The Public Health Risks of Media Violence: A Meta-Analytic Review

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Objective   To conduct a meta-analytic review of studies that examine the impact of violent media on aggressive behavior and to determine whether this effect could be explained through methodological problems inherent in this research field.

Study design   A detailed literature search identified peer-reviewed articles addressing media violence effects. Effect sizes were calculated for all studies. Effect sizes were adjusted for observed publication bias.

Results   Publication bias was a problem for studies of aggressive behavior, and methodological problems such as the use of poor aggression measures inflated effect size. Once corrected for publication bias, studies of media violence effects provided little support for the hypothesis that media violence is associated with higher aggression. The corrected overall effect size for all studies was r = .08.

Conclusions   Results from the current analysis do not support the conclusion that media violence leads to aggressive behavior. It cannot be concluded at this time that media violence presents a significant public health risk. *(J Pediatr 2009;154:759-63)*

Recent reviews place the potential health risks of exposure to media violence on par with smoking cigarettes.[1,2] If this were correct, it would represent one of the most significant public health findings of modern times. However, other reviews have argued that significant and systemic methodological problems with the media violence literature, including use of invalid and unreliable measures and failure to control for mediating "third" variables such as personality, genetics, or even sex, greatly impair conclusions based on this research.[3-7] This meta-analysis attempts to improve on earlier work by examining whether methodological weaknesses may explain the proposed link between media violence and viewer aggression.

Meta-analysis can provide a valuable tool for examining the assumptions that underlie the critical view of media violence research, which have not been tested in previous meta-analyses.[8-13] Particularly, the critical perspective hinges on several hypotheses that can be tested. These include: 1) Aggression outcome measures that are unstandardized or unreliable will produce higher effects than established standardized and reliable measures; 2) Aggression measures with poor validity will produce higher effect sizes than those with well-established validity; 3) Effect sizes for outcomes will decrease the closer that outcomes approximate actual physical aggression or violent criminal behavior; and 4) Controlling for "third variables" such as family violence exposure or personality will reduce the observed effect size for results. This last hypothesis relates to the argument raised by some authors[3-7,14] that media violence effects are a symptom of other underlying problems, not a cause of aggressive behavior. For example, individuals genetically prone to aggression or exposed to family violence exposure may have inflated violence risk, while also consuming more violent media. Male individuals are, on average, more aggressive and more likely to consume violent media. Failing to control for sex, particularly in bivariate correlations, may produce artificially inflated effect size estimates.[3-7]

METHODS

Study Selection and Categorization

PsycINFO was searched for all articles published between the years of 1998 and early 2008 that included the following search terms: (video* or computer or arcade or televise* or media or comic* or movie or music) and (attack or fight* or aggress* or violen*). The authors of one of the most recent meta-analysis of general media violence effects[9] provided a reference to their included studies by personal communication, and this was examined for further studies. We also examined the reference sections of several other recent meta-analyses[10-13] for studies that might otherwise have been missed.

Several criteria were used to maximize the homogeneity of the included studies. They were judged relevant when they met the following criteria. First, articles had to have

CMA   Comprehensive Meta-Analysis

been published between the years of 1998 and 2008. Most earlier meta-analyses (eg, 8-10) considered a wide span of research from past decades. However this allows older research, potentially with less valid methods, to "pollute" more recent and perhaps more valid research. Second, articles had to examine the effect of violent media on some measure of aggressive behavior. Acceptable measures included behavioral reports of aggressive or violent incidents, trained rater reports of aggressive or violent behaviors, clinically validated aggression measures, and laboratory paradigms of aggressive behavior. Finally, because this study included an analysis of publication bias in peer-reviewed journals, only articles published in peer-reviewed journals were included in the analysis. Including only published studies is a common procedure in clinical meta-analyses and helps to ensure the homogeneity of the included studies. Earlier reviews have found that attempting to include non-published studies is highly problematic, because parties with a vested ideological interest in the outcome may select which studies to send along, and such data has not been peer-reviewed.[15,16]

Both authors independently rated each study for inclusion. Kappa reliability for inclusion decisions was $r_k = 1.00$. A total of 25 studies comprising 27 independent observations were found that met the aforementioned criteria, including a total sample size of 12 436. A table of included studies is available on request to the first author.

