The Effects of Media Violence Exposure On Criminal Aggression: A Meta-Analysis

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THE EFFECTS OF MEDIA VIOLENCE EXPOSURE ON CRIMINAL AGGRESSION

A Meta-Analysis

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The article presents a meta-analysis of studies—yielding 26 independent samples of subjects—on the relationship between exposure to media violence and violent aggression. Mean effect sizes from aggregate and experimental studies do not suggest that media violence and criminal aggression are positively associated, but findings from prospective longitudinal studies are more ambiguous. Summary statistics based on models strictly conforming to each study's original design were compared to post hoc models (in which measures or model specification were not consistent with the original description). Overall, "original" models, controlling for "trait," did not suggest that exposure to media violence is associated with criminal aggression. The summary statistic for boys reached statistical significance, but the effect size was small, and conclusions based on this finding are attenuated because of known biases in the coefficients estimated.

Keywords: *violent crime; media violence; meta-analysis; aggression*

The literature on media violence has been reviewed extensively, with most reviewers concluding that there is strong evidence that exposure to media violence causes aggression. This is based on a large number of studies employing many types of research design. Some reviewers (e.g., Donnerstein & Linz, 1995; Palermo, 1995) also conclude that exposure to media violence is likely to cause violent behavior. For example, Huesmann and Miller (1994) wrote that "the current level of interpersonal violence in our societies has been boosted by…childhood exposure to a steady diet of dramatic media violence" (p. 155), and Sege (1998) asserted that "one of the best documented causes of the modern upsurge in violence appears to be childhood exposure to television violence" (p. 129). More recently, Anderson et al. (2003) stated that there is "unequivocal evidence that media violence increases the likelihood of aggressive and violent behavior in both immediate and long-term contexts" (p. 81). It is this claim that the present article addresses because it is this claim that has been reified in the popular press and has most influenced policy initiatives. For example, the American Medical Association (1993) has expressed its "vigorous opposition" to television violence. For decades, a steady stream of legislative initiatives has blamed television for violence in our society, recommended actions the government should

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take to counter its effects, and proposed laws related to this issue (see Albiniak & McConnell, 1999; Reid, 1999).

Few reviews have focused on violence per se. Savage (2004) reviewed the literature and concluded that "the body of published, empirical evidence on this topic does not establish that viewing violent portrayals causes crime" (p. 99). Here, we present a meta-analysis of the research on the role of television and film violence in causing criminal aggression to provide estimates of effect sizes and a detailed analysis of the role that certain methodological features play in biasing them.

With this purpose in mind, we make a distinction between studies of aggression and studies of violent behavior. Although some authors treat aggression and violence as one and the same (e.g., van der Vort, 1986), we do not. Although aggression is a broader concept and includes a host of behaviors that are often annoying but not necessarily illegal, violence is more narrowly focused on the "exertion of physical force so as to injure or abuse" (*Merriam-Webster's Collegiate Dictionary*, 1998). Criminologists are generally interested in violent behavior that transcends normal aggression and causes physical harm to others in a manner that is designated as illegal in the criminal code. If media violence policy is aimed at reducing violent crime, a narrow focus on studies that examine effects on more serious aggression is in order.

As it happens, there are not very many studies that actually measure the effects of media violence on *criminal* violence. The vast majority of the published studies employ measures of aggression that are neither violent nor criminal. For example, many early studies used a learning paradigm in which subjects were told to administer shocks to another individual. The outcome measure was often the maximum level of shock that each subject chose to administer or how many shocks were delivered (Kaplan, 1984). If the group that had viewed a violent television show in an earlier experimental session had a higher average score than the group that had viewed a control program, evidence of an effect was said to be demonstrated. In most studies, there was no likelihood of retaliation or punishment for administering the shocks, as there would be in real life, and in many of the studies, subjects were first frustrated or angered to enhance the effect. Other studies have examined a variety of outcomes associated with aggression, including violent attitudes.

Several reviewers, including Kaplan (1984), Felson (1996), and Freedman (2002) have questioned the validity of using measures from the shock administration paradigm for understanding real-life violence and aggression. There are several reasons why scholars interested in criminal violence might be skeptical of the generalizability of findings from these studies. First, subjects are *directed* to commit the aggressive act (sometimes referred to as a *demand* effect; Felson, 1996). This is generally untrue of criminal aggression. Second, there is a major psychological and behavioral difference between pressing a button and hitting, stabbing, or shooting someone. Third, there is no rule or law against participating in a laboratory experiment and following the experimenter's instructions. We can imagine that many ordinary people who would not be willing to violate the law or harm others under normal conditions might be willing to administer pain or harm in a setting where it is legal and expected and where retaliation is unlikely to occur. For example, police routinely use reasonable force on suspects, nurses administer injections, and parents scold and spank their children—these are not seen as "aggression." Although some argue that aggression is on a continuum, and thus, causes of minor aggression are likely to be the same as the causes of major aggression, we have made a significant effort to track down empirical sources that establish this link and find no empirical evidence to support this assumption.

Without delving into a complete critical review here (please see Felson, 1996; Freedman, 2002; Savage, 2004), we mention two important points. First, there are apparently fewer studies on this topic than the 1,000 or more that many reviewers claim (e.g., American Academy of Child and Adolescent Psychiatry, 2002; Muscari, 2002). In their meta-analysis, Paik and Comstock (1994) identified just more than 200 studies of the effects of television on antisocial behavior, and Freedman (2002) corroborated that figure in his book. Anderson and Bushman (2002) summarized 284 studies in their two-page article but did not specify the scope of the studies that were included. After eliminating studies that use an outcome that is not "criminal or analogous," Savage (2004) concluded that there are approximately 36 studies related to the effects of exposure to media violence on violent behavior—even including 3 studies about the advent of television and a few studies that use dependent variables that barely met her criteria.

