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AREA REVIEW

Media Violence Effects: Confirmed Truth or Just Another X-File?

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This article presents a critical review of the extant literature on media violence effects on violent criminal behavior. Problems within the media violence literature that limit the causal conclusion appropriate to this body of literature are discussed. It is argued that, contrary to common belief, the current literature does not provide ample support for the conclusion that media violence causes aggressive or violent behavior. This article offers positive suggestions for the future of media violence studies. Suggestions for the role of forensic psychologists in communicating media violence research information to courts and the public are also provided.

KEYWORDS *aggression, genetics, family violence, media violence, violent crime*

Several decades of research have focused on the relative impact of violent media (definitions of which vary widely), including television, video games, movies, and music on aggressive behavior in both children and adults. Recent reviews of the literature by the authors involved in this field have concluded that evidence is unequivocal and that “. . . the scientific debate over whether media violence increases aggression is essentially over . . .” (Anderson et al, 2003, p. 81). Despite this claim, other scholars have raised objections over the

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quality and claims of the media violence literature (Ferguson, 2002; Ferguson et al. (2008); Freedman, 1996; Moeller, 2005; Olson, 2004; Pinker, 2002; Savage 2004; Tedeschi & Quigley, 2000). Olson specifically contends that the quality of the extant literature does not merit the strength of their conclusions. Savage (2004) notes that no conclusive evidence linking media violence to violent criminal activity exists. This article concerns itself with claims of causality in the media violence literature with suggestions for positive directions for the future to help understand the link between media violence and violent criminal behavior.

RESEARCH METHODS OF MEDIA VIOLENCE AND VIOLENT CRIME

Research regarding the impact of media violence (including depictions of interpersonal aggression in medium ranging from cartoons to music and video games) on violent crime (or the considerably more vague term *aggression*) takes two fundamental forms: experimental and correlational. Because “violence” can be difficult to study both practically and ethically, “aggression” is often substituted. It is assumed in the literature that these two constructs are close enough that factors that influence aggression may also influence violence. In correlational research, there is usually an attempt to measure the relationship between media violence exposure and some measure of aggressive behavior, thoughts or emotions (although seldom violent criminal activity). Experimental research usually involves examining the impact that a brief exposure to media violence (exposure lengths vary from several minutes to the length of a feature film but are seldom more than a half-hour and often considerably less) on subsequent aggressive behavior (such as punishing an opponent in a reaction time test), thoughts, or emotions. Naturally, owing to ethical constraints, violent criminal behavior is not a dependent variable in such studies. Thus, it is implied that examining the relationship between media violence exposure and aggressive behavior (broadly defined) can imply a societal link between media violence and violent crime.

Difficulties with the link between “aggression” and violent crime (and how these are adequately measured) are discussed later in this article. However, the premise of the media violence–violent crime literature is that an appreciable increase in media violence is associated with an appreciable increase in violent crime. However, despite the general belief that media violence is increasing, violent crime has been declining for over a decade (Federal Bureau of Investigations, 1951–2000). Furthermore, that media violence rates *increased* at some point in the twentieth century is taken as a bona fide without benefit of empirical evidence. No studies have been conducted to examine actual fluctuations in media violence rates. Thus, what exactly is meant by “media violence” in society may be as nebulous a concept as what is

meant by “aggression.” Similarly, how to adequately measure an individual’s exposure to media violence in correlational studies remains contentious. Whether self-report (or parent report) measures truly measure exposure or only the degree to which certain kinds of exposure are more memorable to certain individuals is a question of concern.

Within the specialty field of media violence effects, the most current theory of how media affects aggression (and by extension, violent criminal activity) is the General Aggression Model (Bushman & Anderson, 2002) which borrows from social modeling theory and other cognitive theories (Anderson & Dill, 2000, discuss how this model is based on other, related models of aggression). Briefly, this model suggests that aggressive behavior occurs when aggressive cognitive scripts are activated by particular environmental stimuli. This model theorizes that repeated exposure to violent stimuli such as that found in violent media provides opportunity to form aggressive scripts that can then be subsequently activated. The greater extent to which a person is exposed to violent stimuli, the more aggressive scripts that are formed and the more often these scripts are called upon when presented with potentially hostile environmental stimuli. Thus, it is predicted, an individual exposed to more violent stimuli is likely to interpret ambiguous stimuli as hostile, or harmful stimuli as more intentional, and respond aggressively. This theory implies “passive modeling”¹ in which individuals exposed to violent media will more often engage in violent behavior, regardless of personality, family environment, genetics, or other biological contributions. Although researchers in the area acknowledge that some populations may be more susceptible than others, at the same time they argue that anyone exposed to such media will become more aggressive (e.g., Carnagey & Anderson, 2004). Thus, this passive-modeling theory implies that individuals can begin with no preexisting motivation to aggression or violence but acquire it through repeated exposure to media violence, and it is, as such, a “tabula rasa” approach (Pinker, 2002).

ENDEMIC METHODOLOGICAL PROBLEMS IN MEDIA VIOLENCE LITERATURE

Arguably all scientific manuscripts contain errors and methodological problems. Thus, the intent of this section is not to discuss problems with specific studies but rather to note methodological problems that exist across the existing literature. Specific studies may be held up as examples of problems endemic to the entire body of literature.

¹ The alternative “active modeling” by contrast implies an internal motive that drives an individual to seek out a modeling experience, so as to better perform a specific task or behavior.

