Metaanalysis of the relationship between violent video game play and physical aggression over time

Anna T. Prescott[a], James D. Sargent[b], and Jay G. Hull[a,1]

[a]Department of Psychological and Brain Sciences, Dartmouth College, Hanover, NH 03755; and [b]Department of Pediatrics, Geisel School of Medicine, Dartmouth College, Hanover, NH 03755

Edited by David E. Meyer, University of Michigan, Ann Arbor, MI, and approved August 10, 2017 (received for review August 27, 2016)

To clarify and quantify the influence of video game violence (VGV) on aggressive behavior, we conducted a metaanalysis of all prospective studies to date that assessed the relation between exposure to VGV and subsequent overt physical aggression. The search strategy identified 24 studies with over 17,000 participants and time lags ranging from 3 months to 4 years. The samples comprised various nationalities and ethnicities with mean ages from 9 to 19 years. For each study we obtained the standardized regression coefficient for the prospective effect of VGV on subsequent aggression, controlling for baseline aggression. VGV was related to aggression using both fixed [$\beta = 0.113$, 95% CI = (0.098, 0.128)] and random effects models [$\beta = 0.106$ (0.078, 0.134)]. When all available covariates were included, the size of the effect remained significant for both models [$\beta = 0.080$ (0.065, 0.094) and $\beta = 0.078$ (0.053, 0.102), respectively]. No evidence of publication bias was found. Ethnicity was a statistically significant moderator for the fixed-effects models ($P \leq 0.011$) but not for the random-effects models. Stratified analyses indicated the effect was largest among Whites, intermediate among Asians, and nonsignificant among Hispanics. Discussion focuses on the implications of such findings for current debates regarding the effects of violent video games on physical aggression.

video games | aggression | metaanalysis | ethnicity | longitudinal

A controversy has developed over the relation of violent video game play and aggression (1–4). Whereas the majority of those who conduct research on this topic argue that playing such games increases aggressive behavior, a vocal minority has argued that the relation of game play and real-world aggressive behavior is at best overstated and at worst spurious. The controversy has had important real-world implications. In 2011, the US Supreme Court struck down a California statute designed to limit purchases and rentals of extremely violent video games by children (5). The majority opinion expressed skepticism about the importance of effects of violent video games, likening them to a "harmless pastime" (5).

Violent Video Game Play and Aggression

The case that violent video game play increases aggressive behavior has been made most forcefully by Anderson et al. (6; see also refs. 7 and 8). Specifically, these authors undertook a comprehensive metaanalysis of the literature on the impact of violent video game play on six categories of aggressive response: cognition, affect, arousal, empathy/sensitization to violence, overt aggressive behavior, and overt prosocial behavior. Their metaanalysis examined effects from over 130 research reports based on over 130,000 participants. On the basis of these analyses, the authors concluded that violent video game play is positively associated with aggressive behavior, aggressive cognition, and aggressive affect, as well as negatively associated with empathy for victims of violence and with prosocial behavior. Furthermore, the authors concluded that these effects are statistically reliable in experimental, cross-sectional, and longitudinal studies, are observed across cultures, gender, and game types (e.g., first vs. third person perspective; human vs. nonhuman targets; and so forth), and that methodologically superior studies tended to yield larger effects. A more recent metaanalysis by Greitemeyer and Mügge (9) came to similar conclusions.

Although hailed by some as conclusively demonstrating a link between violent video game play and aggression (7), the Anderson et al. (6) metaanalysis did not decrease skepticism among a vocal minority of researchers (10). In a wide range of articles, Ferguson (2, 11–16) has leveled four criticisms at research purporting to show that video game violence (VGV) increases real-world aggression: (*i*) many studies that support such a link use measures of "nonserious aggression" (e.g., accessibility of aggression related words, aggression related feelings) that inflate effect-size estimates; (*ii*) many studies do not include important covariates as statistical controls and hence any observed effects may be spurious consequences of third variable relationships; (*iii*) there is a bias to publish studies supporting a VGV → aggression link as opposed to those reporting a null effect; and (*iv*) even if one accepts the existence of a VGV → aggression relationship, the estimated effect size typically reported is exceedingly weak. Despite the fact that these arguments have been vigorously rebutted by Anderson and his colleagues (8), Ferguson and his colleagues have continued to stand by their critique (2, 15, 17, 18). With respect to the critiques raised by Ferguson et al. (19–21), it is noteworthy that these researchers have conducted three rigorous longitudinal studies that have found no significant relationship between violent video game play and aggression. They attribute these noneffects in part to: (*i*) using measures of "serious" aggression (e.g., overt physical aggression), and (*ii*) including appropriate control covariates.

Ethnicity and Game Play

Some evidence exists supporting the potential of ethnicity and culture to moderate VGV effects. Anderson et al. (6) noted in their metaanalysis of aggressive behavior in longitudinal designs that the VGV effect was somewhat larger in Western than Eastern cultures and this difference approached statistical significance ($P = 0.07$). At the same time, in these comparisons cultural differences were confounded with variation in research designs, such that "it was unclear whether the difference should be attributed to cultural differences in vulnerability or to the use of different measures" (6).

The potential for ethnicity to moderate the effects of video game exposure on aggression was corroborated by Ferguson (15) in his own recent metaanalysis. In that work, Ferguson found a statistically significant association between exposure to video

This paper results from the Arthur M. Sackler Colloquium of the National Academy of Sciences, "Digital Media and Developing Minds," held October 14–16, 2015, at the Arnold and Mabel Beckman Center of the National Academies of Sciences and Engineering in Irvine, CA. The complete program and video recordings of most presentations are available on the NAS website at www.nasonline.org/Digital_Media_and_Developing_Minds.

