aps ASSOCIATION FOR PSYCHOLOGICAL SCIENCE

Perspectives on Psychological Science
2015, Vol. 10(5) 646–666

DOI: 10.1177/1745691615592234
pps.sagepub.com
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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

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Abstract

The issue of whether video games—violent or nonviolent—"harm" children and adolescents continues to be hotly contested in the scientific community, among politicians, and in the general public. To date, researchers have focused on college student samples in most studies on video games, often with poorly standardized outcome measures. To answer questions about harm to minors, these studies are arguably not very illuminating. In the current analysis, I sought to address this gap by focusing on studies of video game influences on child and adolescent samples. The effects of overall video game use and exposure to violent video games specifically were considered, although this was not an analysis of pathological game use. Overall, results from 101 studies suggest that video game influences on increased aggression (r = .06), reduced prosocial behavior (r = .04), reduced academic performance (r = −.01), depressive symptoms (r = .04), and attention deficit symptoms (r = .03) are minimal. Issues related to researchers' degrees of freedom and citation bias also continue to be common problems for the field. Publication bias remains a problem for studies of aggression. Recommendations are given on how research may be improved and how the psychological community should address video games from a public health perspective.

Keywords

video games, aggression, mental health, academics, prosocial behavior

The degree to which video games, including those with violent content, have a deleterious influence on children's and adolescents' mental well-being remains an issue that is hotly debated both in the general public and scientific community. In 2011, the U.S. Supreme Court in the *Brown v. EMA* decision struck down a California law seeking to regulate the sale of violent video games to minors. In the majority decision, the justices were also critical of the psychological research, concluding that it was incapable of supporting causal links to "harm" in minors including, but not limited to, aggressive behavior. However, several justices in minority opinions found the research more credible.

The tragic Sandy Hook elementary school shooting in late 2012, in which 20-year-old Adam Lanza killed his mother, 20 elementary school children, and 6 adult school employees in Newtown, Connecticut, reawakened public and scholarly community concerns over video game violence. Rhetoric on videogames as a potential cause of the shooting surfaced given reports suggesting that Lanza may have played violent games at least occasionally, although the final investigative report suggested that he was more a fan of nonviolent games.[1] This concern over games arose, despite that being a gamer would not have differentiated Lanza from the majority of young men his age who also game (Griffiths & Hunt, 1995; Lenhart et al., 2008; Olson et al., 2007). Furthermore, mass homicide perpetrators are not unusually likely to be gamers (Ferguson, Coulson, & Barnett, 2011; Fox & DeLateur, 2014; U.S. Secret Service and U.S. Department of Education, 2002). Following the Sandy Hook shooting,

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the National Rifle Association attempted to shift blame for the shooting from gun control to video games (Kain, 2012), and Senator Jay Rockefeller called for a "study" of video game violence, although his objectivity could be questioned given that he stated the desired result of the study in advance (Boleik, 2012). Although there is concern that this political pressure on the scientific community might result in damage to the integrity of the scientific process (see Ferguson, 2013), questions about the effect of video games on children—whether related to violent crime or to other concerns regarding mental health, academics, and prosocial behavior—are likely to continue into the foreseeable future.

Such questions are not unreasonable and are certainly well within the purview of science. However, to date, researchers in psychological science have had difficulty answering these questions. In 2005, the American Psychological Association (APA) released a policy statement implicating links between violent video game use and subsequent player aggression. By 2010, the APA appeared to have qualified that position, however, having declined to participate in the U.S. Supreme Court case *Brown v. EMA*, citing inconsistencies in the literature (Azar, 2010).[2] Other professional groups, such as the American Academy of Pediatrics (AAP), have been, if anything, more vocal in supporting links between video games and even societal violence (AAP, 2009). By contrast, government reviews of the field have been less sanguine about the ability of video game research to conclusively answer societal questions about links with violence or mental health outcomes. The 2001 Surgeon General's report on youth violence relegated media violence, in general, to a very minor role and noted inconsistencies and methodological flaws in the literature (U.S. Department of Health and Human Services, 2001). More recent reviews of the video game research field by the Australian Government, Attorney General's Department (2010); the Swedish Media Council (2011); the media watchdog group Common Sense Media (2013); and the U.S. House of Representatives Gun Violence Prevention Task Force (2013) similarly concluded that the research is inconsistent and methodologically flawed.

Arguably, it may be that so much of the video game field simply has not adequately addressed the constructs and populations of interest to the general public. Policy makers, the general public, and scholarly organizations want to know whether exposure to video games, particularly in childhood, can play a causal role (perhaps with other variables) in the development of societally relevant aggression up to and including violent crime, or whether exposure to games might lead to other mental health problems. By contrast, most researchers have conducted recent studies with college students, using proxy measures of minor aggression that do not predict socially relevant aggression or violence. Past meta-analyses of video games (e.g., Anderson et al., 2010; Ferguson, 2007; Sherry, 2001) have generally relied heavily on studies involving college students along with those involving children, and conclusions of these meta-analyses may not generalize well to the societally relevant issues at hand. In these studies, researchers have focused primarily on aggression as an outcome and have not considered either violent outcomes or other mental health issues. Thus, in the current meta-analysis, I seek to expand on previous work by considering studies of video game effects on children specifically, with outcomes related not only to aggression and violence but also to mental health, prosocial behavior, and academic performance. The issues addressed in this article pertain to exposure to video games, whether in general or to violent video games specifically. The issue of pathological gaming, wherein individuals play video games to the point that they neglect other life responsibilities, has been addressed in other work (e.g., Griffiths, Kuss, & King, 2012).

Video Games and Children's Mental Health: A State of the Research

Perhaps given the emotional impact of mass homicides on the national consciousness of which the scientific community has been a part, most researchers have focused on violent content in video games. Despite more than 100 studies, the scholarly community remains divided over whether evidence for causal links with player aggression has been established (as an example of scholarly debate in this field, see the following sequence: Hall, Day, & Hall, 2011a; Murray et al., 2011; Hall, Day, & Hall, 2011b). This body of evidence includes numerous experimental, correlational, and longitudinal studies. The ability of these studies to answer societal questions about links with clinically or practically significant aggression (i.e., aggression that would be harmful to oneself or others—a threshold not often reached by aggression measures used in research) or violent behavior has been limited because of disagreements in findings among these studies as well as several well-known and systematic methodological limitations. These limitations have been discussed at length elsewhere (Adachi & Willoughby, 2010; Ferguson, 2010; Kutner & Olson, 2008; Savage, 2004; however, for a different view, see Strasburger, Jordan, & Donnerstein, 2010), although I reiterate them briefly here.

General problems in studying the effects of video games

Mismatched games in experimental studies. The ability to ascribe any difference in experimental outcomes to violent content depends on games being

matched carefully on other variables—such as competitiveness, difficulty, and pace of action—which has typically not been done (Adachi & Willoughby, 2010). Several studies have suggested that carefully matching video games on competitiveness (Adachi & Willoughby, 2011), difficulty of controls (Przybylski, Rigby, & Ryan, 2010), or frustration (Przybylski, Deci, Rigby, & Ryan, 2014) eliminates differences between violent and nonviolent games.