Use of Unstandardized, Poorly Validated Measures of Aggression

One of the most basic concerns regarding the measurement of “aggression” is whether the dependent variables used to measure this construct, particularly in laboratory settings, are adequate for the task. Given that the assertion made by most media violence researchers that their research is generalizable to violent criminal behavior in the “real world” (e.g. Anderson, 2004; Bushman & Anderson, 2001), the issue is simply one of whether these measures of aggression are predictive of criminally violent behavior outside the experimental setting. At this level, a number of critiques have suggested that evidence for the predictive use of these measures for real-world violence is limited (Freedman, 1992; Ferguson & Rueda, (in press); Savage, 2004). If one were to be more content with a broader, vaguer “aggression” construct, are the measures used to evaluate aggression valid for this construct? Much attention has focused on the validity of such measures (e.g. Carlson, Marcus-Newhall, & Miller, 1989; Tedeschi & Quigley, 2000) and their generalizability to “real-world” aggression.

That debate is premature. Discussion of the validity of a measure of any construct can occur only once its reliability has been established, and the reliability of a measure can be established only if the measure is standardized. None of the common laboratory measures such as the Buss Aggression-Machine and the Taylor Competitive Reaction Time Test (TCRTT; see Tedeschi & Quigley, 1996 and 2000 for a complete discussion of the various ad-hoc tasks used to measure aggression) are used in a standardized way in the literature, nor are reliability data commonly provided for these instruments. These measures of aggression are often varied both in administration (e.g., whether the tasks represents a real competition or whether there are two separate blocks, one with the participant as recipient of a noxious punishment for losing, one with the participant giving noxious punishment to a confederate) and in “scoring” the measure. For example, in the area of video game violence effects, Anderson and Dill (2000), Anderson and Murphy (2003), and Bartholow, Bushman, and Sestir (2006) all use a modified version of the TCRTT in a variety of different ways to measure “aggression.” In some studies, the TCRTT is used to produce multiple indices of aggression, which runs the risk of capitalization on chance, particularly when only some of those indices are ultimately found to be statistically significant but others are not (e.g., Anderson & Dill). Seldom is any form of reliability reported for these measures (test retest or internal consistency) in the studies that utilize them. It is impossible to address what a particular instrument is measuring if it is not used in a standardized way and if data on its reliability are not made available.

Let’s assume for a moment that the measures are standardized, reliable, and valid. Is performance on these measures associated with violent criminal behavior or some other form of broader aggressiveness in the real world? To date, there is no evidence to suggest that it is. Many experimental

dependent variables that are questionable regarding their relationship to “aggression” include asking children whether they wanted to pop a balloon (Mussen & Rutherford, 1961); asking college students whether they would like to have a graduate student confederate (who had just insulted them) as an instructor in a course (Berkowitz, 1965); interpreting the actions of a character in a story (Bushman & Anderson, 2002); and sentencing criminals in an analog scenario (Deselms & Altman, 2003).

Two issues emerge here. First, are these various tools used to measure aggression measuring the same construct? Second, is that construct referred to as “aggression” related to violent criminal behavior in the manner that many media violence researchers claim (e.g. Anderson et al., 2003)?

Regarding the first issue, there does not seem to be any reason to believe that various methods of measuring “aggression” are measuring the same construct. Many studies use multiple dependent measures of aggression and fail to report correlations between them, a fact noted by Carlson et al. (1989). The authors of this examination of the aggression construct itself use those studies that do report intercorrelations between dependent measures to examine the convergent validity between these measures. The authors claim “positive mean correlations” between measures that are statistically significant and suggest that this is evidence of convergent validity and, thus, the stability of the construct. However, the actual mean correlations between these measures are typically much lower than the accepted cutoff of $r = .4$, weak results for a validity outcome. Validity is not established through “statistical significance” per se but through the strength of relationship between measures, or between measures and outcomes, as a means of estimating a relationship between those measures and a hypothetical construct.

Regarding the second issue, as media violence researchers have themselves associated their research with violent criminal behavior (e.g. Anderson, 2004, Anderson et al, 2003), do measures of aggression, whether experimental or correlational, predict real-world violence? There are currently some excellent, structured, clinical measures of violence prediction available (e.g., HCR-20, Webster, Douglas, Eaves & Hart, 1997). These are measures that are standardized and reliable and have promising validity data. These are never used in media violence literature, nor do any of these measures include exposure to media violence among their actuarial indices. Given that some controversy continues over the degree to which these well-designed and reliable measures can accurately predict violence risk (Litwack, 2001), it is unlikely that unstandardized tools can accomplish what these clinical tools continue to struggle for. The methods employed in media violence validation are very indirect, often comparing the results of experimental and non-experimental studies to suggest that external validity is implied by “general agreement” between the two (e.g. Anderson & Bushman, 1997; Anderson, Lindsey, & Bushman, 1999). Importantly, no studies exist to

suggest that the various measures of “aggression” employed in experimental media violence studies are predictive of violent criminal behavior or any other external criterion. Despite protestations to the contrary (Carnagey & Anderson, 2004), there is simply no evidence to support the external validity of these measures. Carnagey and Anderson argue that external validity may not be a relevant issue or cause of concern (citing Mook, 1983), however, to the extent that experimenters wish to generalize their findings to the “real” world, as is often the case in media violence research, external validity, properly investigated, is of tantamount importance.

