

CHAPTER 3

Media Violence Effects and Violent Crime

Good Science or Moral Panic?

Christopher J. Ferguson

Whether exposure of children or adults to violent media is a cause of aggression and violent behavior has been one of the most intensely debated issues in criminal justice and the broader populace. Debates about the effects of media ranging from books to video games have a long history (Trend, 2007). Even religious writings such as the Bible have been the target of criticism, from early Christian writings in the Roman Empire to “native” language translations of the Bible in the late medieval period. In fact, the Bible recently came back in the spotlight with a study suggesting that reading passages from the Bible with violent content provokes aggression in the same manner as violent video games or television allegedly do (Bushman, Ridge, Das, Key & Busath, 2007). The 20th century has seen many other examples, from Harry Potter teaching witchcraft, to the concern (largely evaporated) that playing Dungeons and Dragons would lead to Satanism or mental illness, to the Hays Code “taming” of Betty Boop (which, by forcing her to put on more clothes, doomed the comic strip). Concerns have come and gone that media such as comic books, jazz, rock, rap, role-playing games, and books, as well as television and movies, would lead to waves of rebelliousness, violence, and moral degradation. New media such as video games and the Internet inevitably

stoke the flames of fear with waves of advocates and politicians expressing concern over the fate of supposedly vulnerable children and teens.

Opinions on the matter of media violence effects are wide ranging. Some scholars (Anderson et al., 2003) claim that media violence effects have been conclusively demonstrated, so much so that the certainly equals that of smoking and lung cancer (Bushman & Anderson, 2001). By contrast, other scholars have claimed that the entire media violence research field has been mismanaged, with weak, inconsistent results; poor measures of aggression; a mismatch between the theories and actual crime data; and failure to consider alternative causes of aggression such as personality, evolution, or family violence (e.g., Freedman, 2002; Olson, 2004; Savage, 2004). Several medical doctors have recently questioned the data behind the supposed similarities between media violence research and research on smoking and lung cancer (Block & Crain, 2007), and indeed, as demonstrated in Chapter 1, the effect sizes for smoking and for media violence are nearly on opposite sides of the spectrum. Wherein lies the truth? I suspect that, as happens all too often in the social sciences, “truth” is subjective. With that in mind, it is the goal of this chapter to discuss, bluntly and directly, the research on media violence. I will discuss not only what study authors say they found but how they measured constructs such as aggression, and I will examine their results in greater detail than has been customary in most reviews. The goal is to give the reader an “insider” view of media violence research, from a media violence researcher, so that readers can construct their own informed opinion.

CASE STUDY: VIRGINIA TECH

On the morning of April 16, 2007, the Virginia Tech campus in Blacksburg, VA became the site of the worst school shooting in American history. The attacks began at approximately 7:15 a.m., when two students, Emily Hilscher and Ryan Clark, were shot and killed in a dorm building. At the time of this writing there is no evidence that the shooter, Seung-Hui Cho, had a prior relationship with either of these individuals or any other of his victims. These shootings, like the rest, appear to have been fairly random.

Cho then mailed a “manifesto” to NBC, including videotapes he had taken of himself ranting and posing with weapons. The final massacre in Norris Hall occurred two hours after the initial shootings. The Virginia Tech campus has subsequently been criticized for communication failures in failing to adequately warn students about the initial shootings. Warning students that a shooting had occurred or canceling classes might have prevented or reduced the number of subsequent deaths. However, in all fairness, it is likely that many similar institutions would have stumbled under similar shocking and unforeseen circumstances. Most of us are just not prepared, outfitted, or equipped to deal with events as rare as this one.

Cho then entered Norris hall wielding two handguns and chained shut the main exit doors. Cho went to the second floor of the building and began the second, much more deadly portion of his massacre, shooting faculty and students in their classrooms. Nine minutes later, 30 people were dead (32 dead total) and 17 had been wounded. There were individual stories of bravery during the shooting, such as Professor Liviu Librescu, who barricaded a classroom door with his own body while most of his students were able to escape through a window. Librescu was killed after being shot through the door. Police responded to the scene swiftly but initially had difficulty entering the building due to the chained doors. As police entered the building, Cho killed himself with a gunshot to the head.

