

VIOLENT VIDEO GAMES, MASS SHOOTINGS, AND THE SUPREME COURT: LESSONS FOR THE LEGAL COMMUNITY IN THE WAKE OF RECENT FREE SPEECH CASES AND MASS SHOOTINGS

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The issue of video game violence continues to attract attention from the legal and policy communities, particularly in the wake of mass shootings. However, focusing on video game violence has generally not resulted in successful legal or public policy. In part this is because the science upon which beliefs of “harm” in video game violence are based remains inconsistent and heavily disputed. The current article examines several issues. First, the article examines the current evidence about video game violence influences on negative outcomes in players. Second, the article concerns itself with the application of video game science to several recent legal cases, involving both criminal prosecutions and attempted regulation/censorship of video game violence in the United States. Finally, the manuscript addresses several common talking points used in legal cases and by policy makers and examines whether these talking statements survive careful scientific scrutiny. It is advised that, consistent with the legal decisions and government reviews in the United States, Sweden, Australia, and elsewhere, current evidence does not support the regulation of violent video games, and legal or policy attempts to connect video game violence to specific crimes are unlikely to survive careful scrutiny.

Keywords: *video games, Supreme Court, violence*

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INTRODUCTION

Within the academic field of video game violence there has been a pronounced habit among scholars (e.g., Anderson & Dill, 2000; Anderson, 2004) to open a discussion by referring to examples of mass shooters who played video games. Perhaps unwisely, this article follows this tradition by beginning with the Sandy Hook Elementary shooting of December 14, 2012 (CNN, 2012). During this tragic event, 20-year-old Adam Lanza killed twenty young children and six faculty or staff at an elementary school in Connecticut. This unfathomable event evoked a wave of searching for answers for why this incident occurred. Soon after the shooting, society began to focus on whether Lanza had played violent video games. Several elements contributed to this belief. First, some news outlets (e.g., Lupica, 2013), mainly relying on rumor and anonymous sources, implied Lanza was “obsessed” with violent games. The Lupica (2013) news report quoted an anonymous law enforcement official as saying, “Police believe Lanza, immersed in a perverse [*sic*] video game world, killed himself to prevent law enforcement from taking his ‘points.’”¹ Second, soon after the shooting, the National Rifle Association deliberately attempted to shift the national conversation from gun control to video games by highlighting video game violence as a potential cause of the shooting in a press conference (Beekman, 2012). Third, soon after the shooting, some politicians linked the shooting to movie and video game violence, particularly Senator Jay Rockefeller (Terkel, 2012) and Representative Frank Wolf (2013), although other federal and state officials followed suit. And some scholars (e.g., Bushman, 2013) also contributed statements implying links between violent video games and mass shootings.

It is important to note that all of this speculation commenced before official word was released regarding Lanza’s actual video game-playing habits. That information only came nearly a year later with the November 2013 release of the official investigation report (State’s Attorney for the Judicial District of Dansbury, 2013). The final report found that, like most 20-year-olds, Lanza did play both violent and nonviolent games. However, the report declined to link video games to the shootings. Further, the report revealed that Lanza favored games were nonviolent rather than violent, with

1. Most modern commercial video games, in fact, no longer award points as a major aspect of gameplay and have not for some time.

particular interest in the games *Dance, Dance Revolution*, and *Super Mario Brothers*. Despite nearly a year of speculation that Lanza was “obsessed” with violent video games, the official investigation revealed his gaming history was, at worst, unremarkable, and he appears to have favored nonviolent games.

It is probably difficult to overestimate the degree to which the Sandy Hook shooting revived debates on video game violence. In light of the investigation report, numerous comments by politicians such as Senator Rockefeller and Representative Wolf (and the strategy of at least one legal defense to be discussed later) appear to have been ill advised. More crucially, the societal debates about video game violence may have done more damage than good, given the degree to which they distract from more pressing issues such as mental health care, poverty, and educational disparities that may actually contribute to violence.

This article will discuss three main areas with the goal of informing attorneys, legal authorities, and policy makers about the science and politics of video game violence. First, the article will consider the actual data on video game violence, particularly with an eye on societal violence. Second, the article will consider how fears about video game violence have fared in the legal arena. Part of this section will focus on efforts to regulate video game violence through the *Brown v. EMA* (2011) Supreme Court case. Also considered will be attempts to involve video games into criminal cases, particularly the recent Beason murder trial in Illinois. Third, the article will concern itself with several talking points that tend to circulate in public discussions, particularly following mass shootings, and how legal officials and policy makers can be on the watch for these talking points and avoid the traps they offer.

I. WHAT DO WE KNOW ABOUT VIOLENT VIDEO GAMES?

Scientific evidence regarding the influence of violent video games is only one issue that is considered during legal cases, whether regulatory or criminal. However, an examination of the scientific evidence is a common facet of legal cases and public policy. At the time of this writing there have been at least one hundred studies examining video game violence influences on aggression and other related outcomes. However, many of these are remarkably crude and have come under increasing scrutiny and criticism both within and outside of the scholarly community.