Second, most studies on which reviewers have been relying for their conclusions are decades old, and they do not employ modern statistical methods to estimate effects. Furthermore, most of these studies were conducted before the advent of cable television and VCRs; programs such as *Batman* and *Bonanza* were used as the high-violence shows. In addition, the reader should be aware that there are very serious methodological problems with many of the studies on this topic, even some of the most widely cited. For example, 2 of these studies addressed the advent of television, not exposure to television violence per se (Centerwall, 1989; Joy, Kimball, & Zabrack, 1986). Each had methodological flaws that preclude us from interpreting their findings with a high degree of confidence (see Jensen, 2001, for a thorough discussion of Centerwall's study).

Although many reviews and critiques have already been published, there is a need for a thorough and systematic summary of this literature for the benefit of those who are interested in the implications for violent crime. Meta-analysis provides a useful discipline that can be used to this end (Lipsey & Wilson, 2001). The estimation of effect sizes can allow us to make some comparisons to effects for other correlates of violent crime to put the role of media exposure in the etiology of violent crime in context. Hearold (1986), Wood, Wong, and Chachere (1991), and Paik and Comstock (1994) have published previous meta-analyses. Wood et al. summarized only the findings from experimental studies. Hearold included all types of antisocial behavior in her analysis, not just criminal violence. Paik and Comstock also looked at antisocial behavior generally, and they provide one estimate of the effect size for illegal criminal behavior and one for aggression during play and social interaction. They included very little detail on this point, did not include some of the very important longitudinal studies, and did not break down the analysis by relevant methodological characteristics, such as controlling for trait aggression. Because calls for policy are being made on a regular basis with the purpose of preventing violent crime, and because these calls are being made on the basis of research in this area, it is important for us to revisit this literature and examine whether it provides evidence that exposure to media violence causes criminally violent behavior.

METHOD

SELECTION OF STUDIES

The research question for the present study is, "Does the published literature establish a link between media violence exposure and criminal aggression?" Thus, the population of interest includes all empirical studies relevant to that research question published in English in scholarly journals. We used the list provided by Savage (2004), who relied principally on the bibliographies of reviews and meta-analyses and published empirical articles to locate studies; electronic and print databases such as *Proquest* and *Criminal Justice Abstracts* were also used to search for recent studies. We made a very serious effort to track down every study that met our criteria and believe we have obtained a complete set of studies through approximately 2004. Nonetheless, limiting ourselves to the published literature places significant limitations on potential conclusions from this research (see section on publication bias below). A meta-analysis of this group of studies is unlikely to provide an unbiased estimate of the "true" effect size, but it can shed light on what the literature has reported and help us to establish whether or not firm conclusions on this matter are justified. Because policy makers are relying on the published literature, we are more concerned with what the published literature has to say than the "true" effect size in this analysis.

Studies selected for inclusion were limited to those with a dependent variable that demonstrated criminal violence or analogous behavior. For example, pushing and shoving among preschoolers was included, as was self-reported fighting, but studies employing the Buss aggression machine (shock box) were excluded because pressing a button or turning a knob is not considered illegal and is not analogous to forms of violence we find in the criminal code.

In addition, studies using a modified version of the Peer Rating Index of Aggression (Walder, Abelson, Eron, Banta, & Laulicht, 1961) were included, though we do not believe this measure is a close approximation of criminally aggressive behavior. (For example, "Who starts a fight over nothing?" and "Who pushes and shoves other children?" are the only 2 items out of 10 that allude to actual violent behavior. Other items include "Who says mean things?" and "Who does things that bother others?"). Studies using this measure were included because the measure does contain some violence (our criterion), and it is the only dependent variable used in the major prospective longitudinal studies that test the most plausible hypothesis for a connection between media violence and violent crime: that children watch violent television every day and that there might be cumulative long-term effects of such exposure. As with the shock measure, we made significant efforts to track down studies that establish strong links between peer-nominated aggression and violent aggression but were not able to discover any. In a recent study published by Huesmann, Eron, and Dubow (2002), for example, the reported effect size of child's peer-nominated aggression with later arrest was statistically significant, but the effect size was only .38, the correlation with number of arrests was .26, and the correlation between peer-nominated aggression and violence of the crimes committed was .13. Although these correlations suggest an important "trait" effect, they are not large enough to assume that peer-nominated aggression could serve as a proxy for criminal aggression (we would expect much higher correlations for that purpose—ideally, close to .8 or greater).

Furthermore, only studies that used some measure of exposure to violent television or film as the independent variable were included. We excluded studies in which only overall television viewing was measured and studies in which the "advent of television" was measured because these are likely to be indicative of many other constructs besides exposure to violent television. The ideal measure for individual-level analyses is one in which subjects indicate the programs that they watch and how often they see them, and scores are based on these ratings multiplied by an independent rating of the violence levels of those programs. However, we did include studies in which violence ratings of favorite programs were used (even if they did not account for frequency of exposure).

SPECIAL PROBLEMS FOR META-ANALYSIS

Because procedures and purposes for meta-analysis have been widely described elsewhere (e.g., Hedges & Olkin, 1985; Hunter, Schmidt, & Jackson, 1982; Lipsey & Wilson, 2001; Rosenthal, 1991), only special issues related to this study will be discussed here.

Publication bias. As Lipsey and Wilson (2001) pointed out, "The range of possible report types may be quite broad and include published journal articles, books, dissertations, technical reports, unpublished manuscripts, conference presentations, and the like" (p. 19). When a meta-analysis is restricted to published reports, it is likely that average effect sizes will be larger than if unpublished ones are included because there is a tendency to publish studies with large or significant effect sizes. This may result here in a summary effect size that is larger than it would be if all studies were included.