Failure to pretest. In most experiments on video games, researchers randomly assign participants to play a nonviolent or violent game, and then they do a posttest on the outcome variable. Differences in aggression noted are presumed to relate to an increase in aggression in the violent game condition. However, it is plausible that any differences may, instead, be a differential reduction in aggression. For instance, in a recent experiment involving pretests and posttests, Valadez and Ferguson (2012) found that all video games reduced hostility over time. In addition, some games with prosocial content can reduce aggression below baseline (Sestir & Bartholow, 2010). Using Solomon four-group designs,[3] researchers could test changes over time while also adjusting for potential demand characteristics of pretest designs.

Unstandardized aggression measures. A major issue with many aggression measures used in this field is that they are unstandardized, potentially allowing researchers to pick and choose from among outcomes from within a single measure those that best fit their a priori hypotheses (Elson, Mohseni, Breuer, Scharkow, & Quandt, 2014; Ferguson, 2013). Although this lack of standardization may be reframed as attempting to test different aspects of aggression, such explanations, although undoubtedly in good faith, may ultimately be self-serving, particularly in a research environment with considerable pressure to produce "statistically significant" findings at the expense of null findings (LeBel & Peters, 2011; Pashler & Harris, 2012; Simmons, Nelson, & Simonsohn, 2011).

Lack of clinical validity. As evidenced by APA's (2005) and even more by AAP's (2009) policy statements, research on video games is often generalized to public health issues or violent behavior. Soon after the Sandy Hook shooting, some scholars implied that exposure to violent media was one mechanism by which mass homicide perpetrators might learn the "scripts" necessary to commit their crimes (e.g., Huesmann & Dubow, 2012; KCCI, 2012). From these claims about video games influencing societal violence up through and including mass shootings, it can be seen that scholars are not limiting their discussions of research to esoteric laboratory aggression measures but rather are generalizing them to societal violence and even mass homicides. However, it has been well-understood for some time that many of the aggression measures used in this research, even ignoring the standardization issue, are not easily generalized to real-life aggression, let alone to violent crime (Ferguson & Rueda, 2009; Ritter & Eslea, 2005; Savage, 2004; Tedeschi & Quigley, 2000). For example, recent evidence has indicated that the unstandardized use of the popular "noise blast" Taylor Competitive Reaction Time Test[4] often used in laboratory video game studies has significant potential to influence effect sizes (Elson et al., 2014). That is to say, observed effect sizes may be highly influenced by scholars' good-faith a priori assumptions about video game effects. Even in correlational or longitudinal studies, well-validated measures—such as the Child Behavior Checklist (Achenbach & Rescorla, 2001)—are often eschewed for measures with lesser known properties and lacking clinical cutoffs. Yet, even minor fluctuations on these measures are often generalized to clinically relevant or public health outcomes.

Failure to control for third variables. When considering the influence of video games on clinically relevant or criminological outcomes, it is best practice to carefully control for any potentially confounding variables in correlational or longitudinal designs (Savage, 2004). As a simple example, boys play more violent video games (Olson et al., 2007) and are also more aggressive than girls. Thus, one is likely to see bivariate correlations between video game violence use and aggression that are simple gender effects (see, e.g., Przybylski & Mishkin, in press). Controlling for gender as well as other theoretically critical factors—such as trait aggression, family violence, peer delinquency, and mental health—is essential. In longitudinal designs, controlling for Time 1 outcome is, likewise, essential. For example, in Anderson et al.'s (2010) meta-analysis, longitudinal relations between video game violence and later aggression dropped from $r = .20$ to $r = .08$ with sex and Time 1 aggression as the only control variables. To the extent that discussions of video game effects rely on bivariate correlations, these discussions may be misleading. By now it is clear that effect sizes are substantially reduced when control variables including gender, trait aggression, and family environment are included in analyses. This observation should be an important part of future discussions.

Selective interpretation. In some cases, study authors may achieve either inconsistent or even null results and overcommunicate these as being in favor of their a priori hypotheses. Given methodological flexibility/researchers' degrees of freedom issues (Simmons et al., 2011), the degree to which null results are converted to statistically significant results may simply be unknown to the field.

However, in some cases, authors may dutifully report all of their results yet choose to highlight only those that fit their a priori hypotheses. Ignoring multivariate controlled results in favor of bivariate results is one such example. In one recent study, the authors found a statistically significant bivariate relationship between video game violence use and youth aggression (Ybarra et al., 2008). Yet, when control variables were applied, the relationship became no longer statistically significant. In discussing their results, the authors essentially ignored their better controlled (and hypothesis disconfirmatory) results in favor of the less rigorous bivariate results. Across studies, selective interpretation of data can result in the perception that study results in a field have been far more consistent than they actually have been.

Citation/selective reporting bias. Citation or selective reporting bias occurs when scholars only cite and report other studies in literature reviews that support their personal hypotheses. Disconfirmatory evidence or failed replications are not reported to the research community or general public. As with selective interpretation, this practice can result in a distorted perception of a research field and is considered a questionable researcher practice (QRP; see Babor & McGovern, 2008). Coupled with the issue of methodological flexibility, it is also possible that authors who use citation bias may also be more prone to using flexible statistical methods (even doing so unconsciously and in good faith) to reach a desired outcome. Citation/selective reporting bias has been found to be widespread in video game research (Ferguson, 2010), including in the APA's (2005) and AAP's (2009) policy statements.

Summary of limitations. It is important to note that the weaknesses described earlier are not particular to one or two studies but are systemic throughout the field (Adachi & Willoughby, 2010; Ferguson, 2010; Kutner & Olson, 2008; Savage, 2004). Some carefully designed studies certainly do exist. For example, several well-designed longitudinal studies of youths have recently been published, both showing evidence for very small effects on aggression (e.g., Willoughby, Adachi, & Good, 2012) and not showing evidence of aggression effects (e.g., Ferguson, 2011b; von Salisch, Vogelgesang, Kristen, & Oppl, 2011). Yet, such well-designed studies are in the minority.

Outcomes besides violence: Video games, mental health, and academics

In the earlier discussion, video game violence is the focus, which, arguably, is the broadest, most discussed, and perhaps most controversial outcome. However, the earlier limitations may also relate to studies in which the influence of video game exposure on other outcomes related to mental health, prosocial behavior, and academics is examined. It is also important in this section to differentiate research on exposure to video games from that on *pathological gaming* (persisting in gaming behaviors despite obvious negative consequences, such as missing school or work), which relates more specifically to gaming behaviors that may be correlated or associated with negative functioning (Kuss & Griffiths, 2012; van Rooij, Schoenmakers, Vermulst, van den Eijnden, & van de Mheen, 2011). Indeed, some scholars have argued that how children play video games is as important as or more important than the content of the games they play (Colwell, 2007).

Outcomes related to mental health—including aggression, prosocial behavior, depression, and attention-deficit/hyperactivity disorder—as well as to academics are likely comorbid. Issues related to aggression tend to occur alongside depression (Ferguson, 2011b), attention problems (Connor & Ford, 2012), and school problems (Risser, 2013). Thus, the specific problems addressed in this article can be conceived as a constellation of potentially related problems that may or may not arise from video game use. Considering many of these issues together in tandem has been consistent in the research for some time (e.g., Anderson & Dill, 2000; Desai, Krishnan-Sarin, Cavallo, & Potenza, 2010), and it is valuable to consider them in tandem in meta-analyses.