Inconclusive or Negative Findings Reported as Positive Findings

Part of the reason that the causal link between media violence and violent criminal behavior has become accepted by the scientific community and the general populace is because this body of literature has been presented as unequivocal, consistent, and uniformly positive. Perhaps the most striking is the American Academy of Pediatrics (AAP) testimony before congress (Cook, 2000) that “Since the 1950’s more than 3500 research studies in the United States and around the world using many investigative methods have examined whether there is an association between exposure to media violence and subsequent violence behavior. All but 18 have shown a positive correlation between media exposure and violent behavior.” This statement is a misrepresentation of the facts. Similar statements from the American Psychiatric Association and American Psychological Association provide scientists and laypersons alike, who are not familiar with the literature, the impression that thousands of conclusive studies exist. Although no reviews conducted by researchers familiar with the field make such claims, neither are they vocal in challenging this misconception.

Freedman’s (2002) review of the literature noted that there are actually approximately 200 empirical studies of media violence effects. This is still an impressive number, although nowhere near the figure cited by the AAP. Of greater concern, however, is that of the studies available that conduct empirical research regarding a link (correlational or causal) between media violence and actual violent behavior, more than half of them failed to support this link. From this analysis, it appears that, far from being “unequivocal,” the research is highly inconsistent.

It would appear that many of the studies that examine media violence effects employ multiple dependent variables (sometimes, as in Anderson & Dill, 2000, from the same instrument) of the same “aggression” construct. Thus, a number of separate analyses are conducted, some of which are significant, some of which are not. Seldom are Bonferoni corrections for multiple analyses applied to the results. To a large degree, these studies are capitalizing on chance. In the discussion of these studies, the disconfirming results are seldom mentioned, and focus is placed upon the results that are

significant. A study can be reported as “positive” when in fact the majority of its findings do not support the hypothesis. Thus, researchers beginning with an a-priori hypothesis seem to be “cherry-picking” results that support that hypothesis. As it will be argued here that the majority of scientists and laypersons alike do not read these studies carefully, this point goes largely unnoticed. The following paragraphs will provide several examples of this phenomenon from some of the most-often-cited studies on media violence effects; however, this problem appears to be endemic to much of the literature and is not limited to a few studies.

One often-cited study is that by Friedrich & Stein (1973), which implies that children who watch violent programs (such as *Batman* or *Superman*) are more interpersonally aggressive. The authors included five measures of aggression (including one composite of two of the basic aggression measures) and provide a number of analyses to attempt to support this view. Generally, the results did not support the hypothesis that exposure to violent programs increased any form of aggression, including hitting other children, verbal aggression, or fantasy aggression. The only significant finding was an interaction between initial aggressiveness and violent programs. However, had a Bonferoni correction for multiple analyses been appropriately applied (it was not), this finding would not have been significant. Furthermore, once gender was added to this analysis, this interaction was no longer significant. Thus, once gender is properly controlled, there were no significant findings to suggest that exposure to violent programs resulted in more violent behavior.

Similarly Lefkowitz, Eron, Walder, & Huesmann (1977) in their longitudinal study of aggression and television violence, use several measures of “aggression” including peer reviews, self-report, and the 4-9 scales of the MMPI (elevations on which are suggestive of “psychopathy” and are common among the criminally violent). Aside from the Minnesota Multiphasic Personality Inventory (MMPI), for which reliability and validity data are widely available (see Hathaway & McKinley, 1989), no reliability or validity data were reported for these instruments. The items on the peer review were questionable in their relationship to violent behavior, as many of the questions referred to things that clearly are related to a broader class of “naughtiness.” “Aggression” items included “Who does things that bother others?” and “Who gives dirty looks or sticks out their tongue at other children?” and “Who does not obey the teacher?” A quick perusal identifies that many items pertain more to disobedience, rudeness, and poor social skills rather than genuine aggression. Only two items—“Who starts fights over nothing?” and “Who pushes or shoves other children?”—are clearly related to physical aggressiveness. Not surprisingly, the results of this study are fairly tepid. For boys, television violence exposure was related only to peer-reviews, not to self-reported aggressive acts, nor to MMPI scores indicating psychopathy. No significant findings were found at all for girls, including on peer-reviews.

No measure of violent criminal acts was provided in this study. Thus, the results appear to be inconclusive at best but, by considering only the peer-reviews in a subsequent regression equation, the authors attempt to bolster their claim that television viewing is predictive of aggression. Interestingly enough, even here, the effect for girls is found to be significant but *negative*, suggesting that girls who watched more violent television were actually *less* aggressive according to their peers. Thus, a close and careful read of the results of this study do not support its conclusions.

More recently, in the violent video game literature, Anderson & Dill (2000) in their laboratory study of violent video game effects compute four methods of measuring aggression using the TCRTT (noise intensity and duration after both win and loss trials), thus capitalizing on chance, and failed to apply a Bonferoni correction to their analyses. Only one of the four measures of aggression (noise duration after loss trials) was reported as significant, although had a Bonferoni correction been appropriately applied, this index also would have been non-significant. Nonetheless, this study is oftentimes cited as one of the leading studies indicating a link between video game violence exposure and aggressive behavior in the lab. Examined closely, however, it appears to indicate quite the opposite. Similarly, Gentile et al. (2004) find significant results for a zero-order correlation between video game exposure and aggressive behavior. However, once gender is added as a predictor variable into a multiple regression equation along with video game violence and trait hostility, the resultant Beta-weights for video game violence exposure drop the relationship between video game violence and aggressive behavior to near zero. Nonetheless, this manuscript ignores its own statistical outcome to confirm a relationship between video game violence exposure and aggressive behavior.