Within hours of the massacre, before the name of the perpetrator had even been released, several pundits had begun suggesting that violent video games were behind the massacre. Jack Thompson, a Florida lawyer and anti-video game activist, blamed video games for teaching children to kill. Dr. Phil McGraw (Dr. Phil) appeared on *Larry King Live* to assert that violent video games and other violent media are turning children into mass murderers. The *Washington Post* included a paragraph suggesting that Cho might have been an avid player of the violent game "Counter-Strike," and then quickly removed that paragraph from an online article without explanation.

None of these assertions proved true, however. In fact, in the final report by the Virginia state review panel commissioned by the Governor, Tim Kaine, video games were entirely and specifically exonerated. Cho, it turned out, was not a gamer. In fact, unusual for a young male, there was little evidence to suggest that he played video games at all, aside perhaps from the nonviolent game "Sonic the Hedgehog" (Virginia Tech Review Panel, 2007). The review panel stated that "He was enrolled in a Tae Kwon Do program for awhile, watched TV, and played video games like Sonic the Hedgehog. None of the video games were war games or had violent themes. He liked basketball and had a collection of figurines and remote controlled cars" and "Cho's roommate never saw him play video games." There were other indications that all was not well with Cho: a long history of mental health problems and stalking behavior toward two female students. Yet, if Cho was odd in any respect in his video game playing habits, it's because he played them rarely and violent games not at all.

Research Methods in Media Violence

If you are curious whether media violence contributes to violent crime, the simple answer to that is we really don't know. In defense of media violence researchers, there are some very good reasons for this. Foremost among them is that studying violent crime experimentally—that is to say, attempting to manipulate some research participants into committing violent crimes—is clearly unethical. That leaves us with correlational research only (e.g., self-reported violent acts or arrest records). Media violence researchers have responded to this experimental problem by instead studying aggression; because not all aggressive acts are illegal or particularly damaging to others, they can ethically be studied experimentally. If studies can experimentally demonstrate a causal effect of media violence on aggression in the laboratory and media violence is correlated with violent

crime in the real world, then an argument can be made that the two phenomena are similar enough to warrant concern.

If we can't ethically examine violent behaviors, how can we measure aggression in the laboratory? One common method for measuring aggression in the laboratory (I've used it myself) is the modified Taylor Competitive Reaction Time Test (TCRTT; Anderson & Dill, 2000; Ferguson, Rueda, Cruz, Ferguson, Fritz, & Smith, 2008). After being exposed to some form of media (e.g., either a violent or nonviolent television program or video game), research participants are told that they will play a reaction time game against a human opponent. In this game, participants are instructed to press the mouse button as quickly as they can whenever a central square on their screen turns red. They are told that their opponent is also trying to press his mouse button quickly (two computers are supposedly linked up through Ethernet or similar connection and are playing against each other). Before each trial, the human participant is told that he or she can set a noise blast punishment for his or her opponent should the opponent lose. This noise blast can be set (from 0 to 10) in terms of both intensity (loudness) and duration. Even the loudest settings are not painful to the human ear; rather, they are more irritating, like the white noise of a television set. Naturally, the opponent is also supposedly setting punishments that the research participant will receive should he or she lose the match. The punishments can be reset after each match, and there are approximately 25 matches in total.

In reality, of course, there is no human opponent, and the participant is just playing against the computer. In theory, people who set louder and longer noise blasts for their supposed opponent are behaving aggressively. This isn't really a measure of violence because the noise blasts obviously aren't damaging, but how does it function as a measure of aggression? It seems intuitive, but despite years of use, the measure has never been shown to be predictive of real-world aggression, let alone violent crime.

One problem with the TCRTT is that, in the past, it has not been used in a standardized way. There are actually many ways to measure aggression with this test: You could measure the number of punishments that are above a certain arbitrary level (say 8 out of 10), or you could take the mean of all 25 matches, or you could just use the mean after win trials or the mean after lose trials. With a little creativity, you could likely think of dozens of ways to use the test to measure aggression, and this is not a good thing. This means that the test lacks standardization. Without a standardized test, researchers can measure aggression however they want and, indeed, can pick the outcomes that best support their hypotheses and ignore outcomes that don't support their hypotheses.

These kinds of problems with laboratory measures are not unique to the TCRTT, and some scholars have questioned the validity of all laboratory measures of aggression (Tedeschi & Quigley, 1996). Aside from instruments such as the TCRTT, other laboratory measures of aggression have included 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), asking subjects to interpret the actions of a character in a story (Bushman & Anderson, 2002), and asking subjects to sentence criminals in an analog (i.e., made up) scenario (Deselms & Altman, 2003). To study aggression in children, researchers can observe children at play, although it has proven difficult to distinguish between aggressive play (e.g., playing cowboys and Indians) and true aggression (e.g., pushing a child down to steal lunch money).

