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Much Ado About Nothing: The Misestimation and Overinterpretation of Violent Video Game Effects in Eastern and Western Nations: Comment on Anderson et al. (2010)

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The issue of violent video game influences on youth violence and aggression remains intensely debated in the scholarly literature and among the general public. Several recent meta-analyses, examining outcome measures most closely related to serious aggressive acts, found little evidence for a relationship between violent video games and aggression or violence. In a new meta-analysis, C. A. Anderson et al. (2010) questioned these findings. However, their analysis has several methodological issues that limit the interpretability of their results. In their analysis, C. A. Anderson et al. included many studies that do not relate well to serious aggression, an apparently biased sample of unpublished studies, and a "best practices" analysis that appears unreliable and does not consider the impact of unstandardized aggression measures on the inflation of effect size estimates. They also focused on bivariate correlations rather than better controlled estimates of effects. Despite a number of methodological flaws that all appear likely to inflate effect size estimates, the final estimate of $r = .15$ is still indicative of only weak effects. Contrasts between the claims of C. A. Anderson et al. (2010) and real-world data on youth violence are discussed.

Keywords: computer games, mass media, youth violence, aggression, child development

Over the last two decades, society has expressed concern that violent video games (VVGs) may play some role in youth violence. To answer some of these questions, we engaged in a series of precise meta-analyses of VVG studies that most closely related to violent outcomes (e.g., Ferguson, 2007; Ferguson & Kilburn, 2009). Indeed, we were well aware that less precise measures tend to overestimate effects (Paik & Comstock, 1994). We also had questions regarding whether journals had been selectively publishing significant studies and potentially ignoring nonsignificant studies. Our results were clear: The influence of VVGs on serious acts of aggression or violence is minimal, and publication bias is a problem in this research field. We also noted (as did Paik & Comstock, 1994) that the best measures of aggression and violence produced the weakest effects and that problematic unstandardized use of some aggression measures, particularly in experimental studies, tended to inflate effects.

Points of Agreement and Disagreement With Anderson et al.

Anderson et al. (2010) critiqued our analyses and offered an alternative of their own. Our analyses agree that the uncorrected estimate for VVG effects is quite small ($r = .15$ in both analyses). We also agree that meta-analytic researchers must take careful steps to minimize the influence of publication bias. But our research groups disagree on many points: whether to include unpublished studies, how best to analyze and correct for publication bias, whether bivariate correlations are a proper estimate of VVG effects, how precise standardized and valid aggression measures need to be to adequately answer research questions, and how effect size estimates should be interpreted. We have concerns that Anderson et al. have made several misstatements about our meta-analyses and meta-analyses more generally and have also made significant errors in their own analyses that render their results difficult to interpret.

Building the Perfect Meta-Analytic Beast

We are honored that Anderson et al. (2010) selected our analyses to contrast with their own. However, readers should be aware that other recent meta-analyses on VVGs and media violence more broadly have been no more supportive of Anderson et al.'s position than our own (Savage & Yancey, 2008; Sherry, 2001, 2007). Anderson et al. surprisingly cite Sherry (2001) as if supportive of their position, but in fact he is quite clear that he does not find the results of his analyses persuasive for the causal position. Indeed, he is specifically critical of the Anderson et al. research group, stating, "Further, why do some researchers (e.g., Gentile & Anderson, 2003) continue to argue that video games are dangerous despite evidence to the contrary?" (Sherry, 2001, p. 244).

Anderson et al. (2010) suggested that we should have included unpublished studies in our analyses and that the best way to negate publication bias issues is to "conduct a search for relevant studies that is thorough, systematic, unbiased, transparent, and clearly documented" (p. xx). We note that, given that one of our questions

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specifically regarded the amount of bias in the published literature, including unpublished studies would be counterintuitive. Although including unpublished studies in meta-analyses is certainly common, is it really as "widely accepted" as they claim? Further, does their meta-analysis live up to their own rhetoric?

First, Anderson et al. (2010) failed to note that many scholars have been critical of the inclusion of unpublished studies in meta-analyses. Baumeister, DeWall, and Vohs (2009) noted that one weakness of meta-analysis is that the inclusion of dubious unpublished works can "muddy the waters" (p. 490). Smith and Egger (1998), echoing our own concerns, noted that including unpublished studies increases bias, particularly when located studies are not representative of the broader array of studies. Others have noted that inclusion of unpublished studies remains controversial, although certainly common, and it is not uncommon for meta-analyses to avoid unpublished studies (Cook et al., 1993). Thus, Anderson et al.'s implication that we essentially invented the notion of avoiding unpublished studies is fanciful, much as we would like to take credit.