Certainly not all research on video games begins with the notion that such games are harmful to mental health or cognition. For instance, video game use has been found to stimulate children's creativity (Jackson et al., 2012), and there is a wide body of research in which investigators consider the beneficial effects of video games, including violent action games, on civic behavior (Granic, Lobel, & Engels, 2014) and visuospatial cognition (Spence & Feng, 2010; however, for a discussion of the limitations of this research, see also Boot, Blakely, & Simons, 2011). Some research suggests that video game influences vary depending on specific outcomes assessed (Jackson, von Eye, Witt, Zhao, & Fitzgerald, 2011) or that video games and personality style interact to produce positive academic outcomes (Ventura, Shute, & Kim, 2012). However, the focus of this analysis is on research in which possible negative influences are examined.

The pools of research, particularly with children, on mental health issues or academic performance tend to be smaller than for aggression but with equally variable results. For example, in one recent study, Swing, Gentile, Anderson, and Walsh (2010) concluded that general video game playing was related to attention deficit symptoms, although, by contrast, Ferguson (2011a) found no evidence for such a relationship. In another study, Desai

et al. (2010) found highly variable results for video game influences on children's mental well-being. They found that video game playing reduced depression in girls, but not boys, and that problematic outcomes were related to pathological gaming behaviors but not to general exposure. Other studies have suggested curvilinear relationships between video gaming and mental health (e.g., Allahverdipour, Bazargan, Farhadinasab, & Moeini, 2010; Kutner & Olson, 2008; Przybylski, 2014). In each case, the greatest levels of mental illness symptoms were among children who played no video games at all.

Epidemiological data

One other pool of data that is worth considering is epidemiological data. During the past few decades in which video game use became far more prevalent among children, societal behavior data on youths indicated either improvements or no change. According to both U.S. (Childstats.gov, 2015) and international (van Dijk, van Kesteren, & Smit, 2007) data, societal violence—including youth violence—declined to 40-year lows. According to the National Center for Educational Statistics (2009), standardized testing of academic performance either improved or held steady. According to statistics from the Centers for Disease Control and Prevention (2013), suicidal ideation and suicide attempts, despite some yearly fluctuations, have either declined or held steady over the past 2 decades. Civic and volunteering behaviors among youths have risen rather than declined (Girl Scout Research Institute, 2009).

None of this data should be interpreted as indicating that video games caused these improvements in youth health. Epidemiological data also do not rule out the potential for small, subtle effects of video games. For instance, video games may have small effects on certain groups of players, despite having little impact on the majority (Markey & Markey, 2010), although recent studies (e.g., Engelhardt, Mazurek, Hilgard, Rouder, & Bartholow, in press) have generally not borne this hypothesis out. Video games may also have differential effects on individual children. In one study, Unsworth, Devilly, and Ward (2007) found that violent games had little impact on the majority of children but increased anger in some children and decreased anger in others. However, the epidemiological data are potentially valuable simply in noting that the hyperbole that often surrounds video game research is at odds not only with the inconsistent nature of the research data but also with the epidemiological data evidencing a lack of broad-based dramatic effects (Olson, 2004). Such epidemiological data should not be ignored as inconvenient, particular when scholars have made dramatic claims about potential video game influences on exactly those societal outcomes. The epidemiological data do demonstrate that, at very least, the widespread use of video games among children has not resulted in a noticeable decline in functioning among children as a whole.

The current study

Questions remain in the general public and scholarly community regarding the impact of video games on children's and adolescents' mental well-being. Therefore, in this meta-analysis, I attempt to answer many questions by addressing three types of problems with the existing research.

First, at present, no researcher has specifically examined studies of video game influence on children and adolescents in a meta-analysis. The research field has relied heavily on college students rather than on child and adolescent participants. In one recent meta-analysis, Anderson et al. (2010) did not specifically look at any subsample of studies of children, although they did include age as a moderator in some analyses. By contrast, in an older meta-analysis, Sherry (2001) found that effect sizes were smaller for younger samples than for larger samples. Consistent with the 2001 meta-analysis, in a third meta-analysis, Ferguson (2007) also found that effect sizes for child samples were smaller than for college-age samples. Thus, it is possible that in meta-analyses in which college samples are heavily depended on, researchers may unintentionally overestimate the effects of video games on children and adolescents. One possible reason for the more pronounced effects seen among college students is that college students may be particularly prone to producing behaviors that they believe the experimenter wants rather than ecologically valid responses.

Second, in most previous analyses, researchers have examined issues related to aggression and prosocial behavior but not to other mental-health-related outcomes or academic performance. In this meta-analysis, I examine five outcomes: aggression, prosocial behavior, academic performance, depression, and attention problems.

Third, given that research on video games is inconsistent, meta-analyses can be valuable in providing methodological reasons for why these inconsistencies may exist. For example, Ferguson (2007) has noted that studies in which standardized aggression measures are used tend to produce lower effect sizes than those studies in which unstandardized aggression measures are used. Because methodological flexibility/researchers' degrees of freedom (Simmons et al., 2011) can influence outcomes, selective reporting bias in articles may also provide potential evidence for unintentional researcher biases that can, even acting in good faith, result in overestimations of video game effects. Thus, using meta-analysis, researchers can examine for systematic issues in a field that may result in over- or underestimation of negative effects.

Selection of Studies

Identification of relevant studies involved a search of the PsycINFO, Criminal Justice Abstracts, Science Direct, Medline, Dissertation Abstracts, and Digital Dissertations databases with the search term "(video game*) OR (Computer game*) OR (digital game*)" and "child* OR adol* OR youth OR juvenile*" and "agress* OR viol* OR (mental health) OR (attention) OR depress* OR school OR grades OR prosocial." In addition, recent reviews of the video game and mental health literature were examined for articles that may have been missed in the literature search. Unpublished studies were sought by posting requests to listservs (e.g., those related to media psychology [Division 46 of the APA], the mass communication division of the National Communication Association) as well as by e-mailing requests to prominent scholars on both sides of the debate. Included studies had to meet the following criteria:

1. In each study, the author(s) had to measure the influence of video games, whether violent or nonviolent, on at least one of the outcomes related to mental or behavioral health (aggression,[5] prosocial behavior, depressive symptoms, attention problems, academic performance).
2. In each study, the author(s) had to present statistical outcomes or data that could be meaningfully converted into effect size "*r*."
3. In experimental studies, the author(s) had to contrast violent video game play with nonviolent video game play. Studies in which researchers did not include a nonviolent video game control condition were not included; in addition, studies in which researchers primarily examined media literacy interventions or contrasting playing versus watching video games were not included. Although such studies may address important questions, they were not central to the research questions of this meta-analysis.
4. A given sample was included only once in the meta-analyses to maintain independence. Some samples, including longitudinal studies, may produce multiple publications, but only one such study was included in the current analysis.[6]

The initial search (carried out in February 2014) returned approximately 750 hits, the majority of which were either nonempirical, were with college student samples, or otherwise did not meet the inclusion criteria described earlier. Using the inclusion criteria, I netted 101 studies in the final search, of which nine were doctoral dissertations, four were unpublished but "in press," and five were unpublished data. The 101 studies in the current analysis provided 122 separate controlled effect size estimates and 136 separate bivariate effect sizes. As these involved different outcomes analyzed separately here, the independence of effect size estimates in the meta-analysis was not compromised.

