https://doi.org/10.1542/peds.2016-2891>
- Roth, P. L., Le, H., Oh, I.-S., Van Iddekinge, C. H., & Bobko, P. (2018). Using beta coefficients to impute missing correlations in meta-analysis research: Reasons for caution. *Journal of Applied Psychology*, 103(6), 644–658. [Supplemental]. <https://doi.org/10.1037/apl0000293>
- Rothstein, H. R., & Bushman, B. J. (2015). Methodological and reporting errors in meta-analytic reviews make other meta-analysts angry: A commentary on Ferguson (2015). *Perspectives on Psychological Science*, 10(5), 677–679. <https://doi.org/10.1177/1745691615592235>
- Strack, F. (2016). Reflection on the smiling registered replication report. *Perspectives on Psychological Science*, 11(6), 929–930. <https://doi.org/10.1177/1745691616674460>
- Straus, M. A. (1979). Measuring intrafamily conflict and violence: The Conflict Tactics (CT) Scales. *Journal of Marriage and Family*, 41(1), 75–88. <https://doi.org/10.2307/351733>
- Teng, S. K. Z., Chong, G. Y. M., Siew, A. S. C., & Skoric, M. M. (2011). Grand Theft Auto IV comes to Singapore: Effects of repeated exposure to violent video games on aggression. *Cyberpsychology, Behavior, and Social Networking*, 14(10), 597–602. <https://doi.org/10.1089/cyber.2010.0115>
- Thompson, K. M., & Haninger, K. (2001). Violence in E-rated video games. *Journal of the American Medical Association*, 286(5), 591–598. <https://doi.org/10.1001/jama.286.5.591>
- Thompson, S. G., & Higgins, J. P. (2002). How should meta-regression analyses be undertaken and interpreted? *Statistics in Medicine*, 21(11), 1559–1573. <https://doi.org/10.1002/sim.1187>
- Tipton, E., Pustejovsky, J. E., & Ahmadi, H. (2019). A history of meta-regression: Technical, conceptual, and practical developments between 1974 and 2018. *Research Synthesis Methods*, 10(2), 161–179. <https://doi.org/10.1002/jrsm.1338>
- Unsworth, G., Devilly, G. J., & Ward, T. (2007). The effect of playing violent video games on adolescents: Should parents be quaking in their boots? *Psychology, Crime and Law*, 13(4), 383–394. <https://doi.org/10.1080/10683160601060655>
- Williams, D., & Skoric, M. (2005). Internet fantasy violence: A test of aggression in an online game. *Communication Monographs*, 72(2), 217–233. <https://doi.org/10.1080/03637750500111781>
- Wilson, A. E. (2016). Exact replications in an inexact context: Commentary on Ebersole et al. (2016). *Journal of Experimental Social Psychology*, 67, 84–85. <https://doi.org/10.1016/j.jesp.2015.12.008>
- Wright, P. J. (2021). Overcontrol in pornography research: Let it go, let it go. . . *Archives of Sexual Behavior*, 50(2), 387–392. <https://doi.org/10.1007/s10508-020-01902-9>
- Wyer, R. S. Jr. (2012). A theory of social information processing. In P. A. M. Van Lange, A. W. Kruglanski, & E. T. Higgins (Eds.), *Handbook of theories of social psychology* (Vol. 1, pp. 156–177). Sage Ltd. <https://doi.org/10.4135/9781446249215.n9>
- Yokota, F., & Thompson, K. M. (2000). Violence in G-rated animated films. *Journal of the American Medical Association*, 283(20), 2716–2720. <https://doi.org/10.1001/jama.283.20.2716>
- Zillmann, D. (1979). *Hostility and aggression*. Erlbaum.

Received January 19, 2021

Revision received July 7, 2021

Accepted July 9, 2021 ■