Author contributions: A.T.P., J.D.S., and J.G.H. designed research; A.T.P., J.D.S., and J.G.H. performed research; A.T.P. and J.G.H. analyzed data; and A.T.P., J.D.S., and J.G.H. wrote the paper.

The authors declare no conflict of interest.

This article is a PNAS Direct Submission.

Published under the PNAS license.

[1]To whom correspondence should be addressed. Email: jay.g.hull@dartmouth.edu.

Published online October 1, 2018.

games and aggressive behavior among studies that used Western samples, but this relationship was not significant among studies that used Asian or Hispanic samples. Because these metaanalytic findings were based on studies that measured exposure to all video games (rather than focusing on violent games), the results may not speak to questions about VGV effects per se, but they do support the view of ethnicity as a potential moderator of aggressive outcomes.

Metaanalysis of Longitudinal Research on VGV and Aggressive Behavior

The present review aims to address the four arguments outlined above that have been made against a relationship between VGV and aggression, and to reassess evidence for ethnicity as a moderator of this relationship. In reviewing the literature we focus on what we regard as providing the most stringent and appropriate test of the violent video game → aggression hypothesis: longitudinal designs that examine the association of violent video game play at one point in time with overt physical aggression at a subsequent point in time, while covarying prior aggression. By focusing on overt physical aggression, we avoid the criticism that other nonserious measures of aggression falsely inflate the effect size seen in the literature. By conducting a metaanalysis, we can estimate the average size, statistical reliability, and heterogeneity of effects in the literature. This allows us to examine the extent to which those estimates vary as a function of (*i*) the statistical covariates included by individual researchers and (*ii*) the culture/ethnicity of the participant. Finally, we looked for evidence of publication bias using a variety of methods.

Methods

Study Retrieval and Selection. We searched the electronic databases PsycInfo, PubMed, Web of Science, and ERIC using combinations of keywords associated with video game play (video gam* OR videogam* OR computer gam* OR electronic gam*), longitudinal designs (longitudinal OR prospective), and aggressive behavior (aggress* OR violen* OR delinquen*). The search included articles published up to April 1, 2017. Studies from any country were eligible for inclusion, and those published in languages other than English were eligible for inclusion as long as they could be translated into English. Articles, dissertations, and book chapters were eligible for inclusion regardless of whether they were published or unpublished.

To be eligible for inclusion in the metaanalysis, studies must have measured violent video game exposure and physical aggression at one point in time and measure physical aggression at least 3 wk later. Because the relationship of interest is specific to a subset of video games with violent or mature content, studies were excluded if they assessed total video game exposure (rather than exposure to violent or mature-rated games) or if they assessed exposure to violent movies or media other than video games. Only studies that measured real-world, overt physical aggression were included, based on the perspective that video game-induced changes in cognition (e.g., attitudes, attributional bias), emotion (e.g., hostility, emotional desensitization), feelings (e.g., empathic concern), and arousal are principally important insofar as they elucidate psychological processes that can serve as mediators for an established behavioral effect. Self-reports of real-world aggressive behavior were acceptable aggression measures, as were similar ratings provided by parents, teachers, or peers. Reports using hypothetical scenarios and reports restricted to verbal aggression were not considered acceptable measures. Finally, the search was restricted to longitudinal designs, given their strength in reducing the plausibility of reverse-causality. Although restricting the review to longitudinal studies of real-world, overt physical aggression does not preclude studies that use experimental designs, it does eliminate from consideration those laboratory-based experiments whose effects might be criticized as involving only temporary effects on behavior. Each set of authors for the resulting studies were contacted to inquire as to any information they might have regarding other published or unpublished longitudinal studies of video game play and aggression.

For all studies, the effect-size estimate used was the standardized regression coefficient associated with violent video game play and subsequent physical aggression, calculated while including prior aggression as a covariate. This estimate was preferred over a zero-order correlation because it better characterizes the relationship of interest, namely the link between violent video game exposure and subsequent change in aggression, which requires prior aggression to be taken into consideration. In addition, insofar as researchers included covariates beyond violent video game play and prior aggression in their originally published effects, we contacted each research team and requested that they supply us with the standardized regression coefficient associated with baseline violent video game play when used to predict subsequent physical aggression while covarying: (*i*) baseline physical aggression only and (*ii*) baseline physical aggression and gender.

Statistical Analysis. We estimated overall effects and heterogeneity in the effect sizes using both fixed-effects and random-effects metaanalytic modeling. We then tested whether some of the observed heterogeneity was predictable from three identifiable study characteristics: majority participant ethnicity, average participant age at study inception, and longitudinal time lag in measurement of aggression. Finally, we performed publication bias analyses described in detail below. We used both SPSS v20 and the R package "meta" (22) to conduct metaanalyses and publication bias analyses.

Results

Literature Search Results. Ultimately, our search yielded 24 studies (19–21, 23–40) (Table 1), of which only 5 appeared in the earlier metaanalysis by Anderson et al. (6) and 8 of which appeared in a more recent metaanalysis by Greitemeyer and Mügge (9). These studies included over 17,000 participants from a wide variety of countries (Austria, Canada, Germany, Japan, Malaysia, the Netherlands, Singapore, and the United States). Participants' average age ranged from 8.9 to 19.3 y, and the longitudinal time lag ranged from 3 mo to just over 4 y. The vast majority of these studies measured violent video game play and aggressive behavior at an initial point in time and then used both measures to predict subsequent aggressive behavior in a simultaneous regression analysis (or path analysis or structural equation model) while including a variety of control covariates. All studies measured exposure to violent video games rather than experimentally manipulating video game exposure.