Both correlation and experimental designs can make use of surveys. Surveys may include self-reported violent criminal activity, self-reported aggression, or symptoms of a psychiatric disorder related to crime, such as antisocial personality disorder. To study young children, parent report measures can be used. Child peer ratings of aggression have also been attempted, but it is not entirely clear whether children have enough insight to actually rate each others' aggressive behaviors rather than turn any negative-sounding set of questions into a popularity contest. Many surveys, such as the Buss Aggression Questionnaire (Buss & Warren, 2000; a measure of aggressive personality traits), are standardized and reliable and have demonstrated validity. One obvious problem with survey measures is that people can easily lie on them. Also, it is not enough to merely label a set of questions "aggression"; they must be tested for validity. For example, Table 3.1

Table 3.1 Items From the Lefkowitz, Eron, Walder, and Huesmann Measure of Aggression

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1. Who does not obey the teacher?
 2. Who often says, "Give me that"?
 3. Who gives dirty looks or sticks out their tongue at other children?
 4. Who makes up stories and lies to get other children into trouble?
 5. Who does things that bother others?
 6. Who starts a fight over nothing?
 7. Who pushes or shoves other children?
 8. Who is always getting into trouble?
 9. Who says mean things?
 10. Who takes other children's things without asking?
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presents a list of peer-rating questions used in some television studies of aggression (Lefkowitz, Eron, Walder, & Huesmann, 1977). Many of the items appear related to naughtiness, but only a few involve actual violent behaviors.

Aside from the validity of aggression measures, one other issue that bears mentioning is the absence on most aggression measures of a clinical cut-off. A *clinical cut-off score* is a score above which a person likely has a particular disorder. For instance, the Minnesota Multiphasic Personality Inventory (a common test for mental illnesses) uses clinical cut-off *t*-scores of 65 (a *t*-score mean is 50, with standard deviation of 10) to indicate the likely presence of mental health problems. A person who scores under 65 is within the “normal” range; above 65 a person is at increasing risk for a mental disorder. Thus, if you were to take a sample of individuals and expose them to some phenomenon (say, media violence) and their scores went from a normal average of 50 past the clinical cut-off to a mean of 70, it would be reasonable to suggest that exposure to this phenomenon put them at significant risk for a mental health problem. Most aggression measures, even well-researched ones, don’t have a clinical cut-off, however. Thus, even if one group scores higher on a measure than another group, does that mean that the first group is at risk of becoming aggressive? This is particularly important because effect sizes in media violence research tend to be very small (with *r* values typically ranging from 0 to .2). If Group A is exposed to media violence and their mean aggression scores are found to be a *t*-score of 52, whereas Group B is not exposed to media violence and maintains the typical mean *t*-score of 50 (and these differences in score are about typical for media violence research), can we really say that media violence has “caused aggression” if none of the participants is pushed over any clinical cut-off?

Theories of Media Violence

Historically, there have been two main approaches to understanding potential media violence effects: the social learning approaches and the catharsis model. In recent years, most researchers have preferred to work from the social learning model. Briefly, this model suggests that individuals are likely to imitate what they see. For instance, a child learning to tie her shoes is likely to first watch an adult do it and then attempt to model the viewed behavior. Social learning models of aggression, such as the General Aggression Model (Bushman & Anderson, 2002), suggest that watching violent media leads to the development of violent *scripts*. People who watch more violent media develop more and stronger violent scripts than those who do not consume violent media. In real life, when presented with

hostile or even ambiguous circumstances, people with more violent scripts are more likely to respond violently. Although such models may allow for individual differences due to biology or personality, biology and personality are seldom discussed much in these models, so they are, by and large, *tabula rasa* models (meaning they consider everyone to be about equal or “blank slates” prior to environmental learning).

By contrast, catharsis models suggest that aggression is primarily a biological drive that requires expression (Lorenz, 1963). According to the catharsis model, media violence may provide an outlet or release for aggressive drives. As such, people who consume violent media would be expected to become less aggressive. Many media violence researchers today take a dim view of the catharsis hypothesis (Bushman, 2002).