Despite the comments of Anderson et al. (2010) supporting a search for unpublished studies that is "thorough, systematic, unbiased, transparent, and clearly documented," they actually provide little information about how they located unpublished studies. However, one common procedure, although certainly not sufficient in and of itself, is to request unpublished studies from known researchers in the field (Egger & Smith, 1998). It is surprising then that, although the Anderson et al. researchers were in contact with us (i.e., C. J. Ferguson), they neither mentioned their meta-analysis nor requested in-press or unpublished studies. As such, they missed several in-press studies (e.g., Ferguson & Rueda, in press; Ferguson, San Miguel, & Hartley, 2009) as well as a larger number of "on review" papers and papers for which data had been collected but not yet written up. We express the concern that other research groups that, arguably, have presented research not in line with Anderson et al.'s hypotheses may not have been contacted (e.g., Barnett, Coulson, & Foreman, 2008; Colwell & Kato, 2003; Kutner & Olson, 2008; Ryan, Rigby, & Przybylski, 2006; Unsworth, Devilly, & Ward, 2007; Williams & Skoric, 2005). For example, we note that several published reports (e.g., Barnett et al., 2008; Olson et al., 2009; Przybylski, Weinstein, Ryan, & Rigby, 2009) from this group of authors have been missed. Thus, from only a small group of researchers, albeit those who differ from Anderson et al. in perspective, a considerable number of published, in-press, and unpublished studies were missed. One can only speculate at the number of other missed studies from unknown authors. On the other hand, when examining the appendix of included studies, one finds that unpublished studies from Anderson et al.'s research group and colleagues are well represented. For example, of two unpublished studies, both are from Anderson et al.'s broader research group. Of three in-press manuscripts included, two (67%) are from the Anderson et al. group. Of conference presentations included, 9 of 12 (75%) are from the Anderson et al. group and colleagues. Whatever techniques used by Anderson et al. to garner unpublished studies, these techniques worked very well for their own unpublished studies but poorly for those from other groups. We do not conclude that this was purposeful on the part of Anderson et al.; rather, this matter highlights our concerns about including unpublished studies.

Publication Bias Exists in VVG Studies

Our original meta-analyses indicated that published studies of VVGs are products of publication bias. Anderson et al. (2010) does not appear to have disputed this but suggested we should have included unpublished studies instead of our publication bias analyses. Anderson et al. focused on our use of the "trim and fill" procedure. As Anderson et al. indicated, the trim and fill is not without imperfections. However, they failed to mention that we actually used a wide range of publication bias analyses and looked for concordance between these analyses. Indeed, we found a general agreement between publication bias tests for studies of aggressive behavior and VVGs. The trim and fill procedure can function as an estimate for the degree of publication bias, particularly when there are sound theoretical reasons to expect publication bias. As Egger and Smith (1998) indicated, publication bias is quite common. Ioannidis (2005) observed that bias is particularly prevalent in new or "hot" research fields, as that on VVGs certainly is. Other scholars have expressed concern that VVG studies have become politicized, which increases the risk for bias (e.g., Grimes, Anderson, & Bergen, 2008; Kutner & Olson, 2008; Sherry, 2007). We find suggestions that VVG studies are immune to publication bias effects to be naive. However, the reader need not take our word for it. Publication bias appears evident in a previous meta-analysis by this research team (Anderson, 2004). Of the published studies ($n = 32$) in this analysis, 19 were supportive of the causal view, nine were inconclusive, and four were nonsupportive. Of the unpublished studies ($n = 11$), one was supportive, one was inconclusive, and nine were nonsupportive. The difference between published and unpublished studies is obvious.

Best Practices or Best of the Worst?

Some of the suggestions offered by Anderson et al. (2010) concerning "best practices" appear reasonable, but we express concern that they did not raise the issue of unstandardized aggression measures used in many VVG studies. A measure of aggression (or any other construct) is unstandardized when the method for calculating outcomes scores is not clearly set; this allows different scholars to calculate outcomes in very different ways (or the same author may calculate outcomes differently between studies). By contrast, a measure may be considered standardized when measurements taken from it (as well as its administration) are "set in stone" and do not vary across studies or across researchers (the aggression score developed from the Child Behavior Checklist is an example of a standardized aggression measure). The benefit of standardized measures is that researchers must accept the outcomes from these measures whether or not the outcomes are favorable to their hypothesis. Unstandardized assessments potentially allow researchers to select from among multiple outcomes those which best fit their a priori hypotheses. For instance, the Anderson et al. research group has assessed the "noise blast" aggression measure differently across multiple studies, with little explanation as to why (for a discussion, see Ferguson, 2007). Our previous analyses have suggested that unstandardized measures tend to inflate effect size estimates, as noted, potentially because researchers may ignore the "worst" outcomes and select the "best" outcomes to interpret (we argue that this is human nature and do not mean to imply any purposeful unethical behavior). Standardization is a basic tenet of psychometrics; thus, it is unfortunate that it has been so ignored in this research field. Unfortunately, the best practices-nominated studies are populated with manuscripts in which unstandardized assessments were used. This fact, rather than the quality of those reports, probably explains why the effect sizes seen for this group or paper were higher than those for other papers.