The problems discussed in the aforementioned studies are not unique to them but endemic to the vast majority of studies in media violence effects in which multiple analyses are carried out. These authors have focused only on confirmatory information while ignoring information that does not support their hypothesis, resulting in misinforming casual readers of the conclusiveness of their results.

Media Violence Effects Literature Does Not Account for “Third” Variables

One of the key problems with the extant literature is the failure of investigators to take account of key variables that may explain both why individuals are attracted to violent media and why they behave violently. Although researchers sometimes claim to have controlled for “personality” or “family history,” this is simply not the case. Once again, many of the measures used lack reliability and validity. Even if that were not an issue, the measures used are generally substandard for the task at hand.

Controlling for “personality,” for example, is an admittedly large undertaking. However, most studies in media violence make no effort to control for personality at all. Those studies that do most often use a single “face-valid” measure of “trait aggression” (e.g., Anderson & Dill, 2000) such as the Buss Aggression Questionnaire (Buss & Warren, 2000). The problem with such measures is that they are typically “face valid,” meaning that their purpose is easy to figure out and thus lie upon. Not surprisingly, these relatively simple straightforward measures are seldom used clinically to predict violence risk. Probably the personality characteristic that has been best associated with violent criminal activity is *psychopathy* (Hare, 1993), a characteristic set of deviousness and lack of conscience that is very common among the violent criminal population. The best available measure for this personality trait is the Psychopathy Checklist (PCL; Hare, 1991). Few, if any, studies to date have controlled for this personality characteristic in the examination of media violence effects using the PCL or any other valid measure that has been associated with actual violent behavior.

Similarly, media violence studies rarely make a serious attempt to control for exposure to violence within the family. To the extent that environmental exposure can predict future behavior (that is to say, independent of biology), family violence is arguably a better fit for social modeling theories than is media violence effects, owing to the proximity and emotional closeness to the modeling source. Yet this factor is almost never considered in media violence literature. To the extent that authors claim that it is considered, a close inspection of the data reveals that this is not the case. Lefkowitz et al., (1977) claim to have considered variables such as parental neglect, although a careful read reveals that this information is based on the self-report of the parents themselves. Problems with such self-report behavior by parents on their own negative behavior should be evident. Johnson, Cohen, Smailes, Kasen, and Brook (2002) present one of the most impressive measures of child “neglect” in the available literature, although the methods used are less geared toward exposure to violence in the family than they are simple neglect (i.e., not helping with homework, not monitoring drug use). Unfortunately, their study does not distinguish between violent and non-violent television and fails to distinguish between disruptive psychiatric disorders and violent behavior. Furthermore, this study does not control for personality, which is surprising, as the sample involved seems to be the same as used for the authors’ other work on personality disorders (Johnson, Smailes, Cohen, Brown, & Bernstien, 2000). The majority of studies (e.g., Anderson & Dill, 2000; Belson, 1978; Huesmann & Eron, 1986) simply make no effort to include this important variable. It is difficult to understand the unique contribution of media violence to violent behavior while studies leave family violence variables uncontrolled.

Biology represents a third variable that has remained uncontrolled in the current body of media violence literature. Several recent analyses have

indicated that genetics plays an important role in the development of anti-social personalities (Blonigen, Carlson, Krueger, & Patrick, 2003; Larsson, Andershed, & Lichtenstein, 2006), with shared environmental influences playing comparatively little role in the development of these traits. To date, no studies have considered genetic heritability in the context of media violence effects. This powerful influence on antisocial behavior has not been included in theoretical models of media violence effects (e.g. Bushman & Anderson, 2002), and genetic effects have never been controlled for in any study of media violence effects. Given studies indicating a dominant role for genetics over environment in regards to personality development (Bouchard & McGue, 2003; Bouchard & Loehlin, 2001), this is a major oversight.

THEORETICAL PROBLEMS WITH THE EXTANT MEDIA VIOLENCE LITERATURE

The Medical Research Analogy is Inappropriate

Media researchers have, in recent years, increasingly attempted to associate their research with medical findings including cigarette smoking research (Bushman & Anderson, 2001) and aspirin's effects on heart attacks (Anderson et al., 2003). This analogy is inappropriate and misleading on a number of counts.

Bushman & Anderson (2001) compare the effects of media violence on aggression to the research regarding cigarette smoking and lung cancer, and assert that the effect size for media violence is almost as high as that for cigarette smoking. This is actually not true. Using the coefficient of determination r^2 from data provided in their own article, the effect size for media violence ranges between 1.2% and 9.6%, whereas that for cigarette smoking and lung cancer is closer to 16%, a significant boost. More important, cigarette smoking causes death through multiple outcomes (stroke, heart attack, emphysema, exacerbation of other chronic illnesses such as diabetes) where there is no similar multiplicity of outcomes for the media violence-violent behavior link. By considering only the link between cigarette smoking and lung cancer specifically, the authors capitalize on the likelihood that the reader will not consider that the effect size for the relationship between cigarette smoking and *death* is likely much higher than 16%. Furthermore, cigarette smoking is a necessary and sufficient cause of lung cancer and morbidity in general (lung cancer is extremely rare in those not subjected to inhaled carcinogens). Media violence is neither a necessary nor sufficient cause of violent behavior (Ferguson, 2002). Lastly, Bushman and Anderson's (2001) calculations for the smoking/lung cancer have been recently challenged, with Block and Crain (2007) suggesting that the effect size (r^2) closer to 80%.