Before addressing this issue it is important to make clear that exactly what a violent video game is remains only vaguely defined in both the scholarly literature and public policy. Indeed, although only one issue, this failure to clarify what a violent video game is has been problematic for many regulatory efforts (e.g., *Brown v. EMA*, 2011; *ESA, VSDA and IRMA v. Blagojevich, Madigan and Devine*, 2005). First, the term *violent video game* itself may be purposefully loaded with emotional content so that it distorts objective scientific inquiry and public policy alike. Second, the term *violent video game* implies a uniformity of games within this category. That is to say, all “violent video games” share some form of collective quality that make them similar to one another. However, evidence for the meaningfulness of such a broad, global category of games is lacking. Third, the boundaries of *violent video game* are so ill-defined that almost all video games could be considered violent video games, from child-oriented games such as *Sonic the Hedgehog* through to adult-oriented games such as the *Grand Theft Auto* series. Some scholars have indeed claimed that the majority of video games contain violence (e.g., Thompson & Haninger, 2001), but such content analyses reach these conclusions by defining violence so broadly that it is difficult for video games not to qualify as “violent” if they have any action in them whatsoever. Indeed in a recent murder case (to be discussed in detail later in the article) one scholar acknowledged that, according to many typically used definitions, *Pac-Man* could be considered a violent video game. Most people in the general public, and certainly people familiar with games, would consider this absurd. To some degree this highlights the disconnect between the academic scholarly community and the phenomenology of gaming itself (Oswald, Prorock, & Murphy, 2014).

Similar caveats exist for use of outcome measures, particularly *aggression*. Although aggression is reasonably well-defined, its use remains unclear, particularly with a moralistic bent to imply aggression is always a bad thing in any amount, despite lack of empirical clarity on this matter (Ferguson & Beaver, 2009). This is compounded by significant problems with many commonly employed aggression measures. Many are unstandardized, allowing researchers to essentially pick and choose from among outcomes those that best fit their a priori hypotheses (Elson, Mohseni, Breuer, Scharkow, & Quandt, 2014; Ferguson & Kilburn, 2009), and there are serious problems with construct and external validity (Kutner & Olson, 2008; Tedeschi & Quigley, 1996). Defenders of such measures often point

to intercorrelations between aggression measures as evidence for their utility (e.g., Anderson & Bushman, 1997; Carlson, Marcus-Newhall, & Miller, 1989). However, particularly given their unstandardized use, such intercorrelations are not evidence of validity (or even reliability), and little evidence exists to document the predictive validity of such measures for real-world aggression or violence. Further, recent reanalysis of this earlier work has failed to return such sanguine results (Mitchell, 2012). However, with these caveats about the constructs of video game violence and aggression in mind, the pools of data available are discussed.

A. Video Game Violence and Aggression

A relatively large pool of studies, perhaps around 150 (Anderson et al., 2010), have investigated the impact on violent video games on aggression. These studies include experiments in which participants, usually college students, are randomized to play different types of video games and correlational studies in which participants are asked about their video game-playing habits and then asked about their aggressive thoughts, feelings, or behaviors. Aggression in such studies is typically defined very broadly with outcome measures including filling in the missing letters of words (“kill” being more aggressive than “kiss” for instance), giving consenting opponents in a reaction time game a burst of white noise, or asking another person to eat spicy hot sauce. These types of aggression measures have been historically controversial (Adachi & Willoughby, 2011; Ritter & Eslea, 2005), and their utility in answering questions about real-world aggression or violence is unclear. This pool of research has been criticized on a number of other issues as well, of which a few of the more important are summarized below.

1. Mismatched Videogame Conditions

In experimental analyses, in order to attribute any difference in outcome to video game violence, video game control and experimental conditions must be matched on all other relevant variables. Most experimenters agree this is difficult to do. However, as Adachi and Willoughby (2010) have pointed out, there have been systematic problems with this issue throughout most experimental studies, with a tendency to contrast complex, highly narrative violent games against simplistic nonviolent games such as *Tetris*. Other scholars have indicated that issues such as frustration with complex controls, rather than violent content, may increase aggression in some

experimental studies (e.g., Przybylski, Rigby, & Ryan, 2010). Thus, if any differences are found in such studies, it is difficult to attribute them to violent content as opposed to competitiveness or frustration.