Mixed quality. Lipsey and Wilson (2001) discussed the problem of including studies of mixed quality as equal contributors to a meta-analysis. If a study with very poor methodology has an effect size that is much larger or smaller than the other studies, for example, the estimate of the overall effect size will be biased. This was addressed in the present analysis by (a) including only published studies, (b) summarizing studies by design type so that studies with suspect design features common to a design type are kept together, (c) coding for features of the study such as whether the comparison was "post hoc" (more below), (d) weighting averages by sample size in most cases, and (e) isolating comparisons that used appropriate control variables in statistical models.

Statistical reporting, missing comparisons. Unfortunately, because of the age of the studies and the variety of methodologies used, it was very difficult to derive summary effect sizes for some of the studies. The net result of this problem is almost certainly an upward bias in average effect sizes computed here, because null findings are not included in the computation of the mean unless the authors provided the associated statistical information.[1]

Post hoc comparisons. Gorman (2004) examined the effectiveness of prevention programs and compared average effects of comparisons that were consistent with the original design of a set of studies and average effects for post hoc comparisons—analyses of models that followed later. He found that post hoc comparisons had higher average effect sizes than comparisons that were built into the original design of the studies. In some cases, there was little evidence that programs that appeared to work well when judged based on their post hoc results worked at all when evaluated based on the original models. In the present study, post hoc comparisons came in several types, including dividing the sample (e.g., by gender, age group, level of initial aggression, valid reporters and less valid reporters, older children and younger children, 3-year-olds and 4-year-olds, etc.); converting variables into logs, "indexes," and so forth without discussing this a priori; adding or removing control variables without explanation; and excluding outliers (also not discussed a priori). But the two most problematic post hoc changes, which will be examined here, were changing the independent variable and changing the dependent variable. In practice we found that, after selecting the original models for our analysis when possible, the post hoc problem was confined to a small subset of prospective longitudinal studies. A thorough discussion will resume in the section on longitudinal studies.

Time lags. In the two time-series studies included here, the authors provided effect sizes for a large number of time lags instead of identifying the particular lags in which an effect was most expected. Some of the time lags had statistically significant findings, and many did not. The authors emphasized the statistically significant ones and concluded that their hypotheses were supported by the data. Averaging all the time lags together to represent the findings of each study, as was done here, is the most defensible course of action in a meta-analysis, but it is likely to result in a downward bias in the estimated effect size because some time lags are certainly more likely to demonstrate an effect than others. Unfortunately, because the authors did not identify a priori which lags these would be, we felt obligated to combine them.

In addition to publication bias, almost all of these problems conspire to exaggerate, in a positive direction, the effect sizes that will be presented here.

PROCEDURE

Coding. In the initial coding phase, for each study a line of data was created for each comparison reported in tables or text that was relevant to the present research question, whether or not an effect size or even statistical significance was reported. Variables included, for example, study and comparison identifiers, subject ages, gender, sample size, design (experimental, cross-sectional correlation, longitudinal, etc.), independent variable, dependent variable, presence or absence of 29 control variables, effect statistic, and statistical significance as reported by the author(s). The principal investigator coded all the studies and also double-checked the coding for each. For approximately half of the studies, a trained graduate student also coded the data, and comparisons were made in an attempt to improve the accuracy of the coding.

Summarization. The second phase involved reducing multiple effect sizes into one effect size for each study for each analysis subgroup (aggregate studies, experiments, correlations, multivariate longitudinal comparisons, male multivariate, female multivariate). In some cases, among the correlational studies and prospective longitudinal studies, more than one study reports findings for the same group of subjects. This violates the assumption of independence and gives too much weight to those studies. Our solution was to create one line of data for each study of separate subjects so that no two records would be based on the same subjects. A detailed list of how this was accomplished can be obtained from the authors.

Second, if authors provided many analyses, we chose “best models” if these were obvious. The choice of “best models” was based on the logic of the study and on preferences that have been expressed in the published literature for certain measures. In particular, among self-report studies, a preference was given to the independent variable computed by multiplying subjects’ reported frequency of viewing various programs by an independent rating of violence for those programs over other measures of media violence exposure. Similarly, if a set of multivariate models was provided that included both statistical models consistent with the original design of the study and post hoc models, the original models were used. If there was no clear "best model" or models among analyses presented, the statistics reported were converted to a common effect and averaged.

Next, effect sizes were combined into a weighted average. For experiments, the standard mean difference ES_{sm} was used as the effect size estimate (see Lipsey & Wilson, 2001). In most cases, we were able to compute this using information provided (group means and standard deviations). In a few cases, computations were accomplished in other ways. For correlational and multivariate studies r was used as the effect size (ES), and β was treated as if it were equivalent to r (Pratt & Cullen, 2000). Partial t was converted to r using a standard formula (e.g., Wolf, 1986). When discussing use of multivariate analyses, Lipsey and Wilson (2001) argued that, because the standardized regression coefficient from each analysis is assumed to be estimating a different population parameter and the standard error of each regression coefficient usually cannot be computed from data reported, one cannot compute the inverse variance weight necessary for a proper meta-analysis. Because of this, Lipsey and Wilson added, at the time, that there really was no way to accurately include multivariate findings from multiple regression, discriminant analysis, or structural equations models in a meta-analysis (see also Hunter & Schmidt, 2004). Most modern studies report multivariate findings, however. In the media violence literature, the most important studies are all multivariate, and we thought it would be extremely misleading to leave these out. So we combined multivariate standardized β into average effect sizes, as had been done by Pratt and Cullen (2000) and others, but we emphasize the fact that these statistics come from models with a variety of control variables and that our estimate of weights, standard errors, and confidence intervals may be imperfect. We note also that some meta-analysts do not agree with this procedure (e.g., Cooper & Hedges, 1994; Hunter & Schmidt, 2004). Furthermore, we provide separate summaries for studies in which trait aggression, for example, has been controlled, which means that the models that we are comparing have similar (but not exactly the same) model specification. We still believe this to be a very useful exercise—a weighted average standardized β can certainly give readers a general idea of the size of effects, even if they decide to ignore the confidence intervals that we also provide. A comparison of these effect sizes can be made to those for other correlates of violent crime to set the "media" effect in context. If readers choose to ignore the summary statistics, they may still gain a better understanding of these effects by looking at the individual lists of effect sizes that we also provide.