Effect Size Calculation

Pearson r, a flexible and easily interpreted index of effect size, was used as the effect size estimate in this study. Correlation coefficients were transformed to Fisher z, weighted, averaged, and transformed back to a pooled r, denoted r_+. In the case in which a study reported nonsignificant results but failed to provide statistical information (eg, F-value), the effect size was calculated by using the provided means and SDs. In the event of multiple measures for the same construct occurring within a study (ie, multiple dependent or independent measures), simple mean correlations were computed. In studies in which both univariate (eg, bivariate correlations) and multivariate (eg, partial correlations) were available, only the latter were included in the meta-analysis, because this provided a better indices of the unique shared variance between violent media exposure and aggression (as opposed to that caused by sex, trait aggression, etc.).

Statistical and Publication Bias Analyses

The Comprehensive Meta-Analysis (CMA) software program was used to fit both random and fixed effects models. CMA provides a highly automated and user-friendly program for the computation of meta-analyses, with options for using multiple effect size indicators. CMA is a relatively new analytical tool with its own set of assumptions. Procedures and assumptions are consistent with current approaches.[23] In accordance with recommendations of Hunter and Schmidt,[17]

random effects models were used. Publication bias estimates are based primarily on funnel plot effects, namely examining whether larger effects are necessary for smaller sample studies to become published (this indicates that "statistically significant" reports are more likely to be published than null reports). General agreement of 6 publication bias measures was considered to be evidence for or against publication bias. Ferguson[11] discusses these publication bias analyses in some detail, although they are discussed in brief here. First is the Fail-safe N. This technique involves computing a combined P value for all the studies included in the meta-analysis and calculating how many additional studies with a zero effect (average z of 0) would be necessary to create a nonsignificant P value. Second is the Orwin fail-safe N, an alternate formula for calculating the number of studies necessary to bring the effect size down to trivial levels (eg, r < .10). Third, Begg and Mazumdar's rank correlation test provides a rank-correlation for the relationship between effect size and the standard errors of the effects. Fourth, Egger Regression attempts to quantify the bias captured in the funnel plot. Finally is the Duvall and Tweedie Trim and Fill. This iterative procedure provides an estimate of what the effect size would be if there was no publication bias in the meta-analysis.

Both effects sizes corrected for publication bias, and uncorrected effect sizes are presented, although corrected effect sizes are better indices of actual effects.

RESULTS

Descriptive Study Results

Of the independent observations included in this analysis, 12 (44%) were correlational in nature, 10 (37%) were experimental, and 5 (19%) were longitudinal. For age range, 16 studies (59%) were conducted with children, and the remaining 11 studies (41%) were conducted with adults.

For the type of medium examined, 15 studies (55%) looked at the effects of video games, whereas 7 studies (26%) examined television specifically, and the remaining 5 studies (19%) examined either movies alone or mixed media.

In this analysis, 18 individual study observations (67%) used aggression measures that were standardized and provided reliability data. The remaining 9 studies (33%) used unstandardized measures of aggression or provided no reliability data. However, only 11 studies (41%) used aggression measures that had been well validated for actual aggression. Well-validated measures included those that directly measured physically or verbally aggressive behaviors toward another person (hitting, kicking, shouting, insults, etc.), data on violent crime (societal violent crime perpetration or victimization rates), or clinical scales with empirically demonstrated validity coefficients related to real-world physical aggression or violence. Poorly validated measures included those that were unstandardized, provided no reliability or validity data, or have been previously identified as "ad hoc" measures lacking generalizability to actual physical aggression and vio-

Table. Meta-analytic results for main analysis and moderator variables, including publication bias analysis