Table 1 summarizes the major characteristics of these studies, including participant nationality and our categorization of the participants as representatives of three primary ethnicities: White, Hispanic, and Asian. In addition, the table includes a brief description of the physical aggression measure used, average age of participants at baseline, time lag to assessment of subsequent physical aggression, and effect-size estimates without covariates other than baseline aggression, with baseline aggression and gender, and with all covariates included in the original report.

Basic Analyses.

Effect-size estimates using only autoregressive lag as a covariate. For all but one of the datasets, we were able to obtain estimates of the standardized regression coefficient associating only initial violent video game play with subsequent physical aggression, covarying initial physical aggression (Table 1). A fixed-effects metaanalysis yielded an average coefficient of $\beta = 0.113$, 95% CI = (0.098, 0.128), $z = 14.815$, $P < 0.001$, and a Q statistic, $\chi^2(22) = 61.820$, $P < 0.001$, that indicated significant heterogeneity. A Hedges–Vevea random-effects metaanalysis yielded similar effect-size estimates, $\beta = 0.106$, 95% CI = (0.078, 0.134), $z = 7.462$, $P < 0.001$, and a Q statistic, $\chi^2(22) = 28.109$, $P = 0.172$, indicating nonsignificant heterogeneity.

Effect-size estimates using autoregressive lag plus covariates. Subsequent analyses were conducted that involved estimates adjusted for all covariates used in the 24 originally reported results. A majority of studies reported positive estimates indicating that violent video game play was associated with increases over time in physical aggression controlling for prior aggression and all other covariates.

A fixed-effects metaanalysis yielded an average coefficient of $\beta = 0.080$, 95% CI = (0.065, 0.094), $z = 10.387$, $P < 0.001$, and a Q statistic, $\chi^2(23) = 50.556$, $P = 0.001$ (indicating significant heterogeneity). A Hedges–Vevea random-effects analysis yielded similar effect-size estimates, $\beta = 0.078$, 95% CI = (0.053, 0.102), $z = 6.173$, $P < 0.001$, and a Q statistic, $\chi^2(23) = 27.404$, $P = 0.239$, indicating nonsignificant heterogeneity. (Results from the analyses that included both the autoregressive lag and gender as covariates

Table 1. Longitudinal studies on VGV and aggression

								Covariates other than Initial Aggression		
Authors	Year	Nationality	Principal ethnicity	Physical aggression measure	n	Average age T1*	Lag (years)	None	Gender	All
Adachi and Willoughby (23)	2016	Canadian	White	Direct aggression (physical and verbal)	1,132	19.1	1.0	0.136	0.077	0.076
Anderson et al. (24)[†]	2008	Japanese	Asian	Trait physical aggression scale	181	∼13.5	0.3	0.144	0.139	0.139
Anderson et al. (24)[†]	2008	Japanese	Asian	Physical aggression in past month	1,050	∼15.5	0.3–0.5	0.115	0.075	0.075
Anderson et al. (24)[†]	2008	American	White	Index of teacher, peer, and self-reports, current school year	364	∼10.5	0.5	0.167	0.158	0.158
Breuer et al. (25)	2015	German	White	Buss & Perry Aggression Questionnaire (physical, two items)	140	16	1.0	−0.151	−0.159	−0.159
Breuer et al. (25)	2015	German	White	Buss & Perry Aggression Questionnaire (physical, two items)	136	19.3	1.0	0.078	0.070	0.070
Bucolo (26)	2010	American	White	Buss & Perry Aggression Questionnaire (physical, five items)	648	13.4	1.5	0.17	0.15	0.14
Ferguson (19)[‡]	2011	American	Hispanic	Child Behavior Checklist Youth Self-Report, aggression, child (YSRac)	302	12.3	1.0	0.035	0.011	−0.030
Ferguson et al. (21)[‡]	2012	American	Hispanic	Child Behavior Checklist Youth Self-Report, aggression, child (YSRac)	165	12.3	3.0	−0.068	−0.016	0.030
Ferguson et al. (20)[‡]	2013	American	Hispanic	Child Behavior Checklist Youth Self-Report, aggression, child (YSRac)	143	12.8	1.0	0.069	0.044	0.100
Fikkers et al. (27)	2016	Dutch	White	Physical aggression	943	11.8	1.0	0.180	0.126	0.126
Gentile et al. (28)	2009	American	White	Self-reported fights, teacher rating of physical aggression	865	9.6	1.1	0.112	0.089	0.089
Gentile et al. (29)	2014	Singapore	Asian	Six items assessing physical aggression	2,029	12.2	1.0	0.065	0.043	0.043
Greitemeyer and Sagiogluo (30)	2017	American	White	Buss & Perry Aggression Questionnaire (physical, two items)	743		0.5	0.032	0.024	0.021
Hirtenlehner and Strohmeier (31)	2015	Austrian	White	Personal violence	371	11.5	1.0	0.190	0.13	0.140
Hopf, et al. (32)	2008	German	White	Student's' violence	314	12	2.7	—[§]	—[§]	0.18
Hull et al. (33)	2014	American	White	Hitting nonfamily members, sent to school office for fighting	2,723	13.8	0.8	0.097	0.088	0.075
Subsample 1			White		1,831			0.103	0.100	0.085
Subsample 2			Hispanic		442			0.062	0.034	0.024
Subsample 3			Asian		49			−0.098	−0.097	−0.040
Krahé et al. (34)[‡]	2012	German	White	Self-reported (five items) and teacher-reported (one item) physical aggression	1,715	13.4	1.1	0.18	0.15	0.15
Lemmens et al. (35)[‡]	2011	Dutch	White	Buss & Perry Aggression Questionnaire (physical, seven items)	540	13.9	0.5	0.09	—[§]	0.09
Möller and Krahé (36)[†,‡]	2009	German	White	Buss & Perry Aggression Questionnaire (physical, seven items)	143	13.3	2.5	0.275	0.213	0.213
Shibuya et al. (37)[†]	2008	Japanese	Asian	Buss & Perry Aggression Questionnaire (physical, six items)	498	∼10.5	0.9	0.072	−0.001[¶]	−0.001
Staude-Müller (38)	2011	German	White	"Aggression-tilt"	472	13.7	1.0	0.046	0.028	−0.020
von Salisch et al. (39)[‡]	2011	German	White	Peer nomination, teacher rating: Latent variable	228	8.9	1.0	−0.021	−0.031	−0.010
Willoughby et al. (40)[‡]	2012	Canadian	White	Direct aggression (overt). Effect relates sustained violent video game play 9–12 with aggressive slope	1,492	13.8	4.0	0.164	0.123	0.070