Fisher's r *to* z *transformation*. As has become customary, each summary effect size originally reported as a function of r was converted to a z, because "the sampling distribution of z(r)-scores is assumed to approach normality, whereas the sampling distribution for r is skewed for all values other than zero" (Pratt & Cullen, 2000, p. 940; also see explanations by Hall, 1995; Pearson, Lipton, Cleland, & Yee, 2002; Rosenthal, 1991).

Weighting. Because estimates of larger samples are thought to be more representative of the general population, it is also customary to use weighted averages in meta-analysis.[2] We compute weights (w) as provided by Lipsey and Wilson (2001).

Some sample sizes in the correlational studies are very large, and it is undesirable to allow the largest studies to overwhelm the small ones in their impact on weighted average effect estimates. Here extremely large sample sizes were "Windsorized"—or set at two standard deviations above the mean n for the remaining studies (see Gorman & Derzon, 2002; Lipsey & Wilson, 2001). Windsorization applied to weighting but not to the estimate of the standard error or confidence intervals, and it was most common among the simple correlations. Among the multivariate comparisons, only Milavsky, Kessler, Stipp, and Rubens (1982) was an outlier on sample size.

For ES_{sm}:

$$w = 1 / SE_{sm}^{2}$$

For z(r):

$$w = n - 3$$

RESULTS

OVERVIEW

Thirty-six studies met the criteria outlined in our introduction, though only 33 of them reported adequate information to code a summary measure of significance and only 29 reported adequate information to code a summary effect size. Furthermore, some of these studies reported comparisons on the same subjects, so the sample was reduced to 32 independent samples of subjects, 26 for which we could compute a summary estimate of effect size. Because the designs varied a great deal, we followed Lipsey and Wilson's (2001) suggestion to avoid the apples-and-oranges problem and categorized the studies by design type. This also helps avoid combining studies with mixed methodological quality, because some of the questionable methodological features are present in the same types of studies. A summary table is available from the authors.

AGGREGATE STUDIES

There are four studies that test the research question of whether media violence is linked to violent aggression and that use a measure related specifically to violent television (as opposed to the advent of television). Three of these studies have methodological features that cause concern. Berkowitz and Macaulay (1971) examined time series data about the JFK assassination (1963) and the Speck murders (which occurred in 1966), and Phillips (1983) examined time series data about televised prize fights. Both of these studies employed a large number of time lags, few controls, and reported very high *df* because of the use of long time periods. The third study is a very simple cross-national correlation between a rating of movie violence and a national homicide rate, with a sample size of 19 (Lester, 1989); it employed no control variables to protect the estimate from the threat of spuriousness.

By contrast, Messner (1986) reported a very high-quality, SMSA-level multivariate analysis of the relationship between violent crime rates (broken down by crime type) and a measure of exposure to television violence, which included violence ratings and Nielsen estimates of the actual television audience for the programs.

Though three of the studies (Berkowitz & Macaulay, 1971; Lester, 1989; Phillips, 1983) are on record with positive relationships between media violence and violent crime, our findings suggest that aggregate effects have not been established. Two of the authors emphasized their significant positive effects from among an array of nonsignificant time

TABLE 1: Summary of Weighted Effect Sizes and Confidence Intervals

Design Type	*No. of Studies* k[a]	*Combined* N[b]	*Effect Estimate* r	*95% CI*	*Ratio of Statistically Significant (+) Studies*[c]
Aggregate comparisons					
Overall	4 (4)	NA	.043[d]		
Not computed	1:4				
Cross-sectional multivariate	1 (1)	281	–.155[e]		0:1
Cross-sectional correlation	1 (1)	19	.240[e]		0:1
Time series	2 (2)	NA	.043[d]		1:2
Experimental, quasi-experimental comparisons					
Overall	10(6)	1567	.057	–.006-.119	2:8
Cross-sectional simple correlations					
Males	16 (13)	3995	.142	.111-.174	9:14
Females	13 (11)	1809	.194	.147-.240	7:12
Overall	14 (12)	10,064	.164	.144-.183	9:12
Multivariate, longitudinal comparisons					
Males, original models	7 (5)	1118	.09	.030-.148	1:6
Males, post hoc	5 (4)	343	.198	.090-.305	2:5
Females, original	5 (5)	888	.047	–.019-.114	1:5
Females, post hoc	3 (2)	115	.283	.095-.471	1:3
Overall, original	2 (2)	1058	.038	–.022-.099	0:2
Overall, post hoc	4 (4)	672	.210	.134-.287	4:4
Combined (original and post hoc)	6 (6)	1730	.118	.071-.165	4:6
Multivariate longitudinal comparisons, controlling for trait					
Males, original	6 (4)	953	.071	.007-.135	1:5
Males, post hoc	6 (4)	300	.193	.077-.308	0:6
Females, original	5 (4)	676	.004	–.072-.077	0:5
Females, post hoc	4 (2)	259	.251	.066-.437	1:4
Overall, original	2 (2)	1058	.038	–.022-.099	0:2
Overall,					
Post hoc	4 (4)	582	.187	.105-.269	2:4
Combined	6 (6)	1055	.102	.053-.150	2:6