To date, which of these models does the research seem to support? In short, neither. Social learning models of aggression, given their popularity in recent decades, have been subjected to frequent (although perhaps not rigorous) testing. Results have been weak, inconsistent, and compromised by poor research methods (Freedman, 2002; Savage, 2004). Meta-analytic studies of media violence effects have consistently demonstrated that links between media violence exposure and increased aggression are close to zero. In the most famous (probably because it is most positive) of these meta-analyses, the effect size for media violence and violent criminal behavior is $r = .1$ (Paik & Comstock, 1994). Results for nonviolent measures of aggression, such as the TCRTT, were slightly higher, with $r = .2$. Most other meta-analyses suggest that even Paik and Comstock’s data may be too high. For instance, Hogben (1998) finds $r = .11$ for the relationship between television viewing and general aggression measures. Bushman and Anderson (2001) find results ranging from $r = .14$ to $r = .2$. Note that these effects are for general measures of aggression, not violent crime, which tends to get even weaker effects. Results for video games have been weaker still (e.g., Sherry, 2001; Ferguson, 2007). Ferguson (2007) found that publication bias (the tendency for scientific journals to publish articles that support a particular hypothesis and not publish those that do not) was a significant problem for video game articles (no similar analysis has been conducted for television) and that unstandardized, poorly constructed measures of aggression tended to produce higher effects than better measures of aggression (perhaps because they allow researchers to pick the results that best support their hypotheses). No support was found for the link between video game playing and higher aggression.

Results have not been kind to the catharsis model either. Although a few early studies initially provided weak support for the catharsis model (e.g., Feshbach, 1961), more recent researchers haven’t given much credence to these early studies. Indeed, in the last few decades, although evidence to

support the social learning theories of media violence has been very weak, evidence supporting the catharsis hypothesis has been virtually absent. Arguably, this may be due to the fact that few researchers actually test the catharsis hypothesis. To do so, a researcher would have to begin by irritating participants, make them angry, and then see whether violent or nonviolent media calm them down. Very few media studies do this. Virtually all media violence studies take the opposite tack; they begin with a (presumably) nonirritated individual and expose him or her to violent or nonviolent media to see whether his or her aggression increases. Thus, arguably, the present body of literature provides little evidence for or against the catharsis model. A few authors have begun to suggest that the catharsis hypothesis should be investigated with more care. For instance, Sherry (2007) has noted that individuals exposed to longer periods of play with violent video games have less aggression than those exposed to shorter periods of play with violent video games. In other words, the longer you play violent video games, the less aggressive you become. While this certainly calls the social learning theories into question, it doesn't truly support the catharsis hypothesis. It is just as likely (more likely, I'd argue) that some people who participate in video game studies are unfamiliar with the games they are randomized to play. This unfamiliarity fosters frustration that diminishes over time once the player becomes accustomed to the game. Studies that include only a short exposure may see increased aggression, but this is due to game familiarity issues rather than violent content (violent video games do tend to be more complex to play than nonviolent games). Similarly, the drop in aggression scores over time is not due to catharsis but rather increasing familiarity. Nonetheless, Sherry (2007) recommends more diligent study of catharsis.

Two recent studies with video games have added a bit of credence to the catharsis model, although not yet enough to engender widespread confidence in it. Unsworth, Develly, and Ward (2007) found that effects of violent video game play varied from player to player, with some players showing cathartic effects after playing violent games. Most players showed no effect, and a small group also became more aggressive. Thus, it may be hard to make conclusive statements regarding whether violent media exerts a cathartic or noncathartic effect, as there is much variation between individuals. In another recent study, Olson, Kutner, and Warner (2008) reported that adolescent boys commonly reported feeling calmer and less angry subsequent to violent video game play and used violent video games to reduce aggression. The authors suggest that the catharsis model should be better examined in future research.