Anderson et al. (2003) mention several medical outcomes that are “deemed important by the medical community” (p. 81). As Rosenthal (1995) notes, many medical research studies that receive a lot of public attention, including the aspirin–heart attack link, actually have quite trivial results (some have a 0% effect size). Comparing psychological results to these medical results is not a defense of psychological research but an excoriation of medical research. At the same time as Block and Crain (2007) note, these calculations may themselves be wrong, as it is difficult to translate medical research into psychological effect sizes such as r . These data are a better fit to the American Cancer Society’s (2007) data suggesting 87% of lung cancers can be attributed directly to smoking.

Another issue left unmentioned is that medical research does have one advantage psychological research does not. The dependent variables commonly used in medicine have “perfect” validity, such as mortality. To be blunt, there is little reason to worry whether death is a valid indicator of death. No gap exists between the measured variable and a “hypothetical construct” for much of the medical literature. Thus, comparison to medical literature is comparing “apples to oranges.” Medical analogies are simply inappropriate and should be avoided by psychological researchers.

Media Violence Research Does Not Match Real Life Events

For a theory to be supported, it must be able to explain events in the “real world” better than other existing theories. It is common for media violence publications to invoke the steady increase in violent criminal behavior in the United States beginning in the 1960s as evidence to support the passive-modeling theory of media violence effects (see Anderson, 2004; Anderson et al., 2003; Bushman & Anderson, 2001). However these authors often fail to look at societal violence in a broader context than simply in the years between 1965 and 1995 in the United States. As these authors imply that the observed concurrence between a rise in media violence and a rise in violent crime in the United States during those three decades is evidence in support of their model (or at least allow the reader to insinuate the same), so too can a broader understanding of the relationship between the introduction of media violence and societal violence provide evidence in support of falsification of the passive-modeling theoretical paradigm.

To use an analogy (recognizing that analogies are seldom perfect), were physicists to develop a theoretical model that suggested the sun were to turn blue to the naked eye every Thursday, the ultimate test of this model is observance of its predictions in the “real” world. Though the mathematics behind the theory may be perfect and laboratory results support the theory, if the sun does not indeed turn blue, the theory is falsified. Physicists might suggest that their theory is not the only factor involved in determining the color of the sun, but unless their theory can be modified to account for the

discrepancies between their predictions and actual observable phenomenon, the theory is inadequate and may be considered falsified. Yet when it is noted that the predictions of the passive-modeling theoretical paradigm do not hold true to “real-life” events, media violence researchers fall back on the “other factors” defense without explaining what those other factors could be and how they fit into their theory (e.g. Anderson & Bushman, 2002; Huesmann & Taylor, 2003). This is a retreat to an unfalsifiable position, namely that their passive-modeling theory never need actually predict social violence because if it does not, this discrepancy can simply be “rationalized” away via some unexplained and unidentified set of variables. A theory that is not held up to the burden of predicting events that it says it does (including events the authors themselves discuss in their own writings) is one that has become unfalsifiable and, thus, unscientific.

Cross nationally, the media violence–violent crime link does not hold. Notably, although Huesmann & Eron (1986) noted that the consumption of violent television is similar across the nations examined in their study, violent crime rates were very different. More important, in that study, the link between television violence and aggressive behavior, once examined using multiple regression, proved unreliable, demonstrating significance only for American girls, and Israeli city children (but not children in an Israeli kibbutz) but not for boys in the United States, girls in Poland or Finland, or children of either gender in Australia, the Israeli kibbutz, or the Netherlands (Moeller, 2001; Wiegman & Kuttschreuter, 1992). Boys in Finland and Poland showed a significant relationship between aggression on an odd composite measure of television exposure and identification with more aggressive role models but tellingly showed no correlation between aggression and television violence exposure itself. More broadly, an examination of violence rates across countries notes that other nations such as Canada, Japan, England, Finland, Australia, and the like, which share our rates of violent media consumption (as Huesmann & Eron, 1986, agree) have widely different violent crime rates, and even within a single country such as the United States, different ethnicities experience much different crime rates (World Health Organization, 2002). Thus, different nationalities, and even subgroups within the United States, are experiencing very different rates of violent crime, despite having essentially the same media violence consumption levels. The passive-modeling theoretical paradigm simply does not explain this.

Of perhaps more serious concern is the inability of the model to explain violent criminal behavior in the United States. Authors such as Bushman & Anderson (2001) make reference to the surge in criminal behavior beginning in the mid-1960s, ignoring more serious crime waves that occurred in the 1930s and 1800s (before the advent of modern mass media). Violent crime rates in the 1930s and 1800s appear, per capita, to have exceeded those ever experienced in the crime surge between the 1960's and early 1990s (Bureau of Justice Statistics, 1988; National Commission on

the Causes and Prevention of Violence, 1969). More recently, a steady and dramatic *decline* of violent crimes in the United States has occurred, despite no appreciable decline in the consumption of violent media (Federal Bureau of Investigations, 1951–2000). The passive-modeling theory of media violence, in the face of these data, simply does not fit.

Given the lack of fit between the passive-modeling paradigm of media violent effects and the experimental and correlation data regarding these effects and violent crime, it may be time to move past the common assumption that media violence is capable of teaching violent criminal behavior through social modeling. What directions can the research then take to improve upon current understanding and what theoretical models can be derived to guide future research?