2. Methodological Flexibility Problems

A core facet of science is the use of standardized and inflexible assessment, which limits the influence researchers have over outcomes. Unfortunately, much of the video game violence literature has been allowed to progress in the absence of clear, standardized procedures. This appears to be a broad problem for psychological science (Simmons, Nelson, & Simonsohn, 2011). Many aggression measures allow researchers to extract “aggression” from them however the researcher wants, selecting from multiple possible outcomes those that best fit their hypotheses or personal beliefs. As a consequence, many video game studies may inform us more about what experimenters *wanted* to see rather than what actually happened. For instance, one recent analysis of the commonly used noise blast test (or Taylor Competitive Reaction Time Test) suggests that, depending upon how data is extracted, researchers using a single sample could show that violent video games increase, decrease, or have no effect on aggression (Elson et al., 2014). Not surprisingly, measures that are unstandardized have been found to have much higher effect sizes than more properly standardized measures (Ferguson & Kilburn, 2009).

3. Failure to Control for Third Variables

As the old saying goes, correlation does not equal causation, and thus, in correlational or longitudinal studies it is best practice to try to identify possible confounding variables to see if any correlation remains (Savage, 2004). For instance, boys both play more violent video games and are more aggressive than girls (Kutner & Olson, 2008), so controlling for gender is critical, or any observed correlation may simply be due to a gender effect. Meta-analyses make clear that controlling for other variables reduces effect sizes considerably. For instance, in the Anderson et al. (2010) meta-analysis, simply controlling for Time 1 aggression and gender in longitudinal studies (correlational studies in which participants are tracked over time) reduced correlational coefficients between violent video games and aggression from $r = .203$ (bivariate or uncontrolled correlation) to $r = .075$ (controlled for

only Time 1 aggression and gender).² Thus, bivariate correlations between video game violence and aggressive outcomes are largely spurious.

Further, these aggression studies have been inconsistent in outcomes, with some studies finding evidence for negative effects (e.g., Anderson & Dill, 2000; Hasan, Bègue, Scharnow, & Bushman, 2013; Hollingdale & Greitemeyer, 2013), whereas other studies have found no evidence for harmful effects (Ballard, Visser, & Jocoy, 2012; Elson, Breuer, Van Looy, Kneer, & Quandt, 2013; Ferguson et al., 2008). There have also been several older studies that have proven difficult to replicate in recent efforts (Przybylski, Deci, Rigby, & Ryan, 2014; Tear & Nielson, 2013). Given that this pool of studies is inconsistent, tends to suffer from methodological problems, and is mainly with college students, it is difficult to base a clear conclusion on this data in relation to the types of questions about societal violence of interest to the general public, legal community, and policy makers.

B. Video Game Violence and Violent Behavior

A much smaller pool of research exists examining correlations between video game violence and real-life violence (typically as measured by surveys of behavior) such as assaults, bullying, or dating violence. Although often reliant on self (or parent) reporting, these studies are closer to those of interest to lawmakers and the public. Results for this pool of studies are generally much weaker, that is to say, little evidence has emerged to link violent video games with violence-related outcomes. Willoughby, Adachi, and Good (2012), for instance, found that violent game exposure, with other factors controlled, was associated with less than half a percent increase in assault behaviors. Similarly, Ybarra and colleagues (2008) found that, with other factors controlled, video game violence had no effect on youth violence.³ Other studies have similarly found an absence

2. Something the authors seem inclined not to mention when they discuss their meta-analyses publicly. Several times the authors have publicly discussed this meta-analysis (e.g., Zanberg et al., 2012), reporting only the bivariate effect sizes without reporting the better controlled, and weaker, effect sizes.

3. Although in their discussion and abstract, the authors seemed to try to ignore their own results, presented in their Figure 2, which imply links between video games and violence that did not hold in their own dataset. Thus, it is crucial that policy makers learn to read scientific manuscripts carefully. Often what is claimed does not match the actual results.

of effect on violence (Ferguson, San Miguel, Garza, & Jerabeck, 2012; Gunter & Daly, 2012; von Salisch, Vogelgesang, Kristen, & Oppl, 2011) or even suggest violent game exposure may reduce real-life aggression (Shibuya, Sakamoto, Ihori, & Yukawa, 2008). Overall evidence to suggest video game violence exposure is a meaningful risk factor for youth violence is lacking.