Note: von Salisch et al. (39) used only peer nominations and teacher ratings to measure aggression; all other studies included self-reported measurements of aggression.
*Age at study onset; Approximate ages (∼) estimated from reported age ranges and/or grade levels.
[†]Appears in metaanalysis by Anderson et al. (6).
[‡]Appears in metaanalysis by Greitemeyer and Mügge (9).
[§]Did not use additional control covariates or effect not reported.
[¶]Interaction of gameplay and moderator variable statistically significant at $P < 0.05$.

fell between the estimates from these two analyses. They are available from the authors upon request.)

Publication bias. We conducted three analyses to assess possible publication bias, none of which found evidence for overestimation of the effect in the literature. Rosenthal Fail-Safe n estimates indicated that in excess of 700 null findings would be necessary to jeopardize the conclusion that a positive longitudinal relation exists between violent video game play and physical aggression (estimates using only the aggression autoregressive lag covariate, Fail-Safe $n = 1{,}334$; estimates using all covariates, Fail-Safe $n = 723$). The Begg and Mazumdar (41) rank correlation $\tau\text{-}b$ was nonsignificant for both the random-effects model that only included the autoregressive lag of prior aggression, $\tau\text{-}b = -0.269$, $P = 0.072$, and the model that included all covariates, $\tau\text{-}b = -0.033$, $P = 0.823$. Finally, a trim and fill analysis (42, 43) applied to these data did not add any effects to the distribution, once again indicating lack of publication bias.

Moderator Analyses. To explore potential moderators of these observed effects, we examined variation in effect-size estimates associated with three study characteristics: participant ethnicity, age, and time lag between measurements of aggression.

Ethnicity. Moderator analyses were conducted to test for variation in effect sizes as a function of participant ethnicity. In all but one case, studies were categorized based on the predominant ethnicity of the sample: White, Hispanic, or Asian (Table 1). In the case of the study by Hull et al. (33) it was possible to calculate effect sizes separately for each of these ethnic categories based