Note. CI = confidence interval; NA = not applicable.
a. The main number in this column refers to the number of published studies; the number in parentheses refers to the number of independent samples used in these studies.
b. For studies with effect sizes.
c. The number of studies indicated in this column may not be equal to the number in column 2 because this column includes only those for which statistical significance is known.
d. Unweighted mean effect size *r*.
e. Summary statistic for one study.

lags tested (Berkowitz & Macaulay, 1971; Phillips, 1983), but because they did not specify which time lags were likely to yield statistically positive results a priori, we combined the time lags to estimate their effect sizes—and these average effect sizes were not statistically significant (see Table 1). Lester (1989) reported the largest effect size, but it was not statistically significant because of the very small sample ($n = 19$).

The Messner (1986) study is similar to numerous aggregate multivariate studies of violent crime. Interestingly, he found such consistent evidence that metropolitan areas exposed to more violent television had *lower* violent crime rates, he was obliged to forward an explanation for why such exposure might *reduce* violence. Messner offered a subcultural argument that TV viewing may reduce exposure to criminal subcultures and a "routine activities" account (household activities such as television watching are not associated with the convergence of offenders, targets, and lack of guardianship).

The unweighted average effect size of the aggregate studies, reconverted to *r*, was .043 (see Table 1), but this should be downplayed because of the combination of very different methodologies. This value would be higher if we had chosen among time lags reported by the two time-series studies, rather than averaging all the time lags tested to represent the study findings. Weighting was not used in this analysis, nor was the confidence interval computed, because the time-series studies reported very high *n* and it was not appropriate to weight their effect sizes by these arbitrarily chosen "sample sizes." At best, we can conclude that the data are inconclusive, though, because the Messner study reported a strong, negative effect and the other three studies failed to control for possible spuriousness.

EXPERIMENTS AND QUASI-EXPERIMENTS

Ten experiments and quasi-experiments were included in this sample. These included studies in which children were either randomly assigned to exposure to violent media, or preexisting groups of children were differentially exposed to violent media and compared. Only six of the studies reported adequate statistical information to compute comparable effect size statistics, though eight provided enough information to code for statistical significance. Most subjects were between the ages of 3 and 10 years old. The effect size used for this set of studies was the standardized mean difference (ES_{sm}) computed, as recommended by Lipsey and Wilson (2001), as follows:

$$ES_{\mathrm{sm}} = (\overline{X}_{G1} - \overline{X}_{G2}) / Sp$$

If we look first at statistical significance for the studies themselves, the findings are completely inconclusive: two studies have statistically significant positive effects, four have null effects, and two studies report statistically significant negative effects. The weighted average effect size, reconverted to *r*, was .057 for six studies. This was not statistically significant (CI 95%: –.006 to .119). A test of homogeneity revealed that the set of effect sizes used in this analysis was heterogeneous ($Q = 20.9$, $df = 5$). A histogram reveals that the effect size reported by Steuer, Applefield, and Smith (1971) is an outlier (r, = .369, $ES_{sm} = .7931$). Removing that effect size reduces the weighted average, which remains nonsignificant.

The interpretation of this summary statistic should be limited to very short-term effects of media violence on the behavior of children. It should be noted that there may be an upward bias in this estimate because of (a) publication bias and (b) imitative violence that some studies included. Children who watch *The Three Stooges*, for example, may imitate behaviors from the show. Such direct imitative behaviors were included in measures of aggression in some of these studies. There may also be a downward bias for several reasons, such as reliability problems in the measures and the use of not-so-violent programs as the experimental condition (i.e., *Batman* and *Bonanza*). Also, some studies failed to use an exciting control condition so that children watching the control were bored and may have needed to burn off more energy after the programs than children who watched the more exciting and violent fare. In general, however, it is our conclusion that improvements in methodology would probably not yield much stronger results for short-term experimental studies.

CROSS-SECTIONAL SIMPLE CORRELATIONS

Studies in this group included those in which exposure to media violence and indicators of aggression were measured at the same time. Most of these studies are self-report surveys. The mean correlations between exposure to television violence and aggression estimated will not be controversial. The vast majority of the studies report significant, positive relationships between the two variables (9 out of 12). The average effect size, converted back to r, was .164 based on 10,064 subjects; this value is statistically significant.

We suspect that our estimate may be biased in the upward direction for several reasons: publication bias, the use of a measure of preference for violent programs rather than actual exposure to media violence in some studies, and the fact that McLeod, Atkin, and Chaffee (1972a, 1972b) may have had a computation error (they report much higher values for correlations among all S's than they do for males or females separately). As others have repeatedly noted, a major concern is the likelihood that this value is biased in a positive direction because of spuriousness. Because it is likely that aggressive individuals *prefer* violent shows (e.g., Fenigstein, 1979; Gunter, 1983), the causal direction of this correlation is also ambiguous. One can certainly imagine a dynamic, similar to that proposed by Huesmann (1986), in which trait-aggressive children choose violent programs and, by doing so, reinforce their aggressive tendencies. There are other potential confounds: factors such as socioeconomic status (SES), parent education, family violence, neglect, and intelligence are also correlated with exposure to TV violence and to aggression. Because of these ambiguities, simple correlations are no longer taken very seriously, and later studies typically employed a prospective longitudinal design whereby researchers could control for early "trait" aggression and other potentially spurious factors.