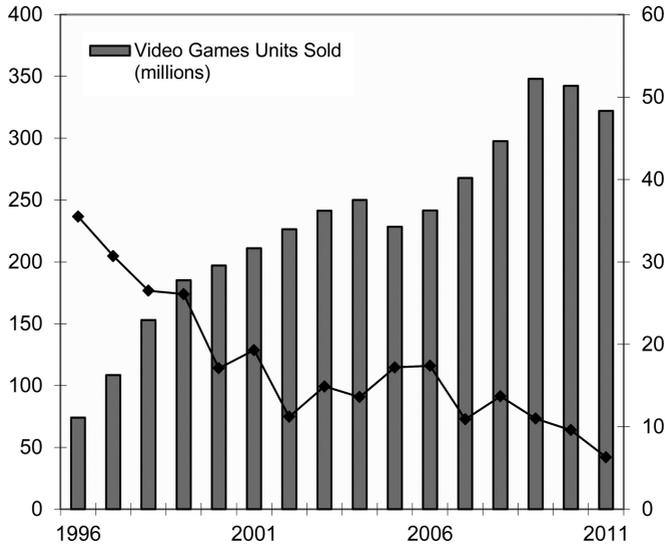
C. Real-Life Violence Data

Many scholars, politicians, and activists have attempted to link video game violence directly to real-life violence. To cite an example among scholars, Craig Anderson, in giving testimony before the U.S. Senate Commerce Committee hearing on “The Impact of Interactive Violence on Children” in the year 2000, shortly after the Columbine Massacre,⁴ said: “[E]ven though one cannot reasonably claim that a particular act of violence or that a lifetime of violence was caused exclusively by the perpetrator’s exposure to violent entertainment media, one can reasonably claim that such exposure was a contributing causal factor. More importantly for this hearing, my research colleagues are correct in claiming that high exposure to media violence is a major contributing cause of the high rate of violence in modern U.S. society” (Anderson, 2000). In the wake of the Sandy Hook shooting of 2012, numerous federal and state lawmakers sought to link violent video games directly to societal violence.

Despite this, actual data from the real world shows the opposite trend. As video game sales have soared, youth violence rates have plummeted by nearly 90% (childstats.gov, 2013). This relationship is presented in Figure 1. Cross-nationally, countries with the highest video game consumption per capita such as South Korea and the Netherlands are amongst the least violent in the world. As such we are not seeing a correlation in real-world data between video game exposure and real-world crime data. Similarly, bullying data among youth have also demonstrated an inverse trend with video game sales (Finkelhor, Turner, Ormrod, & Hamby, 2010). The predicted crisis of youth aggression and violence highlighted by some scholars and politicians has simply not come to pass.

4. The Columbine Massacre of 1999 was perpetrated by two teens who reportedly played a violent video game *Doom*.

Figure 1. Videogame Sales and Youth Violence Trends in the United States



D. Mass Shooters

It is probably difficult to underestimate the degree to which mass shooting events have contributed to debates about video game violence. However, “links” between mass shooters and video games are based on illusory correlation and confirmation bias. Put simply, the social narrative that mass shootings and video games are linked persists by paying attention to cases that fit the narrative such as the Columbine Massacre (Ward, 2001) or Anders Breivik (Sutter, 2012), and ignoring those that do not such as Amy Bishop (CBS News, 2012), William Spengler (Huffington Post, 2012), or Ljubisa Bogdanovic (Ivanka, 2013). Even some cases that are cited as supporting the “link” are based merely on rumor, not fact (e.g., James Holmes; see Sickles, 2013, where only the relatively mild strategy and RPG games *Starcraft* and *Oblivion* were found), or may ignore official investigation reports that downplay video games such as the Sandy Hook example. Virginia Tech is another example of a shooting often referred to as supporting links with video games, despite that the official investigation report found the shooter had played no video games more violent than *Sonic the Hedgehog*, a mild children’s game (Virginia Tech Review Panel, 2007).

Because the majority of young males play violent video games at least occasionally (Kutner & Olson, 2008), it is not hard to link a crime by a young male to an activity so ubiquitous among males in this age category. Then, when a shooter is an older male such as William Spengler or Douglas Harmon, the issue of video games is simply ignored. But what research evidence is available on mass shooters has not provided preliminary evidence for a correlation. A 2002 review by the U.S. Secret Service and the Department of Education (Vossekuil, Fein, Reddy, Borum, & Modzeleski, 2002) found little indication that school shooters consumed high levels of video game or other media violence (in fact, their consumption rates appeared rather low). And most criminological reviews of mass shootings either decline to mention media as a contributing factor (e.g., Lankford, 2013) or specifically exclude media as a contributing factor (Ferguson, Coulson & Barnett, 2011; Fox & DeLateur, 2014). Taken together, these data suggest that attempts to link mass shootings to video games are not based on evidence.

As can be seen, data linking video games to societal violence is, in fact, rather weak in the aggregate. However, this has not prevented some politicians, advocates, and scholars from proclaiming the evidence to be much more conclusive than it actually is. Within the scholarly community, the exaggeration of video game effects had become so pronounced that some scholars warned the field may experience a credibility crisis in the eyes of the courts (Hall, Day, & Hall, 2011a). It is to this consideration of video game science in the legal community that this article now turns.