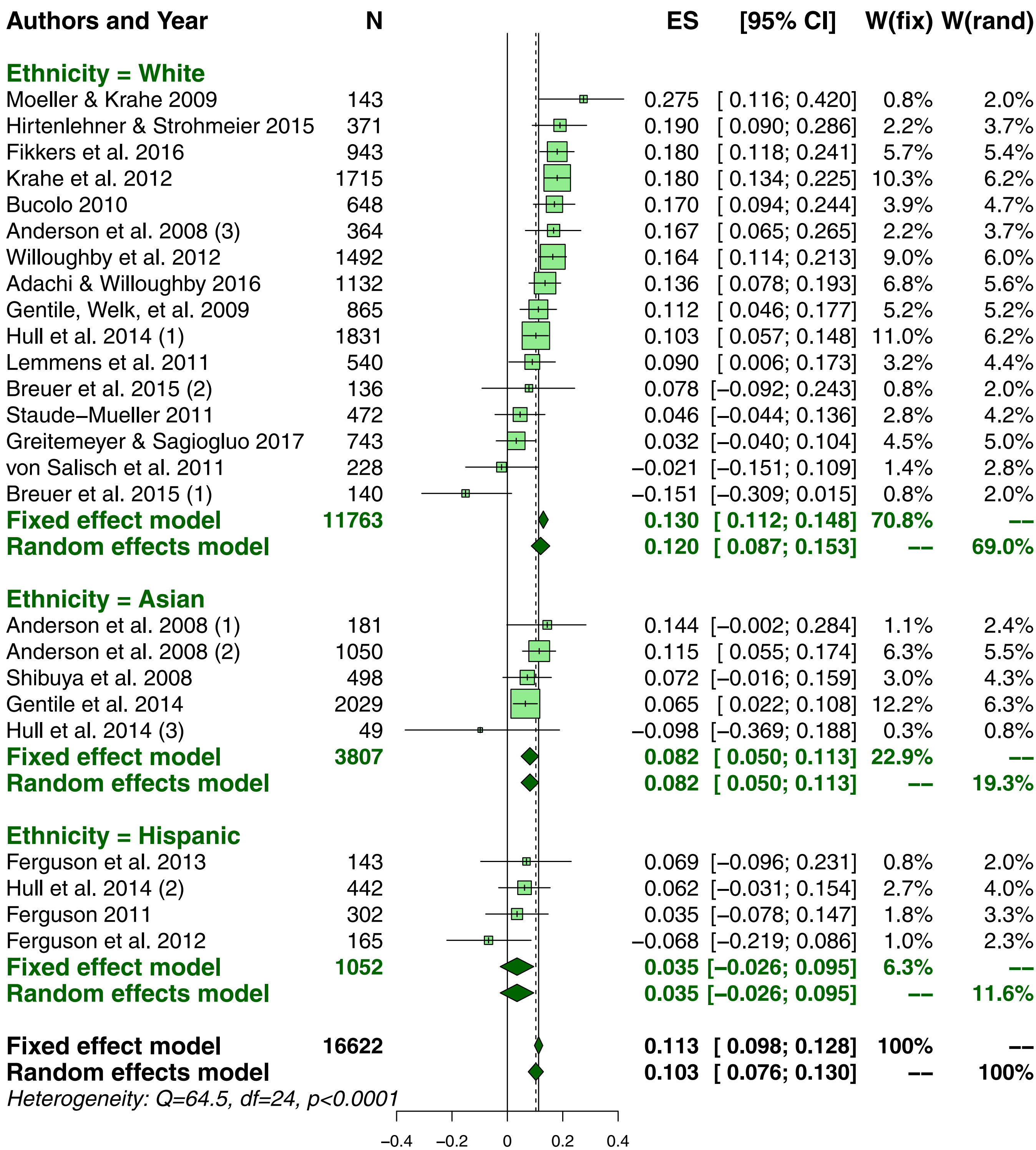

Fig. 1. Standardized regression coefficients (β) associating baseline violent video game play with subsequent physical aggression including an autoregressive lag for aggression and based on data selected for ethnicity moderator analyses. Estimated effect size β (ES; square) and 95% confidence interval (CI; lines) are displayed for all effects entered into the metaanalysis (19–21, 23–31, 33–40). Diamonds represent metaanalytically weighted mean β. Weight percentages for fixed-effects and random-effects models are labeled W(fix) and W(rand), respectively. For studies with multiple independent samples, the result for each sample is reported separately and numbered 1, 2, or 3.

on each participant's self-identification. Although all other analyses used the overall effect-size estimates from the Hull et al. total sample (n = 2,723), analyses testing the moderating effect of ethnicity instead involved the specific effect sizes associated with each of the three Hull et al. subsamples: White (n = 1,831), Hispanic (n = 442), and Asian/Pacific Islander (n = 49).

A fixed-effects moderator analysis using the three ethnic categories in Table 1 applied to the "autoregressive lag only" estimates yielded a significant moderator effect, $\chi^2(2) = 13.658$, $P = 0.001$.

Separate analyses indicated that the effect was largest among White participants, intermediate among Asian participants, and smallest among Hispanic participants (see Fig. 1 for estimates within each group, in addition to overall estimates based on these study samples). The fixed-effects moderator analysis using two ethnic categories of Hispanic vs. non-Hispanic also yielded a significant moderator effect, $\chi^2(1) = 6.820$, $P = 0.009$. Both the random-effects moderator comparison of three ethnicities and the random-effects comparison of Hispanic vs. non-Hispanic samples approached significance, [$\chi^2(2) = 5.125$, $P = 0.077$, and $\chi^2(1) = 3.745$, $P = 0.053$, respectively].

A fixed-effects moderator analysis using three ethnic categories applied to the "all covariates" estimates yielded a significant moderator effect, $\chi^2(2) = 9.059$, $P = 0.011$, of the same form as observed previously. In this case, neither the random-effects moderator comparison of three ethnicities, $\chi^2(2) = 3.915$, $P = 0.141$, nor the Hispanic vs. non-Hispanic comparison, χ^2 (1) = 2.280, $P = 0.131$, achieved statistical significance.

Time lag. A fixed-effects moderator analysis using three time-lag categories (less than 1 y, 1 y, more than 1 y) applied to the "autoregressive lag only" estimates yielded a significant moderator effect, $\chi^2(2) = 14.218$, $P < 0.001$. Separate analyses indicated that the effect was largest in the studies with a lag of longer than 1 y, $\beta = 0.157$, 95% CI = (0.130, 0.184), $z = 11.220$, $P < 0.001$, and smaller in studies with a lag equal to 1 y, $\beta = 0.094$, 95% CI = (0.069, 0.120), $z = 7.243$, $P < 0.001$, or less than 1 y, $\beta = 0.095$, 95% CI = (0.070, 0.120), $z = 7.441$, $P < 0.001$. A random-effects moderator analysis did not achieve conventional levels of significance, $\chi^2(2) = 4.001$, $P = 0.135$.

Age. A fixed-effects moderator analysis using two age categories (age 12 and younger, age 13 and older) yielded a moderator effect that approached significance, $\chi^2(1) = 3.788$, $P = 0.052$. Separate analyses indicated that the effect was slightly larger in studies that examined effects among older children, $\beta = 0.128$, 95% CI = (0.109, 0.147), $z = 13.119$, $P < 0.001$, than those with younger children, $\beta = 0.097$, 95% CI = (0.072, 0.122), $z = 7.456$, $P < 0.001$. A random-effects moderator analysis did not achieve conventional levels of significance, $\chi^2(1) = 0.982$, $P = 0.322$.