MULTIVARIATE, LONGITUDINAL COMPARISONS

Although there were originally 10 separate publications in this group, 11 independent samples were used for this analysis. Two of the publications reported data on separate samples of subjects, and were, therefore, treated as two studies for our purposes. Some of the studies are nonindependent and were combined. For example, Lefkowitz, Eron, Walder, and Huesmann (1972, 1977), and Huesmann (1986) reported comparisons based on data for the same subjects followed up at different time periods. The solution to this was to combine the multivariate longitudinal comparisons from Lefkowitz et al. (1972, 1977). Unfortunately, Huesmann (1986) only reported one path coefficient for 30-year-old subjects and no sample size; this was not comparable to other statistics reported here, so this was left out of the analysis. Table 2 includes the 11 samples used for this analysis.

Almost all of the longitudinal comparisons reported here employed a prospective, longitudinal wave-to-wave design. (The exception is Kruttschnitt, Heath, and Ward, 1986, who reported a retrospective comparison.) Most of the studies originally recruited elementary school children and followed them approximately 3 years or more. Early-wave aggression was measured to control for a possible personality trait. Exposure to media violence, measured at each wave, can thus be treated as an intervention and temporal order can be established.

One major problem with these studies is that four of them failed to report models that were built into the design of the study and, instead, reported effects using post hoc measures. Because all of these studies are part of a set—reported in the same volume and designed to replicate the same study in five different countries (originally six, but the Netherlands study is not reported in the volume)—it is somewhat of a surprise when the

TABLE 2: List of Effect Sizes (*r*) for Multivariate Longitudinal Studies: Comparison of Overall, Males, Females. Original Models Versus Post Hoc Models

Study	*Overall*	*Males*	*Females*
Original models			
Huesmann & Eron, 1986	PH	PH	.192*
Kruttschnitt et al., 1986	NA	.120	NA
Lagerspetz & Viemero, 1986	PH	PH	.105
Lefkowitz et al., 1977	.062	.284*	–.146
Milavsky et al., 1982 (elementary)	.018	.037	.052
Milavsky et al., 1982 (teen boys)	NA	–.033	NA
Sheehan, 1986	NR	NS	NS
Wiegman et al., 1985	NR	.07	.10
Post hoc models			
Bachrach, 1986 (city children)	.397*	.285	.535*
Bachrach, 1986 (Kibbutz children)	NR	NS	NS
Fraczek, 1986	.225*	.200*	.173
Huesmann & Eron, 1986	.170*	.146	NA
Lagerspetz & Viemero, 1986	.173*	.236*	NA

Note. PH original models not reported (NR); see post hoc. NS = not statistically significant (no statistic available); NA = not applicable.
*$p < .05$.

careful reader discovers that the only reports of findings based on the "original model," as we refer to it here, that is detailed in chapter 2 were Huesmann and Eron's (1986a) and Lagerspetz and Viemero's (1986) analyses for girls only.

Bachrach (1986) used the ratio of aggression to avoidance of aggression for the dependent variable. Fraczek (1986) used violence preference instead of the multiplicand of violence ratings and frequency of program viewing. Huesmann and Eron (1986a) and Lagerspetz and Viemero (1986) substituted the multiplicand of exposure to TV violence and identification with aggressive characters for the TV violence exposure measure in their analyses of boys (because they found that the correlation between early-wave exposure to TV violence and later aggression was not significant for boys). Sheehan (1986) employed the original model in his report of Australian children, but because the findings were not significant, he did not provide effect sizes or other multivariate statistics. For this reason, the meta-analysis distinguishes between original models and post hoc models, and Tables 2 and 3 display the comparison and emphasize this point.

There are a few other issues that should be mentioned to help readers understand the values in the tables. Most studies disaggregated their analyses by gender, so not all of them reported overall findings for the total sample. Six studies reported adequate information to compute an estimate of the overall multivariate longitudinal effect of exposure to TV violence (or preference therefore) on later aggression. Because Milavsky et al. (1982) was an outlier on sample size, its sample size was Windsorized to 428 for the purposes of computing weights.

The overall effect size estimate for all of these studies is $r = .118$. This is statistically significant. Four out of six studies reported statistically significant findings (see Table 1).

If we compare the original models with the post hoc models, however, a clear disparity exists. The weighted average effect for the original models ($k = 2$) is $r = .038$ (not significant; see Table 1) and the weighted average effect of the post hoc models was .210 (significant). Although all four of the post hoc models reported statistically significant coefficients, the

TABLE 3: List of Effect Sizes (*r*) for Multivariate Longitudinal Comparisons Controlling for Aggressive Trait and SES: Original Versus Post Hoc Models

Study	*Overall Control Trait*	*Overall Control Trait & SES*	*Males, Control Trait*	*Males, Control Trait & SES*	*Females, Control Trait*	*Females, Control Trait & SES*
Original models						
Huesmann & Eron, 1986	PH	PH	PH	PH	.169	.169
Kruttschnitt et al., 1986			NA			
Lagerspetz & Viemero, 1986	PH	PH	PH	PH	.049	.049
Lefkowitz et al., 1977	.060	NR	.231*	NR	–.120	NR
Milavsky et al., 1982 (elementary)	.018	NR	.037	NR	.051	NR
Milavsky et al., 1982 (teen boys)	NA	NA	–.033	NR	NS	NR
Sheehan, 1986			NR			
Wiegman et al., 1985	NR	NR	.07	NR	.10	NR
Post hoc models						
Bachrach, 1986 (city children)	.397*	NR	.285	.280	.535*	NR
Bachrach, 1986 (Kibbutz children)	NR	NR	NS	NR	NS	NR
Fraczek, 1986	.140	.136	.145	.150	.130	.120
Huesmann & Eron, 1986	.178*	.178*	.188	.188		
Lagerspetz & Viemero, 1986	.131	.132	.208	.210		
Sheehan, 1986			NS[a]		NS[b]	

Note. PH original models not reported (NR); see post hoc. SES = socioeconomic status; NS = not statistically significant (no statistic available); NA = not applicable.
a. For older boys.
b. For older girls.
*$p < .05$.

relationship between exposure to media violence and criminal aggression in both of the original models is not statistically significant. For males, both original and post hoc multivariate longitudinal models suggest that there is a statistically significant positive effect; for females, the mean effect size is not significant for the original models and is statistically significant and positive for the post hoc models.