II. HOW HAS VIDEO GAME SCIENCE BEEN USED OR MISUSED IN LEGAL CASES?

Given the furor or, arguably, moral panic (Ferguson, 2013) over video games during the past few decades, it was inevitable that several legal cases would test the applicability of video game science to the law. Such cases have probably done more damage than good for the credibility of video game science (Hall et al., 2011a), and with very few exceptions, attempts to apply video game science to legal cases have met with failure. Speaking generally, video game science has been applied to multiple First Amendment cases in which states or municipalities sought to regulate the sale of violent video games to minors, or to criminal cases (or civil suits resulting

from criminal acts) in which the behavior of an offender was explained in the context of having played video games. This article will now highlight and discuss a capstone example of each.

A. *Brown v. EMA* (2011)

Controversy over video games is nothing new. As early as 1983, U.S. Surgeon General C. Everett Koop claimed that video games (and here he was speaking of games such as *Pac-Man* and *Asteroids*) were a leading cause of family violence (Cooper & Mackie, 1986). During the 1990s anti-game advocates and politicians alike used highly inflammatory language to equate video games with societal violence. One former military officer and anti-game advocate sought to link games directly to mass shootings, referring to violent games as “murder simulators” (Grossman, 1996), and Senator Joseph Lieberman used terms such as “digital poison” (CNN, 1997). Hearings led by Senators Lieberman and Herbert Kohl pressured the video game industry to create a voluntary ratings system, the Entertainment Software Ratings Board (ESRB), to forestall threatened government regulation. Government regulation of video game violence was again threatened after the Columbine shooting in 1999, and again after Sandy Hook in 2012.⁵ Federal legislation proposed by Senator Hillary Clinton⁶ stalled in Congress, but several state and local governments went forward with regulation-based legislation.

Without exception, state and local efforts to regulate violent video game sales to minors were struck down on constitutional grounds. First Amendment issues were, of course, at the forefront of these cases, with the proposed laws criticized on multiple grounds, including the failure to clearly define what constituted a violent video game (something the scholarly community has also failed to do, as noted earlier). Related to the research on video games, courts could merely have indicated that the research failed

5. Although by Sandy Hook, such threats had largely been rendered moot by *Brown v. EMA*.

6. In introducing her legislation, Clinton repeated a claim that the impact of media violence and youth aggression was similar in magnitude to the effect of lead ingestion on children’s IQ. Such comparisons to medical science have since been debunked as based on flawed statistics (see Block & Crain, 2007, and Ferguson, 2009, for an in-depth discussion), and the Australian Attorney General’s Office (2010) has since referred to such comparisons as misleading.

to reach the level of *strict scrutiny*. That is to say, the courts could merely have said that the research, such as it was, did not rise to the level of arguing for a pressing societal need that would necessitate government intervention in speech-related issues despite otherwise being valid. But in the court cases, judges regularly questioned the validity of the research and whether both legislators and some scholars had, in effect, cherry-picked specific studies and ignored others to make video games look as bad as possible (e.g., *ESA, VSDA and IRMA v. Blagojevich, Madigan and Devine*, 2005; *VSDA and ESA v. Schwarzenegger*, 2009).

The *Brown v. EMA* (2011) case repeated the basic pattern with the exception that the case went all the way to the U.S. Supreme Court. California's law was proposed by California Assemblyman and later State Senator Leland Yee, a child psychologist. The law would have required a warning sticker beyond the existing ESRB rating system and made it an offense to sell violent video games to anyone under 18 with a maximum penalty of \$1000 per infraction. The law left "violent video game" undefined, and in defending the law in the press and in court, its backers tended to refer mainly to a single game, *Postal 2* (e.g., California Attorney General's Office, 2010), which was already difficult to obtain in brick-and-mortar stores and which, in several large samples, no children reported playing (Ferguson, San Miguel, & Hartley, 2009; Kutner & Olson, 2008; Lenhart et al., 2008). The bill was signed into law by then Governor Arnold Schwarzenegger and quickly given an injunction by the district court, which ultimately ruled the law unconstitutional. The State of California appealed the ruling to the 9th Circuit Court of Appeals in *VSDA and ESA v. Schwarzenegger* (2009). The 9th Court upheld the district court ruling and was specifically critical of the research California cited to defend their law.

California then appealed again to the U.S. Supreme Court, which decided to hear the case. Given the unanimity of lower court decisions and the decision by the Supreme Court not to carve out new areas of unprotected speech related to animal "crush" videos (sexual fetish videos in which small animals are crushed under women's heels) in *United States v. Stevens* (2010), the decision by the Supreme Court to hear the case was unexpected. This led some speculators to wonder if the Supreme Court may have been signaling a willingness to carve out new areas of speech, at least related to video games, which may be subject to government regulation (Denniston, 2010).