Table 1. Basic Characteristics of Studies Included in the Current Meta-Analysis

Characteristics	Value
Number of studies	101
Number of samples (all outcomes)	
Experimental	19
Correlational	64
Longitudinal	31
M age range (in years) of included studies	5.5–17.2
Overall *N*	106,070

Basic characteristics of the studies included in the meta-analysis are presented in Table 1. Details of the effect size extractions are presented in Appendix A.

Analysis

The Comprehensive Meta-Analysis software program was used to fit both random and fixed effects models. Hunter and Schmidt (2004) have argued that random effects models are appropriate when population parameters may vary across studies, as is likely here. Thus, only random effects are reported.

All results discussed later in this article are coded such that positive effect sizes represent associations with negative outcomes. Thus, a positive effect size between video game use and prosocial behavior, for instance, would represent an indication that video games harmed prosocial behavior by reducing it. This coding was done to represent negative effects consistently across effect sizes.

Overall effects for video games on child outcomes

As expected, studies in which control variables were used were heterogeneous regarding which controls were used. Some researchers controlled only for gender or, in longitudinal studies, for Time 1 aggression. Personality traits related to trait aggression or antisocial traits as well as family environment variables were also commonly controlled across a majority of studies in which controls were used (consistent with recommendations by Savage, 2004). Peer-related variables were also controlled in some studies, but beyond these few choices, control variables were quite heterogeneous and may have reflected the variables on-hand in a given data set rather than strategies for

Table 2. Meta-Analytic Results for Video Game Exposure on Outcome Variables for All Studies With Controlled Effect Sizes (Top) and Bivariate Effect Sizes (Bottom)

Effect size	*k*	r_+	r_c	95% CI	Homogeneity test	I^2	Publication bias?
Studies with controlled effect sizes							
Aggressive behavior	66	.06		[.04, .08]	$\chi^2(65) = 223.47$, $p < .001$	70.9	No
Prosocial behavior	21	.04		[.00, .07]	$\chi^2(20) = 70.70$, $p < .01$	71.7	No
Academic performance	12	−.01		[−.04, .01]	$\chi^2(11) = 14.34$, $p = .21$	23.3	No
Depressive symptoms	15	.04		[.01, .07]	$\chi^2(14) = 45.60$, $p < .001$	71.7	No
Attention deficit symptoms	6	.03		[.00, .06]	$\chi^2(5) = 14.04$, $p < .01$	64.4	No
Studies with bivariate effect sizes							
Aggressive behavior	68	.14	.08	[.12, .16]	$\chi^2(67) = 656.79$, $p < .001$	89.9	Yes
Prosocial behavior	21	.14		[.08, .19]	$\chi^2(20) = 226.50$, $p < .001$	91.2	No
Academic performance	19	.08		[.04, .13]	$\chi^2(18) = 143.57$, $p < .001$	87.5	No
Depressive symptoms	19	.04		[.01, .07]	$\chi^2(18) = 125.40$, $p < .001$	85.6	No
Attention deficit symptoms	9	.10		[.06, .13]	$\chi^2(8) = 20.03$, $p < .01$	62.0	No

Note: This table includes both studies of violent game exposure and studies of overall game exposure together. *k* = number of studies; r_+ = pooled effect size estimate; r_c = the effect size corrected for publication bias when applicable; CI = confidence interval; I^2 = heterogeneity statistic; publication bias = decision that is based on the Tandem Procedure.

controlling key effects. Note that simply including some variables as control variables is not necessarily a panacea to spurious effects, as the inclusion of particularly theoretically relevant control variables rather than the raw number of control variables is most important (Baumrind, Larzelere, & Cowan, 2002).

All studies. The results for all studies of video games on the five child outcomes are presented in Table 2. Bivariate results suggest that video games may have small covaried relationships with aggressive behavior, reduced prosocial behavior, and attention deficit symptoms, although effects for depressive symptoms and reduced academic performance are close to zero. However, controlled effects render all results near zero.

General videogames. The same outcomes are presented in Table 3 but for studies of general video game use only—that is to say, studies in which researchers examined total video game viewing but did not specifically measure exposure to violent content. Analyses for which too few studies were present are represented with an "N/A." Bivariate results suggest small links between video game use and attention deficit problems, but other outcomes are near zero. With controlled effect sizes, all results are near zero.

Violent videogames. The same outcomes are presented in Table 4 but only for studies in which video game violence specifically was examined. Bivariate results suggest that violent video games have small relationships with aggression, decreased prosocial behavior, and reduced academic performance but not with depression. However, controlled effect sizes show that links between video game violence and aggression as well as reduced prosocial behavior are near zero (there were too few studies in which academic performance was examined with controlled effects).

Thus, broadly speaking, across analyses, bivariate results are generally small to very small, and controlled analyses, such as those from multiple regression, tend to produce effect sizes only marginally larger than $r = .00$. Studies in which aggression was examined were, by far, the most common, and thus moderator analyses were conducted on these studies. Heterogeneity statistics are significant for studies of video game violence on aggression, suggesting the presence of moderators.

Moderator analyses

In Table 5, I present meta-analytic results for violent video games on childhood aggression, broken down across three main study types, using controlled effect sizes. Results indicate that the influence of video game violence on aggression is near zero across all three study types: correlational, longitudinal, and experimental.

Meta-analytic results across categorical moderators are presented in Table 6. Regarding the issue of standardized aggression measures, results indicate that standardized aggression measures are associated with somewhat smaller effects than unstandardized measures.

The concern of citation/selective reporting bias was also considered. It is plausible that citation bias in the literature review of a study could indicate researcher biases

Table 3. Meta-Analytic Results for General Video Game Use on Outcome Variables for Studies With Controlled Effect Sizes (Top) and Bivariate Effect Sizes (Bottom)

Effect size	*k*	r_+	r_c	95% CI	Homogeneity test	I^2	Publication bias?
Studies with controlled effect sizes							
Aggressive behavior	11	.03		[−.01, .07]	$\chi^2(10) = 69.43$, $p < .01$	85.6	No
Prosocial behavior	N/A						
Academic performance	8	.00	−.02	[−.04, .02]	$\chi^2(7) = 11.06$, $p = .14$	36.7	Yes
Depressive symptoms	10	.05		[.01, .09]	$\chi^2(9) = 45.33$, $p < .001$	80.1	No
Attention deficit Symptoms	N/A						
Studies with bivariate effect sizes							
Aggressive behavior	20	.07		[.04, .10]	$\chi^2(19) = 124.94$, $p < .001$	84.7	No
Prosocial behavior	5	.08		[.02, .14]	$\chi^2(4) = 23.06$, $p < .001$	82.7	No
Academic performance	12	.07		[.02, .12]	$\chi^2(11) = 77.16$, $p < .001$	85.7	No
Depressive symptoms	14	.05		[.01, .09]	$\chi^2(13) = 108.63$, $p < .001$	88.0	No
Attention deficit symptoms	6	.10		[.05, .14]	$\chi^2(5) = 16.72$, $p < .01$	70.1	No

Note: General video game use refers to studies of overall game exposure but not violent game exposure specifically. *k* = number of studies; r_+ = pooled effect size estimate; r_c = the effect size corrected for publication bias when applicable; CI = confidence interval; I^2 = heterogeneity statistic; publication bias = decision that is based on the Tandem Procedure; N/A = not applicable.

that could influence results, particularly in regard to methodological flexibility/researchers' degrees of freedom (e.g., Simmons et al., 2011). The potential for researcher bias is worth considering as a moderator (Starr & Davila, 2008). Studies were coded as experiencing citation bias only if the authors did not cite a single study disconfirming their arguments, whether for or against effects. Studies demonstrating citation bias in the literature review returned larger effects on average than those with more balanced literature reviews.