Both the social learning theory and the catharsis model continue to have advocates, although thus far, research evidence for either is weak. Ferguson

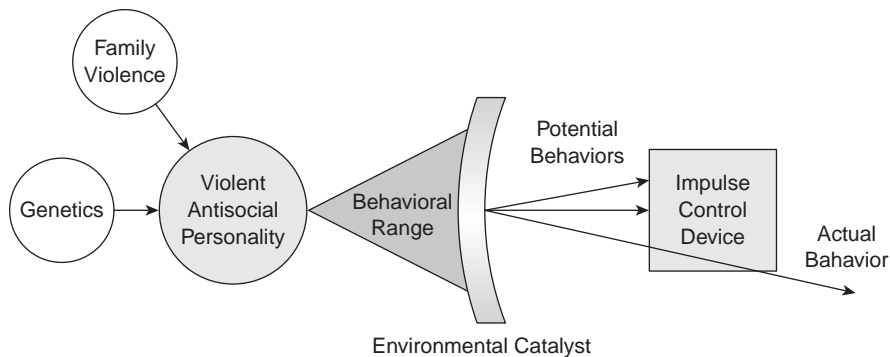


Figure 3.1 A Catalyst Model for Violent Antisocial Behavior

et al. (2008) have proposed an alternative model (the Catalyst Model) of aggression (Figure 3.1) that attempts to explain the interaction between biological and external forces on aggression. At least half of the variance in violent and antisocial behavior can be explained by genetics (Ferguson, in press; Rhee & Waldman, 2002); thus, this Catalyst Model is an evolutionary model. Antisocial personalities develop through the interaction between genetics and physical abuse early in life, an observation consistent with research data (e.g., Caspi et al., 2002). Aggression is a normal response to hostile provocation. Restraining aggression is also adaptive because aggression can carry risks of injury or ostracism from the community; consequently, humans have also evolved an *impulse control device*, which restrains aggressive (and other) impulses. This impulse control device appears to be located in the frontal lobes of the brain; damage to these areas increases aggression (see Chapter 6). Either an aggressive personality or damage to the impulse control devices can result in increased aggressive responses, particularly under periods of increased environmental stress (e.g., loss of job, divorce). These environmental stressors are *catalysts* for violence; they don't *cause* violence, but they may stimulate specific violent acts in a particular individual who is already prone to violent behavior. According to this model, media violence neither is a cause of violent crime nor stimulates it, but may act as a *stylistic catalyst*. This means that individuals who decide to act aggressively may sometimes do so in a way similar to what they have seen in the media (e.g., someone who watches *CSI: Crime Scene Investigation* may decide to use bleach to remove blood evidence from a weapon when she wouldn't have thought of that on her own). Were the media influence removed, the person would still act violently, albeit in a slightly different way (i.e., not using the bleach). Initial testing of the Catharsis Model (Ferguson et al., 2008) demonstrated favorable results in

comparison to the social learning model on a dataset involving violent criminal activity, although further testing of this model will be required.

Examples of Media Violence Research

Reviews of media violence research are fairly plentiful and take different tacks. Anderson et al. (2003) presents the research as essentially flawless, while other reviewers (Freedman, 1984, 2002; Olson, 2004; Savage, 2004) have pointed out systematic problems with media violence research as it exists today. Thus, the quality of the research seems to be something of a matter of perspective. Below I've included a detailed description of three influential and often-cited studies of television and video game effects. These studies are fairly representative of the existing research on media violence effects and are representative of systemic strengths and weaknesses in the field. Understanding these studies should be helpful in understanding media violence data.

One common point raised by critics of media violence research is that other countries with media as violent as (or even more violent than) in the United States have not seen violent crime waves like those that occurred in the United States in the 1970s through the early 1990s. If the introduction of violent television set off massive crime waves in the U.S., why not also in the countries of Europe or Asia where media violence is also commonplace? Huesmann and Eron (1986) examined the effects of media violence on children in multiple countries, including boys and girls in the United States, Australia, Finland, Israel (both a city sample and a kibbutz sample), and Poland. A Dutch group dropped out of the study and published their results separately (Wiegman & Kuttschreuter, 1992), apparently out of concern regarding methodological and interpretive differences. The intent of this study was to examine whether television viewing habits would predict aggressive behaviors in children at a later age (3 years later) while controlling for *trait aggression* (aggressive personality). Out of all six countries (including the Netherlands), significant results based on the original model were found only for American girls. In no other case were significant results reported for television violence exposure and later aggression. Given that the authors did not control error due to multiple comparisons (using something called a Bonferroni correction), it's possible even this one finding could be due to "error." The authors then develop an additional measure in which they compile television viewing habits with a personality measure ("identification with aggressive characters"). This latter personality characteristic is highly correlated with aggressive personality and, as a result, aggressive personality can no longer be teased out from