Ultimately this was not to be the case. In a 7-2 decision (or what some have called a 5-2-2 decision, given 2 concurring justices were at least somewhat concerned about video game effects) the U.S. Supreme Court upheld the lower court rulings. Naturally, much of the decision focused on First Amendment issues, such as the vagueness of the concept of video game violence, and the focus on video games rather than other media.⁷ However, in speaking of the science, the justices could have chosen to say simply that strict scrutiny had not been reached. Instead the majority opinion was specifically critical of the research:

The State's evidence is not compelling. California relies primarily on the research of Dr. Anderson and a few other research psychologists whose studies purport to show a connection between exposure to violent video games and harmful effects on children. These studies have been rejected by every court to consider them, and with good reason: They do not prove that violent video games *cause* minors to *act* aggressively (which would at least be a beginning). Instead, "[n]early all of the research is based on correlation, not evidence of causation, and most of the studies suffer from significant, admitted flaws in methodology." *Video Software Dealers Assn.* 556 F. 3d, at 964. They show at best some correlation between exposure to violent entertainment and minuscule real-world effects, such as children's feeling more aggressive or making louder noises in the few minutes after playing a violent game than after playing a nonviolent game.

Even taking for granted Dr. Anderson's conclusions that violent video games produce some effect on children's feelings of aggression, those effects are both small and indistinguishable from effects produced by other media. In his testimony in a similar lawsuit, Dr. Anderson admitted that the "effect sizes" of children's exposure to violent video games are "about the same" as that produced by their exposure to violence on television. App. 1263. And he admits that the *same* effects have been found when children watch cartoons starring Bugs Bunny or the Road Runner, *id.*, at 1304, or when they play video games like Sonic the Hedgehog that are rated "E" (appropriate for all

7. One facet of most media violence theories is that mere exposure to any kind of violence, whether in a Bugs Bunny cartoon, *Grand Theft Auto*, or even simply seeing a picture of a gun, should have essentially the same negative influence. Media effects theories even explicitly fail to distinguish between fantasy violence and real-life violence exposure in terms of how these should influence children (e.g., Bushman & Huesmann, 2014). Such a perspective was clearly unconvincing to the justices, who appeared concerned that *Brown v. EMA* would open the door to censorship of broad classes of media.

ages), *id.*, at 1270, or even when they “vie[w] a picture of a gun *id.*, at 1315–1316. (*Brown v. EMA*, 2011, pp. 12–13)

Thus, the Supreme Court expressed skepticism of the objectivity and breadth of the state’s review of the evidence and the quality of the evidence cited.

Echoing concerns among some researchers regarding the poor quality of aggression measures used in many studies (Ritter & Eslea, 2005; Savage, 2004; Tedeschi & Quigley, 1996), the Supreme Court noted the disconnect between *aggression* as used in many of the studies and how it is perceived in the general public:

One study, for example, found that children who had just finished playing violent video games were more likely to fill in the blank letter in “explo_e” with a “d” (so that it reads “explode”) than with an “r” (“explore”). App. 496, 506 (internal quotation marks omitted). The prevention of this phenomenon, which might have been anticipated with common sense, is not a compelling state interest. (*Brown v. EMA*, 2011, p. 13, n.7)

In this case, the justices echo common concerns about the validity of aggression measures typically employed in the field and whether they are valid indicators of problematic behavior in the real world.

Justices Alito and Roberts concurred with the majority opinion, but expressed concerns about violence in video games and were willing to consider less restrictive means to control such content. Justice Breyer also appeared convinced by causal arguments but expressed frustration regarding what to do with conflicting social science data. Justice Breyer appeared convinced by California’s argument that interactivity of video games makes them different from other media, stating “the closer a child’s behavior comes, not to *watching*, but to *acting out* horrific violence, the greater the potential psychological harm” despite the lack of consensus on this even among advocates of the causal position. The majority opinion were dismissive of Justice Breyer’s efforts, stating “we do not see how it could lead to Justice Breyer’s conclusion, since he admits he cannot say whether the studies on his side are right or wrong” (*Brown v. EMA*, 2011, p. 13, n.8).

The impact of *Brown v. EMA* (2011) can probably be witnessed in the aftermath of the Sandy Hook shooting. Despite that nothing official was known at the time about Lanza’s video game habits (and which turned out to be unremarkable), several state legislatures introduced legislation to tax video games, regulate them, or “study” their effects (usually with the

expressly stated implication that such “studies” would be used to support later legislation). Most of these efforts floundered. The only effort to pass known to this author was a bill in New Jersey ordering the State Department of Education to provide a pamphlet “warning” parents of the dangers of violent media. The language of the bill was biased and problematic, and any pamphlet produced using such language would be more misleading than helpful, but nonetheless it does not violate the First Amendment.

In the *Brown v. EMA* case, California, like other states before it, arguably made several major errors:

1. The state “cherry-picked” data to support its position, and ignored data suggesting video game violence has limited impact.
2. Of the studies the state did use, the state failed to understand them, their limits, and the controversies surrounding them. Put simply, the state trusted too much in a select group of studies.
3. The state also appeared to put too much trust in policy statements by professional advocacy organizations such as the American Psychological Association and American Academy of Pediatrics, as well as anti-media advocacy groups such as Common Sense Media, failing to understand the social, political, and financial incentives for such groups to produce overly conclusive statements that did not reflect the science.
4. The state failed to make clear what forms of media would and would not be covered under the law.
5. The state failed to make the case that the proposed bill would improve upon the existing ESRB rating system, which even government reviews (e.g., FTC, 2009) have found to be reliable.