Discussion

Researchers have been divided with respect to the question of whether or not playing violent video games is associated with subsequent increases in physical aggression. Although a majority of researchers have argued for such an association, a vocal minority has claimed that existing evidence is flawed in multiple respects. Our results speak to three of the four specific criticisms of this literature outlined previously.

First, to address the criticism that many existing studies used "nonserious" measures of aggression (e.g., aggressive cognitions or affect), we limited our metaanalysis to studies that measured changes in overt, physical aggression over the course of months or years. Our results demonstrated a reliable metaanalytic effect in longitudinal studies even when controlling for baseline levels of physical aggression, suggesting that the effects of violent video games extend to meaningful behaviors in the real world.

Second, to address arguments that estimates of this effect were spurious based on a failure to include adequate statistical controls, we conducted our analyses first with baseline aggression as the sole covariate and again with all covariates originally included in each study. Results showed that inclusion of covariates appears to have only a minor impact on the estimated association of game play and aggression. Indeed, for two of the three studies reported by Ferguson et al. (20, 21), inclusion of their preferred covariates slightly increased the size of the association (Table 1).

Third, whereas existing metaanalyses have been criticized as failing to take into account the potential for publication bias, we observed no evidence that studies with null or negative effect sizes have been underrepresented in the literature, despite using three different analytical approaches to assess publication bias. Importantly, the analytical approaches used to arrive at this conclusion have been demonstrated to possess complementary qualities: the trim-and-fill technique has high statistical power but a high type I error rate, whereas the Begg and Mazumdar's rank correlation test has lower power but yields virtually no type I errors (44). The fact that both of these tests reach the same conclusion suggests the results are reliable.

With respect to the fourth criticism, focused on the size of these effects, our metaanalysis yielded a modest effect size of ≈0.11 when additional covariates were not included. Ferguson and his colleagues have noted that a regression coefficient of 0.10 is associated with only 1% of the variance in the outcome and concluded that this is so small as to be meaningless. However, others countered that squared regression coefficients provide a less-appropriate metric for judging the practical significance of effects compared with estimates of relative risk (1, 45). In fact, Rosenthal (45) argued that reliance on r^2 values to interpret effect sizes is particularly problematic in the context of studying antisocial behaviors, such as aggression, stating "our ability to predict and control antisocial behavior is not at all trivial in practical terms, despite the apparently small r^2s obtained in most studies" (45). Regardless of one's subjective definition of a meaningful effect size, it is clear that a statistically significant, reliable effect exists in the literature.

Although our study supports a skeptical view of aforementioned criticisms of the literature on VGV and aggression, our results offer a possible alternative explanation for the differing conclusions reached by researchers on opposite sides of the debate. Specifically, we found evidence that the effect of VGV on aggression is moderated by sample ethnicity, with White participants showing the strongest effect and Hispanic participants showing no significant effects. Effects for Asian participants fell between those for the other two groups.

The possibility that the effects of violent video games on aggression are moderated by ethnicity was raised in a previous metaanalysis by Anderson et al. (6) that included both Western and Asian (but not Hispanic) samples. At the same time, these authors found that: (*i*) the moderating effect of ethnicity only approached conventional levels of significance and (*ii*) could not be disentangled from variation in research methodology. A subsequent metaanalysis by Ferguson (15) replicated and extended this finding by showing that video game effects were present among Western but not Asian or Hispanic samples. However, because those analyses involved studies of all design types (including nonlongitudinal) and did not take into account the type of game (violent vs. nonviolent) in the studies' video game exposure measurements, the results do not speak directly to the question of VGV effects over time.

In contrast, the present metaanalysis focused specifically on studies of violent video game exposure that used longitudinal designs and expanded upon the findings by Anderson et al. (6) by including many longitudinal studies published since and by distinguishing Hispanic in addition to White and Asian samples. Our results showed a statistically significant moderation effect of ethnicity (albeit using fixed-effects estimates), such that the strongest association was observed among White samples, an intermediate association for Asian samples, and a small, nonsignificant association for Hispanic samples. That said, given the small number of studies with Hispanic samples, more studies of this population are clearly needed before making firm conclusions about the effect of violent games on this group.

Even if differences between ethnic groups are established, the question remains as to why ethnicity might moderate the influence of violent video games on aggressive behavior. Anderson et al. (6) elaborated five reasons to expect smaller media effect sizes in Eastern than Western societies. Specifically, they discuss

COLLOQUIUM PAPER

PSYCHOLOGICAL AND COGNITIVE SCIENCES

cross-cultural differences in: (*i*) how violence is contextualized in the media; (*ii*) the extent to which individuals attend to the situational context of action; (*iii*) the meaning, experience, and processing of emotions; (*iv*) the public–private context in which video games are typically played; and (*v*) the social networks of gamers. To these reasons, we would add variation across cultures in the meaning of being a perpetrator and a victim of aggression. From this perspective, cultures that promote social responsibility and empathy toward victims of violence may decrease the effects of violent game play by leading individuals to psychologically distance themselves from their virtual aggression and from its implications for their personal values and real-world behavior. Conversely, cultures that promote rugged individualism and a warrior-like mentality may lead individuals to identify with the role of aggressor and dampen sympathy toward their virtual victims, with consequences for their values and behavior outside the game.