DIRECTIONS FOR A FUTURE SCIENCE OF MEDIA EFFECTS AND VIOLENT CRIME

Given that the majority of media violence researchers assert that media violence is a direct cause of violent criminal behavior and those skeptical of this approach suggest that any relation between media violence and exposure and violent crime is merely the symptom of a deeper causal factor such as biology or family violence, this section provides practical suggestions for assisting researchers in resolving the relevant debates.

Use of Standardized, Reliable Measures

The debate on the effects of media violence on violent crime will remain unresolved so long as the evidence proffered depends on unstandardized, unreliable measures such as the TCRTT. This is unfortunate as the possibility remains that some measures such as the TCRTT *could* be standardized and tested empirically for reliability and ability to predict violent criminal behavior. As noted earlier, experimental procedures for studying the effects of media violence on aggression are particularly weak in this regard and would benefit greatly from such measures. Developing such standardized measures would greatly strengthen the claims of researchers either in favor of or against the relationship between media violence and violent criminal behavior. Although this article focuses on the reliability and validity of measures of aggression, measures of media violence exposure present the same concern and should be held to the same standards.

Obtain Empirical Data on Media Violence Rates

As of yet, no empirical evidence has been provided to document fluctuations in media violence across the twentieth century. Although such fluctuations

likely occurred, understanding their relationship with violent crime remains impossible until they are empirically documented. Once fluctuations in media violence rates are known, they can be examined for any relationship to fluctuations with actual violent crime. If such relationships exist, this strengthens the argument for a relationship between the two phenomena. Conversely, if little or no relationship exists, the argument that media violence is associated with violent crime should be discarded.

At Risk Groups?

One possibility that exists is that exposure to media violence has little to no effect on the majority of the population but that small groups of individuals may be affected. The root cause of violent criminal behavior in such individuals may be a biological predisposition, but exposure to media violence could act as a trigger or “catalyst” for violent criminal behavior. As such, the debate over global effects of media violence exposure on society may be less fruitful than identifying at-risk groups of individuals for whom special care might be taken to limit exposure to media that could play a role in subsequent violent criminal behavior. Thus, it may be beneficial for researchers to develop programs to identify who, if anyone, is most at risk for violent criminal behavior, subsequent to exposure to violent media.

Examining the Link Between Family Violence and Media Violence

Whether violent families are the root cause of both a preference for media violence and violent crime is an issue that remains poorly addressed. (Ferguson et al., 2008) Relatively simple regression models could be employed with the proper data to examine whether media violence exposure retains predictive value once family violence exposure is controlled for. Such data could serve to either confirm or disconfirm an alternate explanation for any link between media violence exposure and violent crime.

Integrating Study of the Effects of Genetics into Media Violence Research

Given research suggesting that biological factors influencing violent criminal behavior are stronger than environmental factors (Blonigen et al., 2003; Hare, 1993; Larsson et al., 2006), it would be of great value to utilize more sophisticated theoretical models and hypothesis testing to integrate research on media violence with research from biology and genetics. Specifically, once the genetic variance in violent criminal activity is accounted for, does media violence exposure provide any further predictive value? Addressing such issues may help us to understand whether the contribution of media

violence to violent crime is causal or whether biological factors lead both to violent crime and a preference for violent media.

IMPLICATIONS FOR FORENSIC PSYCHOLOGISTS

Forensic psychologists may be called upon to provide information regarding media violence research in two main contexts, those being as an expert witness in legal cases and in providing statements to the media or public. Legal cases may include both criminal defense cases and (more often) civil cases related either to legislative attempts to block access to violent media at certain times or to certain groups (usually minors) or liability cases (torts) in which a media company is accused of responsibility for a criminal act owing to influencing the criminal's behavior. These recommendations made here are consistent across both contexts. Generally, I argue here that the psychological community has been remiss in failing to adequately inform both the legal system and the public regarding significant problems with the research on media violence effects. I submit here that psychologists have an ethical responsibility to provide full disclosure of research limitations to the courts and public so that an informed conclusion can be reached. Related to this issue, the following suggestions are made.

Discussing Causality

Forensic psychologists must be aware that the courts have adopted a high standard for determining causality in social science research regarding media violence effects (e.g. *ESA, VSDA and IRMA v. Blagojevich, Madigan, and Devine*, 2005). Current precedent has been consistent in finding that this causal relationship has not been proven by existing research. Some authors (e.g. *Gentile, Seleem, & Anderson*, 2007) have argued in favor of a less-rigorous standard of causality, which they refer to as "probabilistic" causality, rather than the use of the "necessary and sufficient cause." The essence of this lower threshold for causality is that any statistical relationship, no matter how small in size, is sufficient for demonstrating causality when based on the use of "probability statistics" such as those commonly employed in the social sciences. This differs from the "necessary and sufficient" causality where in a clear relationship between cause and effect has been consistently demonstrated. Necessary and sufficient causality represents a higher threshold and has been demonstrated in cases such as with smoking and lung cancer (*American Cancer Society*, 2007) wherein lung cancer is rare among non-smokers (the remaining cases are explained through genetic inheritance, a second necessary and sufficient cause).

The problem inherent in the lesser probabilistic variant of causality is that, owing to issues related to invalid measures of aggression, publication bias in academic publications (Ferguson, 2007) and the selective interpretation of inconsistent results, probabilistic causality can be based on poor data with weak effect sizes. Thus, almost any relationship could be said to be “causal” under such a low threshold, sowing great confusion in the public and reducing confidence in statements made by psychologists. Put another way, the probabilistic causal approach is fundamentally unfalsifiable and thus non-scientific. Forensic psychologists must take care to explain how causality is demonstrated in research and how research with weak results can be too easily corrupted, limiting our ability to have confidence in causal relationships.