It is worth noting that the *Brown v. EMA* case also exposed the considerable acrimony that exists among scholars in the field. Two amicus briefs were filed by scholars in the case, one group supporting California (Gruel Brief, 2010), the other opposing California (Millett Brief, 2010) and pointing to research not cited by California. Later, two signers of the Gruel Brief along with a lawyer wrote a law review piece alleging that they and their close colleagues had more experience than the signers of the Millett Brief (Pollard-Sacks, Bushman, & Anderson, 2011). Soon after, several scholars who were not involved in either amicus brief declared that the Pollard-Sacks analysis was not credible, consisting of significant methodological flaws and obvious self-interest (Hall, Day, & Hall, 2011b). Nor did the

Pollard-Sacks review address the substance of the two briefs, consisting mainly of ad hominem and argument-to-authority logical fallacies.

In the months following the *Brown v. EMA* decision, two of the scholars who signed the Gruel Brief (Gentile & Anderson, 2011) implied they never supported the California law. Although they attached their names to an appendix to the Gruel Brief regarding the scientific literature, they stated, “We are not surprised that the law was struck down, as we believe that our society should be very careful about limiting First Amendment protections.” Whether it is wise for scholars to attach their names to an amicus brief supporting a law that they themselves do not support, is for readers to decide.

B. The Beason Murder Trial

Because of the controversies surrounding video games and because older adults in particular are both less experienced with them and more fearful of their harmful effects (Przybylski, 2014), video games make tempting targets in criminal trials and civil lawsuits. In both cases video games may be offered as a causal explanation for a criminal’s behavior, typically with the implication that the offender (usually male) was playing out scenes witnessed in a game. The anonymous comments by a police officer in the Lanza case mentioned at the beginning of this article are typical of this line of thinking, in this case, the notion that Lanza was concerned about “points” just like in a video game (despite that few commercial video games still focused on “points” by that time).

Several efforts to hold video games accountable for homicides in the early 2000s were promoted by an attorney and anti-game activist, Jack Thompson, who was later disbarred due to erratic behavior connected with several video game cases (Ostrovsky, 2007). In 2003, Thompson sought to represent Dustin Lynch, a 16-year-old boy accused of murdering a teen girl. Thompson argued that Lynch was influenced by the shooter game *Grand Theft Auto III*. Lynch declined Thompson’s offered legal counsel, and Lynch’s mother later stated, “It has nothing to do with video games . . . , and my son’s no murderer” (Hudak, 2003).

Thompson was also involved in the 2005 case of Devin Moore, an 18-year-old youth who shot three police officers and fled in a police car after being arrested in Alabama. Moore was convicted of the murders, and the families of two victims later sued the video game industry for responsibility

in the shootings. Said Thompson of the shootings, “What we’re saying is that Devin Moore was, in effect, trained to do what he did. He was given a murder simulator. . . . He bought it as a minor. He played it hundreds of hours, which is primarily a cop-killing game. It is our theory, which we think we can prove to a jury in Alabama, that, but for the video-game training, he would not have done what he did. . . . The video game industry gave him a cranial menu that popped up in the blink of an eye, in that police station. And that menu offered him the split-second decision to kill the officers, shoot them in the head, flee in a police car, just as the game itself trained them to do” (Leung, 2005). During the murder trial the judge barred Moore’s defense from introducing evidence suggesting the video game led to the shooting. The civil case appears to have languished. As of this writing the author could find no references to its final disposition (the last news coverage dated to approximately 2007–2008).

Following the failure of the video game defense in Lynch and Moore cases, efforts to use video game social science in criminal cases in the United States appeared to wane. This changed in 2013 after Sandy Hook, when a video game–related defense was tried in a case commonly known as the Beason Murders.

In September 2009, five members of the Gee family, parents and three children, were murdered in their home in Beason, Illinois, bludgeoned to death. Soon after, Christopher and Jeffrey Harris were arrested in connection with the deaths. Jeffrey plead guilty and received a 20-year sentence in exchange for testifying that he had helped his brother cover up the murder of the family. Christopher Harris was not put on trial until April 2013. After the Sandy Hook shooting his defense strategy became public. Harris alleged that, in fact, the Gee family’s 14-year-old son, Dillen Constant, had killed the other family members, and Harris had walked in on the murders and had to defend himself against Constant. Autopsy reports indicated that Constant had been bludgeoned over fifty times with a tire iron.