Effect sizes	k	r_+	r_u	95% CI	Homogenity test	FSN	OFSN	RCT	RT	Bias?
All observations	27	.08	.14	.03-.13	$X^2(26) = 138.89, P \leq .001$	937	1	$P < .01$	$P < .05$	Yes
Outcome type										
Proxy*	10	.25	.25	.11-.38	$X^2(9) = 49.00, P \leq .001$	159	15	$P < .05$	NS	No
Aggression	14	.08	.10	.03-.13	$X^2(13) = 45.57, P \leq .001$	221	0	NS	NS	Inc
Violence	3	.02	.07	−.12-.16	$X^2(2) = 19.36, P \leq .001$	5	0	NS	NS	Inc
Controls third variables										
No	13	.09	.21	−.02-.18	$X^2(12) = 104.31, P \leq .001$	314	3	$P < .06$	$P < .06$	Yes
Yes	14	.08	.10	.03-.12	$X^2(13) = 32.32, P \leq .002$	154	0	NS	NS	Inc
Standardized/reliable aggression measures										
No	9	.24	.28	.10-.38	$X^2(8) = 44.63, P \leq .001$	155	15	$P < .05$	NS	Yes
Yes	18	.08	.09	.03-.12	$X^2(18) = 67.37, P \leq .001$	316	0	NS	NS	Inc
Validated aggression measures										
No	16	.09	.19	.01-.18	$X^2(15) = 79.45, P \leq .001$	423	5	$P < .05$	$P < .05$	Yes
Yes	11	.05	.08	−.02-.11	$X^2(10) = 53.62, P \leq .001$	91	0	NS	NS	Inc
Media type										
Video game	15	.05	.15	−.03-.13	$X^2(14) = 46.56, P \leq .001$	159	2	$P < .05$	$P < .01$	Yes
Television	7	.04	.12	−.08-.17	$X^2(6) = 78.25, P \leq .001$	73	0	NS	NS	Inc
Mixed/movies	5	.10	.15	.02-.17	$X^2(4) = 13.27, P \leq .01$	76	1	NS	$P < .05$	Yes
Participant age										
Adult	11	.03	.15	−.18-.15	$X^2(10) = 45.56, P \leq .001$	84	1	$P < .01$	$P < .01$	Yes
Child	16	.08	.13	.02-.14	$X^2(15) = 93.33, P \leq .001$	439	1	NS	NS	Inc
Type of Study										
Experimental*	10	.25	.25	.11-.38	$X^2(9) = 49.00, P \leq .001$	159	15	$P < .05$	NS	No
Correlational	12	.08	.11	.03-.13	$X^2(11) = 40.23, P \leq .001$	219	0	NS	NS	Inc
Longitudinal	5	.03	.05	−.08-.15	$X^2(4) = 24.84, P \leq .001$	4	0	NS	NS	Inc

k, Number of independent studies; r_+, pooled correlation coefficient (corrected); r_u, uncorrected effect size estimate; FSN, Fail-safe N; OFSN, Orwin Fail-safe N; RCT, significance of Begg and Mazumdar rank correlation test; RT, significance of Egger Regression; NS, non-significant; Inc, inconclusive.
*Identical grouping of studies.

lence.[7,11] Some measures, such as peer and teacher nominations of aggression, have been shown to have poor validity coefficients.[18]

For the type of aggression-related outcome, 10 studies (37%) used "proxy" measures of aggression involving no direct physical aggression or violent behavior. Fourteen studies (52%) measured aggression toward another person, such as physical assaults, fighting, or arguing. Only 3 studies (11%) examined violent criminal behavior.

For controls for "third" variables, approximately half of the studies (14; 52%) attempted to control for at least one "third" variable, such as sex, personality, or family environment.

Main Analysis

Results from the meta-analysis, including those for moderator variables, are presented in the Table. Data presented include the effect sizes corrected for publication bias, the uncorrected effect sizes, confidence intervals, and several analyses of publication bias. Only experimental studies with "proxy" measures of aggression did not experience any publication bias. Of the remaining study types, 7 demonstrated consistent evidence of publication bias, whereas 9 were inconsistent in results. As such, we considered the corrected effect sizes to be better indication of the actual size of effects.

Results suggested that the overall effect for exposure to media violence on subsequent aggression was $r_+ = .08$. This figure is lower than most recommended levels for even the smallest clinically significant effects.[19] Uncorrected results are also fairly weak ($r_u = .14$), yet these results appear to have been inflated by publication bias, thus giving us confidence that the $r_+ = .08$ figure is more accurate.

Results also support that outcome varies widely in the methodology used. In relation to the initial study hypotheses: 1) Aggression measures that were unstandardized/unreliable produced the highest effects ($r_+ = .24$); effect sizes for measures that were reliable were much lower $r_+ = .08$; 2) Similarly, aggression measures with poor validity data produced higher effect sizes ($r_+ = .09$) than did those with good validity data ($r_+ = .05$); 3) "Proxy" measures of aggression, that did not make use of directly aggressive or violent behaviors, produced the highest effect sizes ($r_+ = .25$), with effect sizes for aggressive behavior toward another person ($r_+ = .08$) and violent behavior ($r_+ = .02$), considerably lower; and 4) Research designs that controlled for "third" variables tended to produce lower effect sizes ($r_+ = .08$) than did those that failed to control adequately for "third" variables ($r_+ = .09$). This difference was fairly minor, however.

Much higher effect sizes were found for experimental designs than for either correlational or longitudinal designs.

Unfortunately, experimental designs are greatly impaired by their consistent use of poor aggression measures. Slightly larger effects were seen for children than for adults. Studies of "mixed" media produced slightly larger effects than did those for video games or television.