A "best practices" approach was used to examine whether studies with better methodologies would demonstrate higher or lower effect sizes. The following best practices criteria were used:

1. In the studies, researchers used well-validated and standardized outcome measures. Such measures did not give scholars flexibility to choose from among various possible outcome indices but rather specified in advance how aggression would be measured. Such measures had also been well-validated as measures of real-world aggression.
2. In the experimental studies, researchers carefully matched video games on variables other than the independent variable of interest (Adachi & Willoughby, 2010).
3. Video games used in experimental studies accurately reflected the content intended (e.g., nonviolent video games really contained no violence).

Table 4. Meta-Analytic Results for Violent Video Game Use on Outcome Variables for Studies With Controlled Effect Sizes (Top) and Bivariate Effect Sizes (Bottom)

Effect size	*k*	r_+	r_c	95% CI	Homogeneity test	I^2	Publication bias?
Studies with controlled effect sizes							
Aggressive behavior	55	.06		[.04, .09]	$\chi^2(54) = 135.87$, $p < .001$	60.2	No
Prosocial behavior	18	.04		[−.01, .09]	$\chi^2(17) = 49.56$, $p < .001$	65.7	No
Academic performance	N/A						
Depressive symptoms	5	.00		[−.03, .04]	$\chi^2(4) = 1.85$, $p = .76$	00.0	No
Attention deficit symptoms	N/A						
Studies with bivariate effect sizes							
Aggressive behavior	48	.17		[.14, .20]	$\chi^2(47) = 309.49$, $p < .001$	84.8	No
Prosocial behavior	16	.15		[.06, .24]	$\chi^2(15) = 155.84$, $p < .001$	90.4	No
Academic performance	7	.11		[.02, .20]	$\chi^2(6) = 65.15$, $p < .001$	90.8	No
Depressive symptoms	5	.02		[−.05, .09]	$\chi^2(4) = 11.85$, $p < .05$	66.2	No
Attention deficit symptoms	N/A						

Note: *k* = number of studies; r_+ = pooled effect size estimate; r_c = the effect size corrected for publication bias when applicable; CI = confidence interval; I^2 = heterogeneity statistic; publication bias = decision that is based on the Tandem Procedure; N/A = not applicable.

Table 5. Meta-Analytic Results for Violent Video Game Use on Child Aggression Across Study Types for Controlled Effect Sizes

Effect size	*k*	r_+	95% CI	Homogeneity test	I^2	Publication bias?
Correlational	19	.04	[.01, .08]	$\chi^2(18) = 46.37$, $p < .001$	61.2	No
Longitudinal	20	.08	[.05, .11]	$\chi^2(19) = 52.29$, $p < .001$	63.7	No
Experimental	16	.09	[.03, .16]	$\chi^2(15) = 24.26$, $p = .06$	38.6	No

Note: *k* = number of studies; r_+ = pooled effect size estimate; CI = confidence interval; I^2 = heterogeneity statistic; publication bias = decision that is based on the Tandem Procedure.

4. In correlational studies, researchers controlled, at minimum, for gender and (for video game violence studies) trait aggression (or similar constructs such as antisocial traits). In longitudinal studies, researchers also controlled for Time 1 outcome variables in addition to those described earlier.

Best practice studies were associated with slightly smaller effects compared with nonbest practice studies.

Regarding ethnicity, Eastern samples returned smaller effects, as did Latin/Hispanic samples, than did Western samples. However, given that effect sizes across studies were generally small, differences among these moderator variables were also fairly small. Gender differences were also negligible. Meta-regression analyses revealed that age of the child, publication year, and length of the longitudinal period for longitudinal studies were not significant moderator variables.

Publication Bias

It has been known for many years (e.g., Rosenthal, 1979) that the selective publication of statistically significant reports can bias research fields and meta-analyses drawn from them. Thus, the problem of publication bias was carefully considered in the current analysis. One way of addressing this concern is to use several tests of publication bias, as suggested by Ferguson and Brannick (2012). Given that their individual weaknesses differ, combining them to make decisions about publication bias reduces the potential for Type I error. Therefore, the Tandem Procedure[7] suggested by Ferguson and Brannick was

Table 6. Moderator Analysis for Categorical Moderators of Aggression Studies on Children for Controlled Effect Sizes

Effect size	*k*	r_+	r_c	95% CI	Homogeneity test	I^2	Publication bias?
Standardization							
Standardized	22	.04		[.02, .07]	$\chi^2(21) = 70.33$, $p < .001$	70.1	No
Unstandardized	43	.07		[.05, .10]	$\chi^2(42) = 121.17$, $p < .001$	65.3	No
Citation bias							
Yes	35	.09		[.06, .12]	$\chi^2(34) = 86.13$, $p < .001$	60.5	No
No	31	.03		[.00, .05]	$\chi^2(30) = 111.28$, $p < .001$	73.0	No
Best practices							
Yes	17	.03		[−.01, .06]	$\chi^2(16) = 65.82$, $p < .001$	75.7	No
No	48	.07		[.05, .10]	$\chi^2(47) = 124.44$, $p < .001$	62.2	No
Ethnicity							
Eastern	7	.03		[−.03, .10]	$\chi^2(6) = 24.76$, $p < .001$	75.8	No
Latin	4	−.03		[−.13, .08]	$\chi^2(3) = 7.78$, $p = .05$	61.4	No
Western	54	.07		[.05, .09]	$\chi^2(53) = 187.7$, $p < .001$	71.8	No
Gender							
Male	16	.04		[.00, .08]	$\chi^2(15) = 32.69$, $p < .01$	54.1	No
Female	12	.05		[.03, .07]	$\chi^2(11) = 6.17$, $p = .86$	00.0	No
Dissertation							
Yes	5	.02		[−.03, .08]	$\chi^2(4) = 3.08$, $p = .55$	00.0	No
Published/in press	53	.07	.04	[.04, .09]	$\chi^2(52) = 197.63$, $p < .001$	73.7	Yes
Unpublished	8	.02		[−.06, .11]	$\chi^2(7) = 21.59$, $p < .01$	67.6	No

Note: *k* = number of studies; r_+ = pooled effect size estimate; r_c = the effect size corrected for publication bias when applicable; CI = confidence interval; I^2 = heterogeneity statistic; publication bias = decision that is based on the Tandem Procedure.

used. Publication bias was also addressed through a search for unpublished manuscripts as noted earlier.

Moderator analyses indicate that dissertations in which video game violence on aggression was examined as well as unpublished data in general had lower effect sizes than did published studies overall. Tandem Procedure results also indicate that published studies demonstrate evidence for publication bias, suggesting that publication bias remains a problem for the field.