television viewing habits in this study (Savage, 2004). In other words, it isn't very surprising to find that aggressive individuals behave aggressively. Ultimately, results from differing countries use differing measures of television exposure, many of which include this personality variable. Even with this personality measure combined with television viewing substituting for television viewing habits alone (which was the actual study hypothesis), the end results were mixed, with some groups showing weak effects and others (such as the Dutch and Australians, children on the Israeli kibbutz, and girls in Poland or Finland) still showing no effects. Although Huesmann and Eron (1986) nonetheless interpret the results as supportive of the link between television violence and aggression cross-nationally, the Dutch authors came to the opposite conclusion (Wiegman & Kuttschreuter, 1992). However, if we return to the question of whether viewing television violence was associated with aggressive behavior, the study found evidence for this only in American girls and possibly Israeli city children, not in American boys or in children from Poland, Australia, the Netherlands, or the Israeli kibbutz. One other concern with this study is that it does not attempt to control for the potential effects of exposure to family violence. It is quite possible, given the weak effects found for even the few significant results, that no effects would have been found had family violence been adequately controlled.

One often-cited study is by Friedrich and Stein (1973); it 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 Bonferroni 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.

Within the realm of video games, one of the most often-cited studies is Anderson and Dill (2000), who used the TCRTT. In their laboratory study of violent video game effects, the authors computed four methods of measuring aggression using the TCRTT (noise intensity and duration after both win and loss trials), without applying the appropriate Bonferroni correction to their analyses. Only one of the four measures of

aggression (noise duration after loss trials) was reported as significant, although had a Bonferroni correction been appropriately applied, this index also would have been nonsignificant. Remember also that the noise duration indices appear not to work (Ferguson, Smith, Miller-Stratton, Fritz, & Heinrich, in press) as a valid measure of aggression. Nonetheless, this study is often 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. The authors include a correlational analyses as well, and though they find a relationship between violent video game play and violent acts, they did not control for family violence exposure. In at least one study, controlling for family violence exposure has been found to eliminate all predictive value of violent game exposure (Ferguson et al., 2008). Thus, it would appear to be that family violence exposure predicts violent behaviors, not exposure to violent video games.

The above studies present a fairly representative mix of the kind of studies conducted in the media violence realms. Strengths and weaknesses are fairly systematic across most media violence studies and are, perhaps, part of the reason some scholars question the utility of such studies in attempting to examine the media violence hypothesis (i.e., Freedman, 2002; Savage, 2004).

Violent Crime Data

In the end, perhaps the ultimate question we should be asking is whether media violence research can explain real-world phenomena. Does violent media availability and exposure in a culture relate to levels of violence in that culture? If so, then removing or restricting violent media would appear to be an easy way to reduce societal violence. Disappointingly, the answer is clearly “no.” In the 1970s through the 1990s, a surge in violent crime in the United States led some researchers to conclude that the introduction of television may have been at least partially responsible (Bushman & Anderson, 2001), but this now appears not to have been the case. First, comparing the 1980s to the 1950s (a period of remarkably low crime in the United States) was probably too limited in scope. In fact, violent crime waves in the 1930s and late 1800s and early 1900s were worse than those of the peak years of the 1980s, despite the relative absence of violent mass media (Bureau of Justice Statistics, 1988; National Commission on the Causes and Prevention of Violence, 1969). As such, the surge in violent crimes in the latter half of the 20th century, although unfortunate, was not terribly remarkable in the landscape of American crime trends.

More remarkably, as noted in Chapter 1 (and throughout this book), the United States experienced a dramatic plummet in violent crime rates beginning in the early 1990s, to the point that current rates are about the same as in the late 1960s and early 1970s (Federal Bureau of Investigation, 1951–2004). In other words, despite television, movies, and even music becoming more graphically violent and despite the introduction of violent video games, actual violent crime rates have experienced a massive decline, and the United States is the safest it has been in 40 years. Figure 3.2 presents trends in the overall violent crime rate across time, whereas Figure 3.3 presents trends in the per capita rate of murders and nonnegligent homicides (Bureau of Justice Statistics, 2007). As mentioned earlier, the introduction of television and video games in other countries (particularly in Europe and Asia) was never associated with any wave of violent crimes. From this, we can say that violent media is not precipitating an epidemic of violent crime (or youth violence, as violent crime data is similar for youth and adults) because there is no epidemic of violent crime. It is only a short further step to conclude that, despite the reasonableness of concern regarding media violence and violent crime, media violence is not related to violent crime; the data simply conflict (Olson, 2004).