Table 2 displays the post hoc analysis problem. As can be seen, authors reported few of the coefficients necessary for the computation. In some cases, the authors reported that the original model was not significant and provided no further analyses (i.e., Sheehan, 1986; Wiegman, Kuttschreuter, & Baarda, 1985). In some cases, authors reported that original models or correlations were not significant and then reported coefficients from post hoc models (which were significant; i.e., Huesmann & Eron, 1986a, and Lagerspetz & Viemero, 1986 for boys). Finally, in some cases, the authors did not report original models at all; they reported only models using post hoc variables (i.e., Bachrach, 1986; Fracek, 1986). Thus, in addition to biases related to missing control variables and publication bias, average effect sizes are likely to be inflated here because the coefficients for studies that found nonsignificant effects are not included in the estimate of the mean. There may also be sources of downward bias, such as any problems with reliability of measures.

Controlling for aggressive trait. The critical feature of the prospective longitudinal studies is the ability to control for aggressive "trait." In this analysis, because of one outlier (Milavsky), the sample size was Windsorized to 397 for the purpose of weighting. Table 3

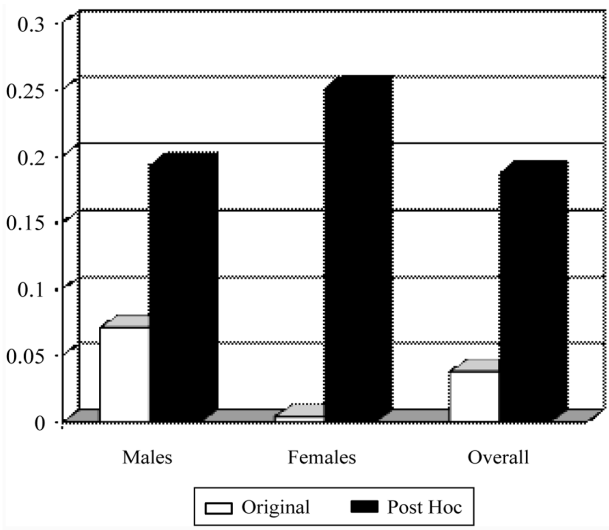

Figure 1: Aggressive Trait. Comparison of Original Models to Post Hoc Models

displays the effect sizes for original and post hoc models for each study in which early aggressive trait was controlled. The weighted average effect size for the original models was $r = .038$ (see Table 1) and was not statistically significant; the weighted average effect size for the post hoc comparisons was $r = .187$ and was statistically significant. The combined weighted average for both original and post hoc models was $r = .102$. Taken individually, none of the overall summary scores for the original models was statistically significant, but 50% of the summaries for the post hoc comparisons were.

For males, the mean effect size among the original multivariate models that controlled for trait barely reached significance; for females, it was not statistically significant. For both sexes, the mean effect for post hoc models was statistically significant. As Table 3 shows, these estimates are based on a small number of coefficients. Figure 1 displays a comparison of original and post hoc models for males, females, and combined samples. The graph clearly shows that for all groups, post hoc models had much higher average effect sizes. The highest average effect size for original models was for males (.07). Again, the effect sizes for the original models are likely to be biased in the upward direction.

Only a very small number of studies reported effect sizes for models for which both trait and SES were controlled, and, unfortunately, almost all of them use post hoc models. SES is just one of a variety of other control variables that would be necessary to assure us that correlations between exposure to TV violence and aggression are not spurious. Although most studies examined a series of covariates, their approach was usually to look at simple correlations and, if these were not statistically significant, leave the variable out of future models. Also, authors tended to prefer testing controls one at a time in some early analyses. Though this is one approach to model specification, because partition of error changes as every variable is entered into a model, it is more appropriate to include all the potential sources of spuriousness simultaneously if possible. Summary statistics suggest that, again, these relationships are very small.

INTERACTION EFFECTS

Some authors have proposed that the effects of media violence may be concentrated in those who are most aggressive by nature. Approximately seven studies have addressed this issue using the more "criminal" outcomes. Unfortunately, there are very few effect sizes reported, and the statistical analyses are often casually reported. Although Feshbach and Singer (1971), Sprafkin, Gadow, and Grayson (1987), and Huesmann and Eron (1986b) all found no evidence that "trait" aggression enhances the effects of exposure to media violence, three other studies found strong evidence in favor of this hypothesis. In two of the studies, it was reported that media violence effects were significant for high aggressive subjects and not significant or negative for low aggressive subjects (Josephson, 1987; Robinson & Bachman, 1972). At this time, it is not possible to draw any firm conclusions, but this particular hypothesis should be explored further.