Defining Aggression

Arguably when most non-scientists think of aggression, they likely picture children or adults attacking each other physically. Many people would be surprised to learn that aggression measures used in media violence research seldom examine such behavior. Indeed validity problems with many aggression measures have been noted (Ferguson & Rueda, (in press); Henry & Metropolitan Area Child Study Research Group, 2006; Ritter & Eslea, 2005). It is imperative that these issues be discussed openly and honestly in court proceedings and with the public. Not to do so is to allow non-scientists to form opinions based on misinformation. The conclusions that jurors, judges, media representatives, and the public make about the meaningfulness of media violence research may vary considerably based upon their knowledge about how aggression has been measured in research. For instance, aggression measures with greater validity tend to show smaller effects (Ferguson & Kilburn, (in press); Paik & Comstock, 1994) with effects for violent crime as an outcome near to zero. Measures used in media violence seldom include those used in violent risk assessments, and reasons for this discrepancy should be acknowledged.

Thus, an honest accounting of media violence research would include defining aggression and briefly detailing how aggression tends to be measured. An appraisal of the difficulties researchers have in measuring aggression is an essential component of any disclosure of relevant research results.

Acknowledging Weak Effect Sizes

Ultimately, there is very little that we still know about violence causation. What we do know is that, despite an active attempt by some researchers to promote the hypothesis that media violence promotes societal violence, there is little evidence to document this belief. Ignoring that the research

has employed poorly validated measures of aggression and tends to ignore disconfirmatory results, the effects seen in the research are not very large. Some authors (Bushman & Anderson, 2001) have attempted to suggest that the effect sizes for media violence and aggression are similar to those for smoking and lung cancer. However, as discussed earlier, these calculations appear to be mistaken and don't fit with the American Cancer Society's (2007) data. Such claims are misinformative and risk confusing the public's perception of research. Making exaggerated claims ultimately risks eroding public confidence in psychology and statements made by psychologists. Conversely, such statements may also give the public the mistaken impression that data on smoking and lung cancer are much weaker than they actually are.

Giving Testimony on Media Violence

Forensic psychologists are most likely to be called as expert witnesses related to the potential impact of media violence exposure on viewers in relation to cases in which a defendant is using media violence as an explanation for reduced culpability or in which legislation has been enacted to "protect" minors. It is incumbent upon expert witnesses to be well versed in regard to both sides of the debate on media violence effects and to acknowledge that a lack of consensus exists within the social science community regarding either side of the debate. Previous judicial decisions have questioned the objectivity of some social scientists who have given testimony (e.g., *ESA, VSDA and IRMA v. Blagojevich, Madigan and Devine, 2005*), a trend that could harm the credibility of social scientists if allowed to continue.

In most cases, an expert witness will be expected to give testimony "friendly" to one side of the debate, either pro-media or anti-media. There is, of course, nothing either unethical or scientifically questionable about taking either side in this debate, so long as both sides are acknowledged and sufficient information is provided for jurists or courts to make a reasoned, well-informed decision. Giving an honest appraisal of the research also increases credibility during cross-examination. Forensic psychologists may also act as consultants during cross-examination, if other expert witnesses give testimony that is scientifically questionable.

As a facet of human nature, it is expected that an expert witness would naturally argue in favor of one side of the debate as having greater merit. However, ethical boundaries are approached when testimony ignores or denies the existence or relevance of opposing views or fails to acknowledge significant methodological limitations of the existing research. Arguably the AAP's testimony regarding the number of research reports on media violence (Cook, 2000) is an example that approaches this line. The original testimony could have been made in good faith, which would not be

unethical even if mistaken. However, to the knowledge of this writer, the AAP has declined to correct their statements, despite a clear discrepancy with the actual number of existing research reports (Freedman, 2002). The risk is that jurists, courts, legislators, and the public have been *manipulated* (rather than informed) into viewing the media violence debate through a politicized lens based on faulty data.

Thus, from either “side” of the media violence debate, expert witnesses are on firmest ground when acknowledging the relevance of the “other side” (or at very least its existence) and noting limitations of what is known about media violence effects and what is not known. Expert witnesses are on less certain ground when they mistake their passionate advocacy for a given position as tantamount to scientific “truth.”

On a related issue, it is here advised that “ad hominem” attacks, which have become prevalent in the media violence debate, add little to the discussion and likely serve to confuse court testimony. For instance, in scholarly writings (Gentile et al., 2007; Huesmann & Taylor, 2003), anti-media writers have taken to implying that their opponents are “shills” of media industry, despite that most skeptics of the media violence debate are not affiliated with media companies. Such comments ignore also that many anti-media scholars affiliate with or are members of anti-media advocacy organizations, and the “credibility” question could easily cut both ways. Bottom line is that it is the data that should be discussed both in scholarly forums and in the courts. Ad hominem attacks are to be expected from counsel, of course, but are generally recognized as a faulty aspect of scientific discourse.

Speaking with the News Media

It is likely that news outlets will contact forensic psychologists, oftentimes after a tragic violent event. More often than not, I would argue, news outlets are particularly interested in salacious quotes from an expert that will help propel hysteria. Less interested are the news media in “it’s no big deal” responses from experts. This is to be expected and is a perhaps inalterable facet of the marketability of news media (i.e., bad news sells better than good). As with giving court testimony, it is incumbent upon forensic psychologists to do their best to provide rational and technically correct information. For instance, in some news reports, I still see it insinuated that Seung-Hui Cho, the Virginia Tech shooter, played violent video games, despite that the Virginia Tech Review Panel (2007) found that he did not and no evidence exists to refute this in-depth investigative report.