With respect to such an account of the ethnicity-based moderation of the effect of VGV on aggression observed in the current metaanalysis, Anderson et al. (6) found that culture moderated the impact of violent video game play on desensitization to violence and empathy such that participants from Western cultures showed greater desensitization and larger decreases in empathy than those from Eastern cultures. Findings by Ramos et al. (46) suggest that, similar to those from Eastern cultures, Hispanic participants appear to maintain empathy for victims in the face of media depictions of violence. With respect to desensitization and decreased empathy being a cause of the impact of VGV on subsequent aggression, Bartholow et al. (47) found that empathy mediated the impact of VGV on aggression in an experimental design. At the same time, whereas empathy for the victim of VGV may decrease subsequent aggression, empathy for perpetrators may actually increase subsequent aggression by motivating justification of their actions (e.g., refs. 48 and 49). Obviously, although our account is consistent with a variety of empirical findings, additional research is necessary to establish empathy as a plausible mediator of the observed moderating influence of ethnicity on aggression in the current metaanalysis.

Conclusion

On the basis of this metaanalysis, we conclude that playing violent video games is associated with greater levels of overt physical aggression over time, after accounting for prior aggression. These findings support the general claim that violent video game play is associated with increases in physical aggression over time. Furthermore, the results speak to three specific criticisms of this literature by demonstrating: (*i*) that violent video game play is associated with increases in measures of serious aggressive behavior (i.e., overt, physical aggression), (*ii*) that estimates of this effect are only slightly decreased by inclusion of statistical covariates, and (*iii*) by finding no evidence of publication bias.

Results further suggest the VGV effect on aggression may be moderated by sample ethnicity such that it is most strongly observed among White participants, less strongly but reliably observed among Asian participants, and unreliably among Hispanic participants. In addition, designs that involve longer time lags appear to be associated with larger effects, a finding consistent with observations in multiwave studies (e.g., ref. 33).

In sum, the results of our metaanalysis pose serious challenges to several major criticisms of the literature linking VGV and physical aggression, and they offer a simple explanation for the inconsistent findings by researchers on opposing sides of the debate. We hope these findings will assist the field in moving past the question of whether violent video games increase aggressive behavior, and toward questions regarding why, when, and for whom they have such effects.

1. Bushman BJ, Huesmann LR (2014) Twenty-five years of research on violence in digital games and aggression revisited: A reply to Elson and Ferguson (2013). *Eur Psychol* 19: 47–55.
2. Elson M, Ferguson CJ (2014) Twenty-five years of research on violence in digital games and aggression: Empirical evidence, perspectives, and a debate gone astray. *Eur Psychol* 19:33–46.
3. Krahé B (2014) Restoring the spirit of fair play in the debate about violent video games: A comment on Elson and Ferguson (2013). *Eur Psychol* 19:56–59.
4. Warburton W (2014) Apples, oranges, and the burden of proof—Putting media violence findings Into context: A comment on Elson and Ferguson (2013). *Eur Psychol* 19: 60–67.
5. Brown v. Entertainment Merchants Association, 564 US 786 (2011).
6. Anderson CA, et al. (2010) Violent video game effects on aggression, empathy, and prosocial behavior in eastern and western countries: A meta-analytic review. *Psychol Bull* 136:151–173.
7. Huesmann LR (2010) Nailing the coffin shut on doubts that violent video games stimulate aggression: Comment on Anderson et al. (2010). *Psychol Bull* 136:179–181.
8. Bushman BJ, Rothstein HR, Anderson CA (2010) Much ado about something: Violent video game effects and a school of red herring: Reply to Ferguson and Kilburn (2010). *Psychol Bull* 136:182–187.
9. Greitemeyer T, Mügge DO (2014) Video games do affect social outcomes: A meta-analytic review of the effects of violent and prosocial video game play. *Pers Soc Psychol Bull* 40:578–589.
10. Ferguson CJ, Kilburn J (2010) Much ado about nothing: The misestimation and overinterpretation of violent video game effects in eastern and western nations: Comment on Anderson et al. (2010). *Psychol Bull* 136:174–178, discussion 182–187.
11. Ferguson CJ (2007) Evidence for publication bias in video game violence effects: A meta-analytic review. *Aggress Violent Behav* 12:470–482.
12. Ferguson CJ (2008) The school shooting/violent video game link: Causal relationship or moral panic? *J Invest Psychol Offender Profiling* 5:25–37.
13. Ferguson CJ (2009) Research on the effects of violent video games: A critical analysis. *Soc Personal Psychol Compass* 3:351–364.
14. Ferguson CJ (2010) Blazing angels or resident evil? Can violent video games be a force for good? *Rev Gen Psychol* 14:68–81.
15. Ferguson CJ (2015) Do angry birds make for angry children? A meta-analysis of video game influences on children's and adolescents' aggression, mental health, prosocial behavior, and academic performance. *Perspect Psychol Sci* 10:646–666.
16. Ferguson CJ, Kilburn J (2009) The public health risks of media violence: A meta-analytic review. *J Pediatr* 154:759–763.
17. Ferguson CJ (2015) Pay no attention to that data behind the curtain: On angry birds, happy children, scholarly squabbles, publication bias, and why betas rule metas. *Perspect Psychol Sci* 10:683–691.
18. Ferguson CJ; Entertainment Merchants Association (2013) Violent video games and the Supreme Court: Lessons for the scientific community in the wake of Brown v. Entertainment Merchant's Association. *Am Psychol* 68:57–74.
19. Ferguson CJ (2011) Video games and youth violence: A prospective analysis in adolescents. *J Youth Adolesc* 40:377–391.
20. Ferguson CJ, Garza A, Jerabeck J, Ramos R, Galindo M (2013) Not worth the fuss after all? Cross-sectional and prospective data on violent video game influences on aggression, visuospatial cognition and mathematics ability in a sample of youth. *J Youth Adolesc* 42:109–122.
21. Ferguson CJ, San Miguel C, Garza A, Jerabeck JM (2012) A longitudinal test of video game violence influences on dating and aggression: A 3-year longitudinal study of adolescents. *J Psychiatr Res* 46:141–146.
22. Schwarzer G (2010) *meta: Meta-Analysis with R*. Available at cran.r-project.org/package=meta. Accessed July 5, 2017.
23. Adachi PJC, Willoughby T (2016) The longitudinal association between competitive video game play and aggression among adolescents and young adults. *Child Dev* 87: 1877–1892.
24. Anderson CA, et al. (2008) Longitudinal effects of violent video games on aggression in Japan and the United States. *Pediatrics* 122:e1067–e1072.
25. Breuer J, Vogelgesang J, Quandt T, Festl R (2015) Violent video games and physical aggression: Evidence for a selection effect among adolescents. *Psychol Pop Media Cult* 4:305–328.
26. Bucolo D (2010) Violent video game exposure and physical aggression in adolescence: Tests of the general aggression model. PhD dissertation (University of New Hampshire, Durham, NH).
27. Fikkers KM, Piotrowski JT, Lugtig P, Valkenburg PM (2016) The role of perceived peer norms in the relationship between media violence exposure and adolescents' aggression. *Media Psychol* 19:4–26.
28. Gentile DA, et al. (2009) Evaluation of a multiple ecological level child obesity prevention program: Switch what you do, view, and chew. *BMC Med* 7:49.
29. Gentile DA, Li D, Khoo A, Prot S, Anderson CA (2014) Mediators and moderators of long-term effects of violent video games on aggressive behavior: Practice, thinking, and action. *JAMA Pediatr* 168:450–457.
30. Greitemeyer T, Sagioglou C (2017) The longitudinal relationship between everyday sadism and the amount of violent video game play. *Pers Individ Dif* 104:238–242.
31. Hirtenlehner H, Strohmeier D (2015) Does playing violent video games lead to more criminal violence among adolescents? *Monatsschr Kriminol* 98:444–463.