DISCUSSION

This work calls into question the significance of media violence exposure as a public health concern. By the most liberal estimates available ($r^2 = .02$ compared with $r^2 = .16$ for smoking/lung cancer), the effects seen for smoking and lung cancer at are least 8-times stronger than for media violence exposure. By using the more conservative figures of $r = .9$ for smoking and lung cancer and $r = .08$ for media violence exposure, that number is closer to 135-times stronger. To put this in context for violent behavior, the effect size for other variables related to violence include genetics ($r = .75$), personal self-control and criminal opportunity ($r = .58$), poverty, ($r = .25$), and exposure to childhood physical abuse ($r = .25$).[20]

Publication bias was found to exist for 7 subgroups of studies and was likely to exist for 9 other subgroups of studies. Only experimental studies using proxy, poorly validated measures of aggression did not demonstrate any evidence of publication bias. This is not surprising, because publication bias is likely widespread throughout social science and clinical research.

Hypotheses supportive of the critical perspective that media violence effects research may be driven by poor methodology, including inadequate aggression measures and failure to consider third variables, in each circumstance were supported. These results indicate that the perception of the strength, consistency, and generalizability of existing media violence research may be greatly overstated. This analysis does not find support for either a causal or correlational link between violent media and subsequent aggression in viewers.

Public Health Effects: How Was the Risk Miscalculated?

Bushman and Anderson[2] calculate the effect size for smoking and lung cancer as approximately $r = .4$ on the basis of binomial effect size display-related effect size calculations.[21] These types of calculation are controversial, because some authors argue that they grossly underestimate effect sizes.[3,22,23] In addition to considering research on smoking and lung cancer, Bushman and Anderson also calculate effect sizes r for other medical effects such as passive smoking and lung cancer, condom use and HIV infections, asbestos exposure and laryngeal cancer, etc, all of which they calculate as less than the effects than for media violence. Unfortunately they fail to make clear that they are attempting to convert odds ratios and relative risks into Pearson r effect sizes, which is considered invalid.[22,23] A related issue is that comparing psychological research to clinical research may be problematic because of concerns about the validity of outcome data. To the extent that clinical research uses mortality or morbidity as

outcome (eg, smoking research), few problems with the validity of the outcome are apparent. However, aggression measures used in media violence research have historically been criticized for validity problems associated with generalizing effects to real-world violence.[4,6,7] Bushman and Anderson's[2] figure of $r = .4$ would suggest that 16% of the variance in lung cancer can be attributed to smoking; however, the American Cancer Society places this figure at 87%,[24] closer to the calculations of Block and Crain[3] of an effect size of approximately $r = .9$.

In comparing media violence effects with those of smoking and lung cancer, Bushman and Anderson[2] and Huesmann[1] use an effect size for media violence research calculated by Paik and Comstock[8] of $r = .31$ for media violence and aggression. Arguably, there are several apparent problems with the use of this statistic. First, no other meta-analysis of media violence effects finds effects this large.[9-13] It is unknown why Bushman and Anderson[2] ignore their own lower effect size results in favor of that of Paik and Comstock.[8] Because Paik and Comstock appear not to have weighted the effect sizes in their analysis according to sample size, it is likely that their result is inflated; they also noted that the effect size results were highly dependent on the type of measure used.[8] Higher effects were found for "proxy" measures of aggression and much smaller results for actual physical aggression and violent crime. The effect size for media violence on violent crime was a much lower $r = .1$. This observation relates back to the observation[4,6,7,25] that the validity of aggression measures is important to consider when measuring the effect size of media violence research.

Thus the comparison between media effects and smoking research appears to have been grossly over inflated, possibly because of ideological factors. In addition to this study, the skeptical view of the relationship between media violence exposure and violent behavior is supported by more recent work. Savage and Yancey[13] analyzed the effects from research that specifically focused on violent behaviors, as opposed to cognitive tests or surveys. The authors concluded that the existing literature provided no evidence of a relationship between media violence exposure and aggressive behavior. Our results concur with this finding.

The concern remains that media violence effects research may continue to be driven primarily by ideological or political beliefs rather than objectivity. Media violence has a long history of being driven by ideology.[26] Why the belief of media violence effects persists despite the inherent weaknesses of the research is somewhat of an open question. There may be a "perfect storm" of political opportunism, a union of far-right and far-left social agendas, and scientific dogmatism that has impaired the scientific community's ability to critically examine this research field. Ultimately, data from this study do not support the conclusion that media violence research is a significant public health concern. If it is the goal of society to reduce violence, scientific, political, and economic efforts would likely bear more fruit in other realms.

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