We also find that Anderson et al. (2010) did not rigidly apply their own standards. For instance, they nominated at least one paper (Konijn, Nije Bijvank, & Bushman, 2007) as best practices, although it included several games with violent content descriptors (The Sims 2, Tony Hawk's Underground 2, Final Fantasy) in its nonviolent game condition, thus making its results uninterpretable. Panee and Ballard (2002) were nominated as best practices even though all participants played the same game. Similarly, Anderson et al. seem particularly disinclined toward Williams and Skoric (2005), despite the fact that this study does indeed (contrary to Anderson et al.'s assertions) include a measure of verbal aggression at least as ecologically valid, if not more so, than that of many of those studies nominated as best practices.

Anderson et al. (2010) included several studies from which it is unclear how effect size estimates meaningful to the basic hypotheses were calculated. For example, Hagell and Newburn (1994) provided only descriptive percentiles and no analyses from which a meaningful effect size estimate could be calculated. Hind (1995) reported only the degree to which offender and nonoffender youths liked different kinds of games, not their reaction to playing these games or any correlation between play and behavior. Kestenbaum and Weinstein (1985) reported p values, but no other statistics, and these for some outcomes but not all. In the Panee and Ballard (2002) study, all participants played the same violent game without any variation in game violence content. Silvern and Williamson (1987) reported only a pretest/posttest design in which all children played the same video game (Space Invaders). We do not believe that these studies (or many others) provide meaningful information related to VVGs and youth violence.

Is Psychology Inventing a Phantom Youth Violence Crisis?

Anderson et al. (2010) neglected to report on one very basic piece of information. Namely, as VVGs have become more popular in the United States and elsewhere, violent crime rates among youths and adults in the United States, Canada, United Kingdom, Japan, and most other industrialized nations have plummeted to lows not seen since the 1960s. Figure 1 (adapted from Ferguson, F1
2008) presents this information for youth violence rates in the United States. Similar patterns are seen for other nations. Even the Anderson et al. group appears to have acknowledged that this kind of data is important to consider: "Nonetheless, dramatic reductions in media violence exposure of children should, over a several year period, lead to detectible reductions in real world aggression by those children. This would further provide evidence for a strong media violence link to aggression" (Barlett & Anderson, 2009, p. 10). In fact, we are seeing the opposite relationship, in which dramatic increases in VVGs are correlated with dramatic decreases in youth violence. The correlation coefficient for this data is $r = -.95$, a near-perfect correlation in the wrong direction. We agree with Barlett and Anderson (2009) that this kind of evidence is strong. Barlett and Anderson, of course, cannot have it both ways, with crime data important only so long as they are consistent with Barlett and Anderson's beliefs.

Last, Anderson et al. (2010) suggested that the $r = .15$ relationship is too conservative and, nonetheless, as strong as that seen in other areas of criminology. The $r = .15$ estimate includes only basic controls; therefore, this estimate is probably too liberal. Our