In one recent unfortunate incident, the psychologist Cooper Lawrence discussed the video game *Mass Effect* on Fox News and compared some scenes in the game to pornography (Gamepolitics.com, 2008). It quickly was revealed that Dr. Lawrence had not actually played the game and ultimately recanted her statements, which were erroneous. It’s quite possible that

Dr. Lawrence was “tricked” in some way by Fox News, yet she also bears responsibility for commenting on a medium with which she was not familiar. This is particularly relevant to emerging media such as video games and the Internet, wherein many commentators (including psychologists), particularly society’s “elders,” are giving testimony on content that they have not personally viewed or have become familiar with. For instance many social scientists and other speakers still comment about games such as *Grand Theft Auto* as “awarding points” for shooting police officers and other antisocial acts. However, almost no modern video game “awards points” for anything anymore but rather seek to tell complex stories. In fact, games such as *Grand Theft Auto* and *Bully* include severe ramifications for antisocial acts (such as more police officers coming in greater amounts, ultimately to shoot the player’s character), although antisocial acts are not specifically prohibited. Comments such as these are immediately viewed as uneducated, moralistic and non-credible by individuals actually familiar with modern media content, and owing to such irresponsible comments, the psychological profession loses credibility with wide swaths of the general population. It is perfectly reasonable to take a moral or scientific stance on the acceptability of impact of such controversial content, so long as such commentary is based on fact and experience rather than innuendo or uninformed judgment.

Several authors have recently suggested that scientists abandon their cautious and conservative talk with the media (Gentile et al., 2007) in favor of more conclusive causal statements. Given that such causal attributions appear to be unwarranted from the existing body of literature, it is unfortunate to see scholars encouraging other scholars away from the normal restraint necessary given the limitations of all social science research, and media violence research specifically. It is likely to be lost on the public that Gentile et al. adopt a very weak definition of “cause” based on (arguably flawed) probability statistics rather than the clear and consistent standards used in court proceedings and the scientific community at large.

Correcting the Hysteria

It is recognized up front that social scientists, including forensic psychologists, likely fall into three general camps. The first of these, the anti-media advocates, have become ensconced in the “causality” hypothesis, arguably valuing advocacy over objectivity (although I might acknowledge that objectivity is more an ideal to strive for than a consistent human state). Adherents to the first camp have generally presented the debate as one-sided, ignore skeptical views, and have advocated the unlikely comparison between media violence effects and smoking/lung cancer research (see Anderson, 2008; Gunter, 2008; Savage 2008).

The other two groups consist of skeptics regarding the relationship between media violence and aggression and scholars who believe that

media violence may promote aggression but who also understand the limitations of social science research and its ability to prove causality. Though differing in opinion regarding media violence effects, these two groups may share an interest in reasoned debate and the ideal of objectivity over advocacy. Both of these groups have a shared interest in reasoned discussion and a clear presentation of the existing research on media violence effects.

In a general sense, forensic psychologists on both sides of the debate can reduce polemics and unnecessary hysteria by acknowledging that media violence effects, far from being comparable to smoking and lung cancer research, are a minor influence on aggression at best (Savage, 2008). Thus, media violence research should be put in the perspective of other potential causes of violence which have been shown to have much higher effects (see Savage for a discussion). Of course, neither the "it's no big deal" or "it may have some minor influence" perspectives are likely to generate the kind of media attention as the comparisons with smoking and lung cancer do, yet it is incumbent upon the social science community to maintain the cautious approach to interpretation that has been its historical purview.

Related to this issue, one thing that forensic psychologists can do is to seek to correct misstatements or factual errors proclaimed by politicians or, indeed, other social scientists. This has been done already several times. For instance Block and Crain (2007) have corrected Bushman and Anderson's (2001) effect size estimates for smoking and lung cancer research. In September of 2002, 33 media scholars (including myself) joined in an *amici curiae* brief in a Missouri video game legislation case, questioning the causal statements made regarding the impact of video game violence on aggression (Brief *Amici Curiae* of Thirty-Three Media scholars in *Interactive Digital Software Ass'n, et al. v. St. Louis County, et al.*, 2002). In regard to the AAP's comments on media violence, including estimating the number of studies at around 3,500, eight media scholars and several First Amendment advocates (Heins et al., 2001) called on the AAP to correct the erroneous statements made. In this regard, scholars have connected well with both First Amendment and criminal justice advocacy groups such as the Free Expression Policy Project and the Justice Policy Institute. Such groups are likely to be able to "get the word out" better than are individual scientists. However, my cautionary note about *ad hominem* attacks kept in mind, it is recommended that scholars refrain, in particular, from financial relationships with advocacy groups or membership in such groups, as such associations invite (perhaps unfair and hypocritical) *ad hominem* attacks in a political debate as furious as that on media violence effects.

In closing, it is my argument that forensic psychologists should acknowledge that the link between media violence and societal violence, aside from the issue of validly measuring aggression, is not very strong. It may be that societal efforts would be better directed at examining other influences that have a stronger effect on violent behavior such as poverty, family

violence or genetics. A direct and honest appraisal of media violence effects may assist courts and the public in putting such data into perspective. Such an appraisal would likely service to prevent unnecessary public hysteria and promote justice in the courtroom.

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