Discussion

The overall results of the meta-analysis indicate that video games, whether violent or nonviolent, have minimal deleterious influence on children's well-being. This is particularly true in studies in which other variables were controlled for and in which well-standardized and validated outcome measures were used. Furthermore, publication bias appears to be a continuing concern for studies of aggression. These results shed light on the relative importance of the issue of videogames on children's well-being, relative to other issues such as poverty, mental health, or family violence.

Differing results for bivariate and controlled analyses

Among other concerns, results indicate the importance of distinguishing between bivariate and multivariate controlled effects. Data involving multivariate controlled effects are considered to be the gold standard in media research (Savage, 2004). This is because bivariate effects may be spuriously high, as noted by game/aggression links being explained by boys both playing more games and behaving more aggressively (Przybylski & Mishkin, in press). Results of the current analysis indicate that, indeed, misuse of bivariate results can misinform the scholarly community and general public about video game influences. Current bivariate results range from small to very small. Bivariate results for video game violence on aggression are consistent with Sherry's (2001, 2007) previous meta-analyses for instance (both between $r = .15$ and $r = .17$). However, multivariate controlled results are universally near zero, suggesting that even small video game effects are probably explained by other, more critical factors. Studies in which researchers used controls naturally were heterogeneous in controls used, although gender, personality (particularly trait aggression), and family/parenting variables were most common, suggesting that these variables are particularly recognized as important control variables (see Savage, 2004).

Results from controlled studies of video game effects suggest that the influence of video games on deleterious outcomes in children is minimal. This is perhaps not surprising, given Sherry's (2001) observation that effect sizes were weaker for child samples than for college student samples. These data also fit with the epidemiological data that have not indicated a blossoming public health issue during the video game era (Olson, 2010). The absence of moderator effects for child age or longitudinal period do not lend support to common beliefs that younger children may be particularly susceptible to negative effects or that effects may accumulate over time.

Confidence intervals for effect size estimates are generally fairly narrow, owing mainly to the large number of studies and the almost uniformly small size of the controlled effect sizes in particular. Controlled effects that reached the level of $r = .30$ are very rare.

It is worth noting that controlling for Time 1 variables in longitudinal studies, particularly studies of aggression, is qualitatively different from using other control variables. Because aggression tends to be highly stable over time, controlling for Time 1 aggression tends to limit the variance in an analysis. However, arguably, this is precisely the importance of controlling for Time 1 aggression. A stable trait such as aggression may be particularly resistant to the influence of video games, and controlling for prescores is crucial to eliminate variance that is due to selection effects (see Breuer, Vogelgesang, Quandt, & Festl, in press; von Salisch et al., 2011).

Better studies, smaller effects

Results also suggest that studies in which researchers used better methodologies tend to be less likely to produce evidence for negative effects. However, given that effects were generally weak, these differences were fairly small. Nonetheless, the potential remains for poorly designed studies to inflate effect sizes. Studies in which researchers used unstandardized outcome measures tend to produce higher, potentially inflated effect sizes. The issue of citation/selective reporting bias is particularly concerning, given that such reporting biases may perpetuate false beliefs in the scholarly community and general public about research results. Results of this study suggest that researcher biases, as evidenced by citation bias in literature reviews, do play a role in influencing the effect sizes of resultant publications. Citation bias was associated with increased effect sizes in this area of research. Citation bias is likely endemic to much of psychology and science more broadly. It is not implied here that citation bias is unique to video game research. However, that it is common elsewhere does not make it any less problematic for this field, and researchers should be encouraged to portray the research field more faithfully in their literature reviews.

One issue that often remains unclear in the field is the interpretation of null effects. This is a particularly difficult

problem both in interpreting smaller studies that may produce null findings because of Type II error as well as larger studies that may produce statistically significant findings with effect sizes near zero (e.g., Willoughby et al., 2012). It is also not clear how many null findings are required to negate a particular theory. Certainly, most scholars would agree that a small number of null findings should not invalidate a given theory, but it may also become overly convenient to dismiss all null findings as Type II error (Simons, 2014), although direct replication failures may be of some particularly importance (e.g., Przybylski et al., 2014; Tear & Nielson, 2013). Arguably, this has been one of the more intractable issues for the field. It may require considerable debate and discussion before it becomes clear exactly when theories of video game violence effects, at least the broad-based-effects model, should be replaced.

Practical recommendations for psychological science

The results of this study provide evidence to support various calls (e.g., Ferguson, 2013; Granic et al., 2014; Hall, Day, & Hall, 2011a; Savage, 2004; Sherry, 2007) for some degree of reform or reevaluation of the video game research field. In this section, I offer several pragmatic suggestions for how this research field may improve in theoretical and empirical rigor.

The need for new theory. Historically, theories of media effects have been focused on "hypodermic needle" type theories, in which it is implied that media is essentially injected into passive viewers who automatically model viewed behaviors through the activation of cognitive scripts (see Sherry, 2004, for discussion). Such theories arguably have not been well supported by the current literature and may suffer from problematic assumptions such as that the brain treats fictional media similarly to real-life violence exposure (e.g., Bushman & Huesmann, 2014). Given that research suggests that children's processing of fictional media begins to differ from nonfictional data at an early age and develops over time (e.g., Woolley & Van Reet, 2006), these cognitive script theories may lack the proper developmental approach to understanding media effects.

Two related theories, one from communication science, the other from psychology, may provide some useful guidance, particularly in helping to understand how media consumers are active shapers, seekers, and processors of media information. These include the uses and gratifications theory of communication (Sherry, Lucas, Greenberg, & Lachlan, 2006) as well as the self-determination theory in psychology (Przybylski, Weinstein, Murayama, Lynch, & Ryan, 2012; Ryan, Rigby, & Przybylski, 2006). Put briefly here, in both approaches, media users are considered as active shapers of media, who consume media as active agents attempting to meet particular psychological needs. Therefore, for example, a child may play video games to meet needs related to autonomy or competence that are not being met adequately in real life. Understanding why kids play the games they do has generally been a critical factor that has been overlooked in much of the research. Academic dialogue on children and video games has largely failed to take a motivational perspective.

One related theoretical perspective to consider may be that of the downward spiral model in which specific at-risk groups of youths actively select violent media, which, in turn, increases their aggressiveness (Slater, Henry, Swaim, & Anderson, 2003). Studies of video game influences on specific at-risk groups remain few in number. Some evidence suggests that video game violence may interact with preexisting anger in college students (Markey & Scherer, 2009), although only for minor acts of aggression in the laboratory. However, evidence among youths is lacking. In one recent cross-sectional study of at-risk youths, DeLisi, Vaughn, Gentile, Anderson, and Shook (2013) found very small correlations between aggression and video game violence. However, their analysis is limited by the absence of a control group and reliance on asking youths to rate the violence in the video games they played themselves as well as their own self-reported aggression, potentially introducing demand characteristics. By contrast, in another recent correlational study of youth violence, Ferguson and Olson (2014) found no evidence for correlational relationships between violent game use and aggression in a sample of youths with elevated mental health symptoms, although this study too was limited by its correlational and self-report nature (although violence exposure was calculated with game ratings rather than youth self-report). In considering at-risk youths, Unsworth et al. (2007) found that violent game effects are idiosyncratic, having very variable effects on youths, sometimes increasing, sometimes decreasing, but most often having no influence on individual youths. With that in mind, blanket statements of effects may be ill-advised, suggesting media effects may be small and variable rather than large and ubiquitous. It is also worth noting that video games are only one part of a child's media use, and youths may be exposed to violent media through a variety of media, from books through social media.