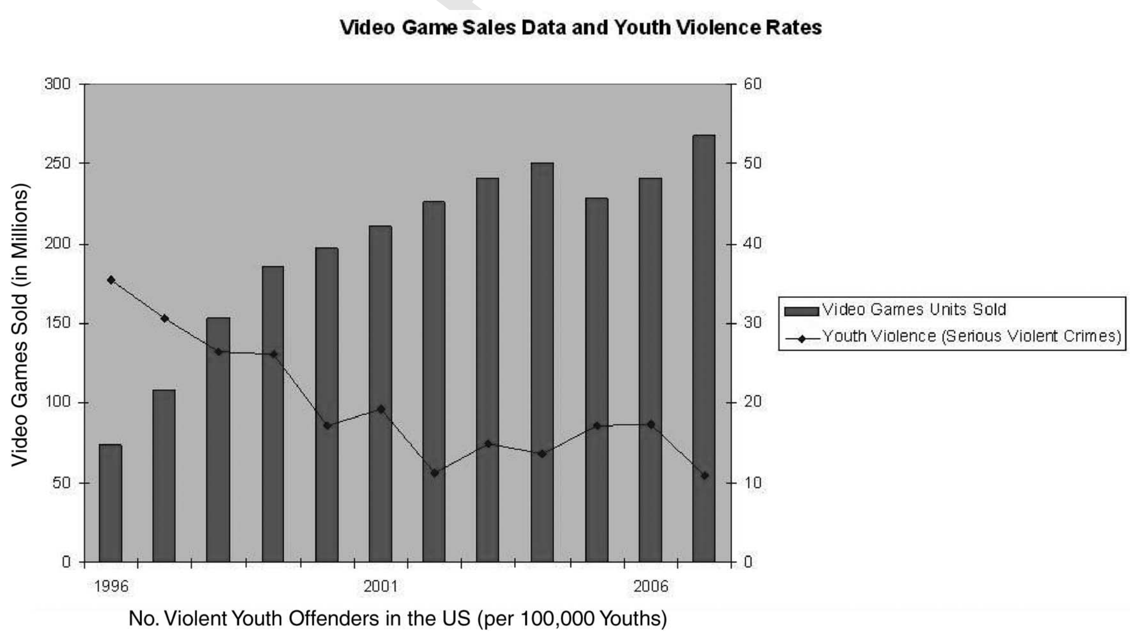

Figure 1. Trends in youth violence and video game sales in the United States. Video game data were obtained from the NPD Group, Inc./Retail Tracking Service. Youth violence data were obtained from Childstats.gov

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own research suggests that when other risk factors (e.g., depression, peers, family) are controlled, video game effects drop to near zero (Ferguson et al., 2009). Indeed, focusing on bivariate correlations is problematic, as they overestimate relationships due to third variables. Males both play more VVGs and are more aggressive than females. Thus, aggression will tend to correlate with VVGs and with any other male-dominated activity, such as growing beards, dating women, and wearing pants rather than dresses. Anderson et al. noticed this themselves. It is obvious that controlling other important risk factors related to personality, family, and even genes (if one could) would further reduce the unique predictive value of VVGs. Anderson et al. ignored this third variable effect, although it has been well known for some time. It is also not true that the $r = .15$ estimate—even if we were to believe that it is accurate—is on par with other criminological effect size estimates. Table 1 compiles a list of effect size estimates from criminology (Ferguson, 2009). As can be seen, compared to other criminological effects, the VVG connection is rather weak. Furthermore, Anderson et al. claimed that small effects may accumulate over time yet found the weakest effects from longitudinal studies, in contradiction to this claim. It should be noted that this 2.25% coefficient of determination reflects a change of nonpathological aggression to the tune of 2.25% within individuals; it does not mean that 2.25% of normal children became antisocial or any other such alarmist interpretation of this effect. We observe that Anderson et al. themselves acknowledged that this effect is for nonserious aggression (Footnote 12) due to the limitations of many of the measures included in this analysis.

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In conclusion, we believe that Anderson et al. (2010) are sincere in their concerns for children and beliefs about VVGs. However, their current meta-analysis contains numerous flaws, all of which converge on overestimating and overinterpreting the influence of VVGs on aggression. Nonetheless, they find only weak effects. Given that discussions of VVGs tend to inform public policy, both scientists and policymakers need to consider whether these results will get the "bang for their buck" out of any forthcoming policy recommendations. There are real risks that the exaggerated focus on VVGs, fueled by some scientists, distracts society from much more important causes of aggression, including poverty, peer influences, depression, family violence, and Gene × Environment interactions. Although it is certainly true that few researchers suggest that VVGs are the sole cause of violence, this does not mean they cannot be wrong about VVGs having any meaningful effect at all. Psychology, too often, has lost its ability to put the weak (if any) effects found for VVGs on aggression into a proper perspective. In doing so, it does more to misinform than inform public debates on this issue.

Table 1
Effect Sizes, Criminal Justice Research

Relationship	Effect size (*r*)
Video game sales and youth violence rates in the United States	−.95
Genetic influences on antisocial behavior	.75
Self-control and perceptions of criminal opportunity on crime	.58
Protective effect of community institutions on neighborhood crime	.39
VVG playing on visuospatial cognitive ability	.36
Firearms ownership on crime	.35
Incarceration use as a deterrent on crime	.33
Aggressive personality and violent crime	.25
Poverty on crime	.25
Childhood physical abuse and adult violent crime	.22
Child witnessing domestic violence on future aggression	.18
Video game violence and nonserious aggression[a]	.15
Television violence on violent crime	.10
VVG playing on serious aggressive behavior[b]	.04

Note. VVG = violent video game.
[a] Indicates calculated by Anderson et al. (2010). All other effects compiled by Ferguson (2009), where original sources are reported. [b] Estimate corrected for publication bias in published studies.

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