32. Hopf WH, Huber GL, Weiß RH (2008) Media violence and youth violence: A 2-year longitudinal study. *J Media Psychol* 20:79–96.
33. Hull JG, Brunelle TJ, Prescott AT, Sargent JD (2014) A longitudinal study of risk-glorifying video games and behavioral deviance. *J Pers Soc Psychol* 107:300–325.
34. Krahé B, Busching R, Möller I (2012) Media violence use and aggression among German adolescents: Associations and trajectories of change in a three-wave longitudinal study. *Psychol Pop Media Cult* 1:152–166.
35. Lemmens JS, Valkenburg PM, Peter J (2011) The effects of pathological gaming on aggressive behavior. *J Youth Adolesc* 40:38–47.
36. Möller I, Krahé B (2009) Exposure to violent video games and aggression in German adolescents: A longitudinal analysis. *Aggress Behav* 35:75–89.
37. Shibuya A, Sakamoto A, Ihori N, Yukawa S (2008) The effects of the presence and contexts of video game violence on children: A longitudinal study in Japan. *Simul Gaming* 39:528–539.
38. Staude-Müller F (2011) Computerspielgewalt und aggression: Längsschnittliche untersuchung von selektions- und wirkungseffekten. *Prax Kinderpsychol Kinderpsychiatr* 60:745–761.
39. von Salisch M, Vogelgesang J, Kristen A, Oppl C (2011) Preference for violent electronic games and aggressive behavior among children: The beginning of the downward spiral? *Media Psychol* 14:233–258.
40. Willoughby T, Adachi PJC, Good M (2012) A longitudinal study of the association between violent video game play and aggression among adolescents. *Dev Psychol* 48: 1044–1057.
41. Begg CB, Mazumdar M (1994) Operating characteristics of a rank correlation test for publication bias. *Biometrics* 50:1088–1101.
42. Duval S, Tweedie R (2000) A nonparametric "trim and fill" method of accounting for publication bias in meta-analysis. *J Am Stat Assoc* 95:89–98.
43. Duval S, Tweedie R (2000) Trim and fill: A simple funnel-plot-based method of testing and adjusting for publication bias in meta-analysis. *Biometrics* 56:455–463.
44. Ruzni N, Idris N (2012) A comparison of methods to detect publication bias for meta-analysis of continuous data. *J Appl Sci* 12:1413–1417.
45. Rosenthal R (1986) Media violence, antisocial behavior, and the social consequences of small effects. *J Soc Issues* 42:141–154.
46. Ramos RA, Ferguson CJ, Frailing K, Romero-Ramirez M (2013) Comfortably numb or just yet another movie? Media violence exposure does not reduce viewer empathy for victims of real violence among primarily Hispanic viewers. *Psychol Pop Media Cult* 2: 2–10.
47. Bartholow BD, Sestir MA, Davis EB (2005) Correlates and consequences of exposure to video game violence: Hostile personality, empathy, and aggressive behavior. *Pers Soc Psychol Bull* 31:1573–1586.
48. Happ C, Melzer A, Steffgen G (2013) Superman vs. BAD man? The effects of empathy and game character in violent video games. *Cyberpsychol Behav Soc Netw* 16: 774–778.
49. Happ C, Melzer A, Steffgen G (2015) Like the good or bad guy—Empathy in antisocial and prosocial games. *Psychol Pop Media Cult* 4:80–96.