Similarly, it may be increasingly important to understand children's video game use from a perspective of normal development (Olson, 2010)—something that, once again, has generally been lacking in the literature. Further research is certainly needed to examine how children process media and how they may process

different media, such as advertising and fictional media, in different ways. More theory is also needed to understand the developmental processes that children use to understand and interpret the media they use. Generally, a developmental focus to video game studies has been lacking, and in traditional theories, the experience of gaming is not put into a larger developmental focus.

From a developmental perspective, it would be valuable to incorporate literature on children's reality testing. Further, it would help to understand how motivational factors for video game use change across age periods. How, also, does video game use fit in with or compete with other developmental processes, such as the influences of family and peers?

The need for standardized measures and rigorous methods. As researchers in the field become aware of methodological flexibility/researchers' degrees of freedom issues, reviewers and journal editors should insist that researchers use standardized, well-validated outcome measures. Recent evidence has clarified that unstandardized outcome measures have significant influence on potentially spurious effect size estimates (Elson et al., 2014). It is not enough for authors to claim that they do not intend their studies to be generalized to socially relevant public health concerns, as this will happen whether study authors intend it or not. Thus, research on children's exposure to video games should be held to a very high clinical standard.

Outcome measures in the field also tend to be highly heterogeneous. Despite this, they continue to be conceptualized as a unitary construct of "aggression" and are often generalized to serious acts of social violence. Some authors have argued for the validity of this approach, suggesting that such heterogeneous measures correlate well with each other (Anderson, Lindsay, & Bushman, 1999). However, given problems with lack of standardization of these measures, such correlations may be reflective of experimenter expectancy effects rather than true intercorrelations. Further, recent reanalysis of these observed intercorrelations has proven less sanguine regarding the interreliability of these heterogeneous measures (Mitchell, 2012). As long as outcome measures continue to be heterogeneous and unstandardized, the ability of researchers in the field to measure true population-approximate effect sizes from samples regarding the construct of aggression may be limited.

Rigorous methodology also entails careful attention to matching of video games in experimental conditions (Adachi & Willoughby, 2010) and avoiding the introduction of expectancy effects and other confounds in all research (Boot et al., 2011). Further, in more experimental studies, researchers should consider including careful pretest/posttest designs, without which it is unclear whether video games increase or decrease negative outcomes. More rigorous research will help guide both scientific position statements and public policy with good, rigorous data.

The need to study mental illness. The results of the current analysis suggest that, at least with samples of typical community children, the influence of video games is negligible. However, the current data are not yet capable of answering whether specific subpopulations of vulnerable children are more prone to negative influences of video games. For instance Markey and Markey (2010), working with college students, found that violent video games may have small interaction effects with the psychoticism personality trait. However, in samples of children, researchers have not always found evidence for such an interaction effect (e.g., Ferguson, 2011b), and in one recent analysis, Ferguson and Olson (2014) found little evidence for video game violence effects in a sample of children with preexisting mental health symptoms. Likewise, another recent study found that neither individuals with autism spectrum disorders nor those without are influenced by violent video games (Engelhardt et al., in press). Nonetheless, samples of children with preexisting mental health symptoms are lacking in the literature and may help answer questions about potentially vulnerable subpopulations, even if video games do not have general negative effects on the majority of children.

Moving beyond the concept of violent video game. One issue for the field that became apparent in the current analysis was the ambiguity with which the term "violent video game" is used. The concept of violent video game is defined so broadly that almost all video games could be considered violent, including those intended for children (K. M. Thompson & Haninger, 2001). In one recent murder case, a testifying psychologist acknowledged that even games such as *Pac Man* could conceivably be considered violent (Rushton, 2013). Thus, it should not be surprising that video games treated as violent vary widely among studies, and yet they are treated by researchers in the field as if occupying a single, meaningful, conceptual space. However, compiling primitive old-school games such as *Pac Man* with multiplayer games such as *World of Warcraft* with shooter games such as *Call of Duty* under the concept of violent video game is likely about as meaningful as compiling the Christian Bible, the *Red Badge of Courage*, and *Cujo* together as "violent literature" in a single conceptual space because all happen to contain violence.

Thus, it is possible that the very concept of violent video game is of limited utility, serving mainly a political and moral purpose rather than a scientific one. It may be valuable for researchers in the field to move beyond such simplistic and moralistic concepts to better understand the full range of video game experiences.

Caution in public statements. The incautious statements of some scholars following the Sandy Hook shooting risked damaging the credibility of the field, particularly when such statements were not consistent with available data (see Hall, Day, & Hall, 2011a). Scholarly statements linking Sandy Hook with video games were often made before any official data were available regarding the individual perpetrator's media use history. Such statements risk contributing to an atmosphere of moral panic (Ferguson, 2013) that can distract society from more pressing issues related to violence. Particularly in light of the official investigation report in which the Sandy Hook shooter was found to be more interested in nonviolent games than violent games (State's Attorney for the Judicial District of Danbury, 2013), this example should serve as a cautionary note for the field.

This is not to say that scholars can never express the opinion that video games may be linked with negative outcomes for some individuals, just as scholars can express the opinion that video games have no such links with negative outcomes. Indeed, although there are good studies that suggest video games have no effect on children, there are also good studies that suggest video games may have some deleterious effects, at least on some consumers (e.g., Giumetti & Markey, 2007; Markey & Scherer, 2009; Willoughby et al., 2012). However, scholars must be cautious to avoid common headline-worthy but untrue "talking points" such as that research consistently demonstrates negative effects, effects are similar to important medical effects, or that the existing research is easily generalizable to societal violence (F. Farley, 2012). Thus, scholars should, at minimum, honestly inform the public about debates and discrepancies within the field and caution that most studies of aggression cannot easily answer "big V" questions about societal violence (F. Farley, 2012). Groups such as the APA also would be well served by soliciting opinions from experts on all sides of politicized academic debates (an approach which they have thus far consistently eschewed). Further, position statements should not be written by scholars heavily invested in a particular position on an issue. Rather a jury-like, voir dire process may help constitute review committees of objective, uninvolved scholars who are able to review a research field without prior convictions about what conclusions a positions statement should reach. Unfortunately, this did not occur for either the 2005 task force or the current task force working as of this writing.

Limitations

As with all studies, the current meta-analysis has limitations. First, studies of video game violence on aggression outnumbered other areas of study. Thus, it was not possible to test for moderator effects with relatively limited pools of research in other areas. There clearly remains a need for studies of child samples in which potential mental and behavioral health issues outside of aggression are examined. Second, although publication bias was assessed, in such assessments, researchers examine only for bias that is due to nonpublication of null results. They do not assess for bias that is due to methodological flexibility issues that also may bias results. For example, methodological flexibility may allow for the conversion of a null finding to a statistically significant finding without necessarily using increased sample sizes. Thus, the true amount of bias in the field is difficult to assess. Third, all meta-analyses are only as good as the studies that are included within them. As indicated, there remains great need for researchers in this field to increase the rigor of their methodology. Thus, results from the current crop of video game studies should only be generalized to public health-related issues with caution.