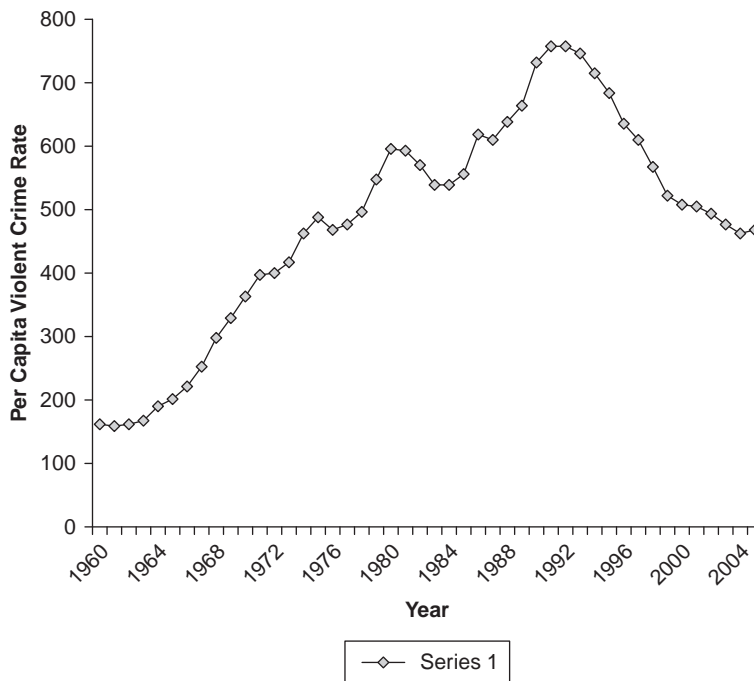


Figure 3.2 Per Capita Violent Crime Rates in the United States by Year

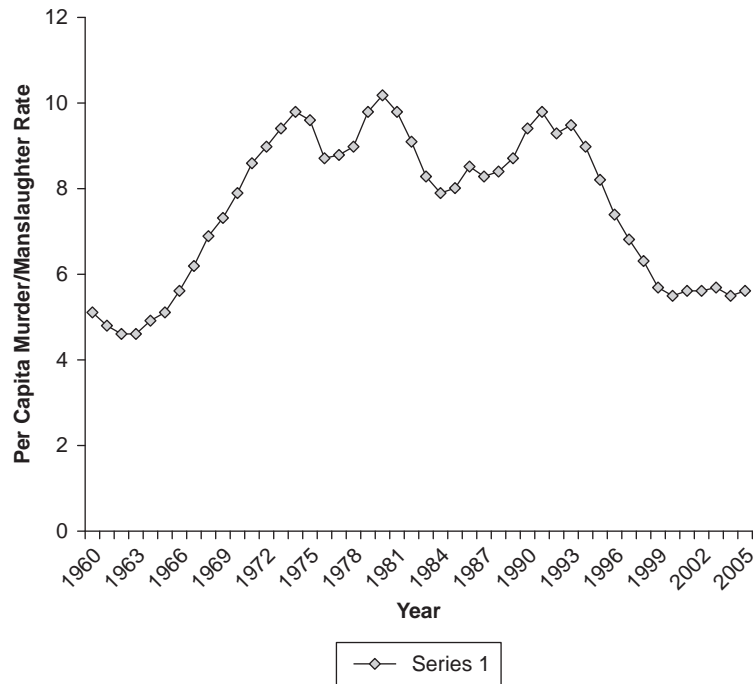


Figure 3.3 Per Capita Murder/Nonnegligent Manslaughter Rates in the United States by Year

If research on media violence is generally flawed with weak results, and violent crime data don't support the conclusion that media violence is an important contributor to violent behavior, you may wonder why media violence is so hotly debated in the United States. Trend (2007) refers to a "media hysteria cycle," of which scientists are a part. Basically, this is how it happens: A rare but well-covered (in terms of the news) violent event occurs. Because the event seems so unexplainable, experts are asked for their opinions. In truth, of course, experts have a great deal of difficulty predicting and explaining serious violent acts, but media violence sounds reasonable and suffices to make the unexplainable seem explainable in the short term. Trend also notes that blaming "media" for all manner of societal vices is nothing new and has probably been practiced through history, often as a means of rationalizing government control over expression. Scientists are not immune from this phenomenon and may stimulate it. There may be some unique cultural and political factors at play in the United States in particular. Media violence is one issue that appeals to many individuals on both the extreme left and right of the political spectrum. Thus, politicians can use "protecting the