DISCUSSION

In the end, there is not one study that reports the comparison we would really like to see to satisfy our curiosity about the media violence–criminal aggression relationship. Such an analysis would use, as a dependent variable, serious criminal aggression or violent crime rates. It would use a measure of exposure to television violence that includes both an accurate assessment of exposure (how much time) and an independent rating of violence in the programs, and it would control for early-wave trait aggression using a reliable and valid measure of early childhood aggression. The analysis would also control for variables such as SES, parent education, parental violence, neglect, and intelligence, all of which are also associated with both exposure to TV violence and aggression. The Huesmann (1986) report approaches this—because criminal convictions are used as the outcome—but, unfortunately, it does not include enough detail about the analysis to judge the merits of the findings reported.

A review of both aggregate studies and experimental evidence does not provide support for the supposition that exposure to media violence causes criminally violent behavior. The study of most consequence for violent crime policy actually found that exposure to media violence was significantly *negatively* related to violent crime rates at the aggregate level (Messner, 1986). With regard to the developmental question of whether a "diet of violent TV" causes violent behavior, we must conclude that the evidence is not adequate to claim that exposure to violent TV is a significant source of violence in U.S. society. The summary presented here used, in most cases, generous decision making for the estimation of effects. For example, we included peer-nominated aggression studies. We elected to use an estimate of sample sizes that resulted in narrower confidence intervals and a greater likelihood of statistical significance. We did not substitute zero values for missing coefficients. In spite of this, estimated effect sizes for females and combined groups of males and females were not significant, and the estimated average effect size for males was very small (.07) and barely significant when original models were examined. We believe that this value is biased in an upward direction because of missing control variables and missing statistics from studies that had nonsignificant results. Because of this, and because the outcome variable is only weakly associated with serious violence, we feel compelled to argue that the evidence does not support claims that "the current level of interpersonal violence in our societies has been boosted by…childhood exposure to a steady diet of dramatic media violence" (Huesmann & Miller, 1994, p. 155); that "one of the best documented causes of the modern upsurge in violence appears to be childhood exposure to television violence" (Sege, 1998, p. 129); or that there is "unequivocal evidence that media violence increases the likelihood of aggressive and violent behavior in both immediate and long-term contexts" (Anderson et al., 2003, p. 81). It is plain to us that the relationship between exposure to violent media and serious violence has yet to be established.

Saying that the effect has not been established is not the same as saying that the effect does not exist. It should be pointed out that most of these studies are quite old, and the operational definition of *exposure to television violence* was mild by comparison to modern programming. Subjects had few channels from which to choose and movies were seen when they happened to be presented on network television. There were no video games at this time. It is possible that exposure to the forms and amount of violence available to children today—R-rated movies that can be watched at home, repeatedly, using video and DVD technology; increased violence in mainstream television; a greater chance that someone seeking out violent programming can find it because of the many channels on cable and satellite TV—may cause aggression. But because violent crime rates have been falling and not increasing in recent years, during which time these technological advances have been made, it is either the case that the effect does not exist or that other factors are so much more weighty that they overwhelm the television's influence on aggregate violent crime rates.

An effect this small—and again, we believe there is still a bias in a positive direction—is likely to be drowned out by the many other factors that wield strong influences in the development of criminality, such as poverty, education, neighborhood, and exposure to real violence. Although it is clear from early studies that children do, in fact, imitate aggression that they see in films (this has been demonstrated many times), it is also clear that "real life trumps TV every time" (Jenkins, 1999). At aggregate levels, factors such as concentrated disadvantage, unemployment, population demographics, and the like may overwhelm individual-level factors in influencing violent crime rates. Why, then, have legislators and others focused so much on media violence rather than, for example, concentrated disadvantage? In our literature on causes and correlates of violent crime, a quick perusal of one author's library found that, in many cases, effect sizes are much higher than the .07 found for males here. For example, effects for measures of economic well-being, such as resource deprivation or income, on aggregate measures of violent crime are often greater than .35 (Hannon & DeFronzo, 1998; Kubrin & Wadsworth, 2003; LaFree & Drass, 1996) and are sometimes as high as .62 and .77 (DeFronzo & Hannon, 1998; Wooldredge & Thistlethwaite, 2003). Among individuals, Pratt and Cullen (2000) computed an average effect of self-control on behavioral measures of .28, Piquero and Sealock (2004) found a correlation of .43 between measures of strain and offending for males, and effects for peer delinquency are usually very high (Piquero & Sealock, 2004, report .42 and Mears, Ploeger, and Warr, 1998 report .32), and these did not suffer from the high probability that their estimates were significantly biased.

In summary, because the burden, in science, is to reject the null hypothesis only when we have a high degree of confidence that actual values are not zero, using the most rigorous methods available before drawing firm conclusions about a phenomenon, it is our conclusion that the effects of exposure to media violence on criminally violent behavior have not been established. The effect sizes examined here are small and almost certainly biased in a positive direction. It is surprising that these findings, which demonstrate only a borderline effect for males at best, have been used to make dramatic claims about violence in society. Proposals to address the "media violence problem" under the guise of reducing violent crime in our society are not likely to succeed. We would recommend exploring other policies for reducing violent crime.

NOTES

1. Statistical reporting overall was poor. In some cases, no estimate of effect size could be coded because the authors did not provide enough information. Missing effect sizes frequently are not significant, and the implication here is that means are probably computed without the lowest values (thus biasing estimates in an upward direction). We chose not to impute zero values for missing values because most of the effects for correlational studies are positive and small, not zero.

2. Studies frequently reported numerous comparisons that did not all have the same sample size. We need a sample size to compute weights, standard errors, and confidence intervals to represent each independent study. After computing the weighted average to represent the effect for the study, we opted to use the largest sample reported in any of the models as the sample size for the study. The major implication of this decision is that confidence intervals will be narrower and the decision could result in a type I error—deeming a mean effect "statistically significant" when, under a more conservative decision rule, it would not be.

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