Although there are reasons to express concern about bias in the field, it is interesting to see that null studies for video game effects remain common. However, it also may be interesting to examine the relative penetration of null and statistically significant findings both in the field and in the general public. It may be, for instance, that even if research results remain inconsistent, studies supporting a particular view may be reported more often both in the field and in the general public, thus creating a false impression of research results.

Concluding statements

The field of video game violence is riven with controversy and politics. Given how enmeshed this field is with tragic events in society (whether rightly or wrongly), the controversy is unlikely to dissipate in the near future. Debates among scholars with different views on this topic are potentially healthy and elucidating for all involved. By contrast, incautious statements not carefully representing the often inconsistent research are likely to damage the credibility of the field in the long term (Hall, Day, & Hall, 2011a). It is hoped that the current analysis may provide some small guidance in these discussions moving forward.

Appendix A

Effect size estimates

One issue that has arisen as a potential problem for meta-analyses is the proper extraction of effect size estimates. To meet the homogeneity assumption of meta-analysis, most conductors of meta-analyses have extracted the equivalent of bivariate "*r*," particularly from correlational or longitudinal data. However, this approach risks

providing spurious estimates of effects. For instance, with video games, it is well established that boys are both more aggressive and more prone to playing violent video games (Olson et al., 2007). Thus, bivariate correlations between aggression and video game use may simply be due to gender, and it is essential that gender is controlled. Given that well-controlled multivariate analyses are considered the gold standard in aggression research (Savage & Yancey, 2008), for meta-analyses to rely solely on bivariate *r* leads to increased risks of misleading causal conclusions coming from these analyses. For a meta-analysis to remain rooted to bivariate *r*, it would be theoretically possible for every single study to have the conclusion that any correlation between video games and negative outcomes was reduced to nonsignificance after other factors were controlled; however, it would also be theoretically possible for a meta-analysis of these studies to have the conclusion that significant effect existed. In this circumstance, reliance on the bivariate *r*, when examining well-controlled multivariate correlational and longitudinal studies in meta-analysis, is problematic.

If reliance on bivariate *r* is problematic, the solution is unclear. Several authors have suggested that betas indeed can be used as effect size estimates in meta-analyses. As Rosenthal and DiMatteo (2001) noted, betas can be used as effect size estimates, with the cautionary note to recall that betas use multivariate controls as opposed to *r*s. Other authors have echoed this basic view (J. U. Farley, Lehmann, & Sawyer, 1995; Raju, Fralicx, & Steinhaus, 1986).

In the present analysis, a dual approach is used. That is, results from both bivariate and multivariate controlled effect sizes are presented. This allows scholars to examine the difference between them. The effect size *r* was used in this analysis both because of the inclusion of numerous longitudinal and correlational effect sizes in the analysis and because *r* tends to be straightforward as an effect size and easy to interpret.

In some cases, researchers presented more than one effect size relevant to a single construct (e.g., using two or more separate measures of aggression or depression). In these cases, they were aggregated for an average effect size. Similarly, in some cases, a single data set may have produced several publications in which the same outcome for the same time point for the same sample was considered. Unless the data represented different time points (i.e., correlational and longitudinal data in separate publications), data sets were included only once in the meta-analysis. In some articles, researchers presented multiple competing statistical models with different effect size estimates, particularly for multivariate analyses. When this occurred, the most conservative model was used as the effect size estimate for the controlled analyses. Given the question of how much variance remains for video game effects, after other factors were well-controlled, this approach was viewed as valuable.

Although it was not common, in several articles, results were reported as nonsignificant without an effect size reported or data sufficient to calculate an effect size. When this occurred, attempts were made to contact the original authors for relevant data. If such data were no longer available, or if authors did not respond, null effects were entered as zero so as not to spuriously exclude null effects from the analysis. Effect size estimates for the included studies are provided online at http://www.christopherjferguson.com/Book1.xlsx. This file provides the most conservative effect size drawn from each study. All effect size estimates are weighted for sample size.

Declaration of Conflicting Interests

The author declared no conflicts of interest with respect to the authorship or the publication of this article.

Notes

1. In the official investigation report by the State of Connecticut, it was revealed that Lanza owned both violent and nonviolent video games, although most games he owned were old and outdated. However, the report clarified that Lanza was most involved in playing nonviolent video games—such as Dance, Dance Revolution, and Super Mario Brothers—rather than violent games. Investigation files released by the State of Connecticut also include examples of police officers warning victims' families not to pay too much attention to video game or other "hoax" theories circulating in media reports. Many of the rumors about Lanza's alleged obsession with violent games appear to have been based on unreliable sources rather than the official investigation. In the official report, investigators did not link violent games to the crime (State's Attorney for the Judicial District of Danbury, 2013).
2. The APA has assigned a new task force to review its policy statement on video game violence, although this task force has also been controversial. In 2013, a group of approximately 238 scholars (Consortium of Scholars, 2013) wrote to the APA asking them to retire their policy statement, in part out of concern that the new task force, much like the 2005 task force, was mainly composed of scholars with a priori antigame positions without moderating voices.
3. These designs include both pretest/posttest experimental and control groups as well as experimental and control groups without pretest. Such designs allow for testing of both the experimental manipulation, allowing for changes over time as well as any potential confounds that are due to testing effects.
4. The Taylor Competitive Reaction Time Test involves players who think they are playing a reaction time game against a consenting opponent. The player sets levels of static noise in terms of loudness and duration that his or her opponent will hear if he or she loses. The consenting nature of the task as well as the official sanction for the behavior in the form of researcher instructions likely violate most definitions of aggression. However, the task is also unstandardized, allowing multiple

methods for extracting aggression with the same sample outcomes, which can be quite variable. Misuse of this instrument has, by now, been well-documented (see Ferguson, 2013, for extended discussion).

5. *Aggression* was defined here as purposeful behavior intended to harm another. Thus, in the current analysis, I focused on behavior. It is acknowledged that how aggressive behavior is conceptualized across studies varies widely and has, at times, been controversial (see Ritter & Eslea, 2005). All studies in which researchers attempted to measure some form of aggressive behavior are included here, although in the analysis I also examine some methodological issues, such as lack of standardization, that may influence outcomes.

6. One study (Gentile et al., 2009) was not included out of concerns of multi-collinearity. In this study, the authors attempted to calculate separate violent and prosocial game categories. The authors noted that these were highly correlated, yet they entered them together into regression equations. The authors then found "bouncing beta" standardized regression coefficients in opposing directions, despite the predictor variables being highly correlated. The authors also reported variance inflation factor levels near 10, which tend to produce spurious multi-collinearity results.

7. The Tandem Procedure is a conservative estimate of publication bias, in which researchers reduce Type I error by using a decision matrix for identifying publication bias. The meta-analytic effect size is examined for fragility, with near-trivial effect sizes considered most prone to bias. Several correlation approaches for sample size and effect size are also considered, as is the trim and fill procedure for potential missing studies. Agreement among measures is considered evidence for publication bias. However, this procedure is very conservative, and it is likely that many cases of publication bias, particularly those arising from QRPs, will not be identified by this procedure. Thus, this should be considered only as a conservative test for bias.

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