children” to appeal to voters from both sides of the spectrum, while simultaneously implying that their opponents are not concerned with child welfare. Despite this “hysteria,” Kutner, Olson, Warner, and Hertzog (2008) found that most parents are not terribly concerned that violent media such as video games will lead to aggression in their own children. Thus the hysteria may be relegated more to special interest groups than the general population. My own impression (admittedly subjective) is that this hysteria is rather uniquely American and that Europeans and other people (including non-American scientists) consider the matter much less dire.

Response of the Criminal Justice System

The media violence hypothesis has received its greatest support from professional organizations such as the American Psychological Association and the American Academy of Pediatrics. Perhaps the most striking is the American Academy of Pediatrics (AAP) testimony before Congress:

Since the 1950s 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. (Cook, 2000)

This statement is a blatant 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, with the impression that thousands of conclusive studies exist. Although no reviews conducted by researchers familiar with the field make such claims (e.g., Paik & Comstock, 1994 included about 200 studies in their meta-analysis), 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 conducted 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.

The standpoint of the criminal justice system has been far more critical. For instance, a recent Secret Service study of school shooting cases found that, despite all the attention paid to video games and other media following these events, violent media consumption was not a useful predictor (United States Secret Service & United States Department of Education, 2002). More interesting, perhaps, has been the response of the judicial system to legislation and criminal tort cases involving media violence. In tort cases, video games, television shows, or musical acts such as Ozzy Osbourne have been accused of causing specific violent acts (such as a youth’s suicide, in the case of Ozzy Osbourne). Legislative bodies at the state and federal level have also moved to censor media with violent content. At the time of this writing, the Federal government is considering options to regulate violent content on television, and there have been at least 10 state legislative efforts to regulate the sale of violent video games. In each case, these efforts (tort cases and censorship) have failed, both on First Amendment grounds and on the weaknesses of the scientific evidence. In many of these rulings, the judges have been critical of the media violence literature and specifically note that the scientific literature does not meet the courts’ standards for conclusive evidence (e.g., Entertainment Software Association, Video Software Dealers Association, and Illinois Retail Merchants Association v. Blagojevich, Madigan, and Devine, 2005). Thus, the case for media violence effects has not passed the inspection of judges, who are perhaps the most neutral observers we could hope to find on this debate.

Conclusion

Much debate remains regarding the impact of media violence on aggressive and violent behavior. At present, the evidence for short-term increases in minor aggression remains inconclusive and a subject of continued debate. However, at present, the weight of evidence does not support a link between media violence and acts of serious aggression or violent crime. Persistent focus on this debate may potentially risk loss of attention to more pressing social causes of crime including poverty, family violence, social inequality, and the drug trade.

Discussion Questions

1. What motivated people to blame video games so quickly following the Virginia Tech Massacre? What does this say about our culture or society?
2. The belief in media violence effects has persisted among many members of the scientific community despite consistently weak evidence of effects. What does this say about the standards of scientific evidence that we use in the social sciences? How might these standards of evidence be improved?
3. One issue that has not been examined yet in media violence research is whether specific groups of individuals or adults may be at risk for aggression in response to media violence. Is it possible that such groups of individuals exist, and what characteristics would they likely have?
4. What factors in our culture contribute to higher violent crime levels compared to other Western cultures? Why have violent crime rates in the United States been declining dramatically over the past 15 years?
5. Does the catharsis hypothesis have any merit? How would you design a study to test the catharsis hypothesis?

Internet Resources

Bureau of Justice Statistics crime data: http://www.ojp.usdoj.gov/bjs/cvict_c.htm

Entertainment Software Association data on video games and gaming: <http://www.theesa.com/facts/index.asp>

Secret Service report on school shootings: http://www.secretservice.gov/ntac/ssi_final_report.pdf

Video Games: The Latest Scapegoat for Violence, by Christopher J. Ferguson (*Chronicle of Higher Education* essay): <http://members.aol.com/dukearagon/VideoGames.html>

Virginia Tech Review Panel Final Report: <http://www.governor.virginia.gov/TempContent/techPanelReport.cfm>

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