During the trial, the defense called on a social psychologist as a witness to support their theory of the crime.⁸ The social psychologist reviewed Constant’s educational and medical records, as well as reports that a Playstation 2 and several violent video games had been found in Constant’s room. Medical records indicated that Constant had been diagnosed with

8. The author of this article consulted with the prosecution but did not testify.

ADHD, and his educational records indicated that he had numerous behavioral issues at school. However, the records also suggested that Constant's behavior at school in the month leading up to his death had much improved. Although the social psychologist was barred from indicating whether he thought Constant could have committed the specific crime of murdering his family, he indicated that the combination of Constant's history of ADHD and behavioral problems coupled with his exposure to violent video games placed him at risk for aggressive and violent behaviors.

Exactly how much exposure Constant had to violent video games was unclear, however. A forensic analysis of the Playstation 2 suggested that Constant had only saved games infrequently in the past months, suggesting relatively infrequent game play. And most boys of his age have violent video games in their possession (Kutner & Olson, 2008; Lenhart et al., 2008), so it is unclear that finding several in Constant's room was particularly remarkable.

Under cross-examination, the social psychologist's credibility was called into question (Rushton, 2013). He acknowledged that he was not a licensed psychologist, and had not conducted a proper risk assessment related to Dillen Constant. He had not interviewed surviving family members or other witnesses, despite that he could have done so. Related to video games, he had to acknowledge it was not possible to know how much exposure to violent video games Constant actually had. He also acknowledged that evidence linking violent games to extreme violence was lacking. Further, when asked if even games such as *Pac-Man* might be considered "violent," he acknowledge that under many definitions of violent video games, *Pac-Man* could indeed be considered a violent video game. Harris was convicted in May 2013 of all five murders.

At its core the Beason murder case presents the risks of selective social science intruding in the courtroom. The misapplication and misrepresentation of video game science in the court arguably might very well have resulted in the release of a multiple murderer and damage to the reputation of an innocent victim unable to defend himself in court. The eagerness to link video games to real-world violence despite considerable evidence to the contrary has done serious damage to the credibility of our field in the eyes of the courts and general public. In the following and final section, several common talking points used in public policy and legal discussions of criminal cases are considered to examine how they fare in light of the research evidence.

III. IF YOU SAY IT OFTEN ENOUGH, IT BECOMES TRUE

Because of the emotionality and “culture war” aspects of issues surrounding video game violence, as well as the First Amendment issues, advocates for regulation and/or censorship of video games are incentivized to put the issue in the starkest possible terms. This has been true within the scholarly community just as it has been in the general public. Hillary Clinton’s comparison of the effects of video game violence to those of lead ingestion on children’s IQ when introducing her bill to regulate violent video games is just one such example. Undoubtedly, individuals who raise these talking points believe them to be true, at least when first created. However, one hallmark of these talking points is that, even as they become increasingly criticized and controversial, people continue to repeat them as absolutely true despite their controversy. The intent of such a strategy appears to have been to capitalize on the interfering effects of repetition on the differentiation of truth from falsehood (Tussing & Greene, 1999). Put simply, something repeated often enough may begin to sound true.

With that in mind, here are several commonly repeated talking points that are either controversial or discredited, yet continue to circulate in discussions of video game violence. To be clear it is not the case that a careful, honest argument could not be constructed for some kind of video game effects, even if other scholars might disagree with such an argument. However, the following statements are used more often as politicized talking points rather than carefully constructed scientific arguments.

Talking Point One: There is consistent research evidence on video game violence

This line of reasoning began to emerge in the scholarly community in the years following the Columbine Massacre of 1999, despite that the research had been and remained inconsistent (see Ferguson, 2013, for timeline). Prior to 1999, most scholars had been upfront about the inconsistent returns from video game violence research. However, overstatements of consistency in the television violence literature had been a long-standing issue (Freedman, 1984) and appeared to have migrated into the video game violence literature with some of the same scholars. These same claims of consistency were at the core of legislative attempts to regulate violent games such as that by California in *Brown v. EMA*.

By 2005, such consistency claims were once again under challenge (e.g., Williams & Skoric, 2005) and became increasingly so in the years since. This has not dissuaded some scholars from continuing to claim consistency by employing *citation bias*, or the citation only of studies supporting the authors' personal beliefs. In one recent example, following Sandy Hook, long-time media critic and politician Representative Frank Wolf commissioned a report by the National Science Foundation to examine the issue of media and other influences on youth violence and mass shootings. Wolf, it is worth noting, chairs the congressional committee that oversees appropriations for the NSF. The resultant report (Subcommittee on Youth Violence, 2013) attempted to draw direct links between media violence and societal violence including mass shootings, but did so through blatant citation bias, that is, simply not citing any of the considerable number of studies failing to support such links. The only exception in this report was citation of work by Joanne Savage (Savage & Yancey, 2008), which was mis-cited as supporting links between media violence and violent crime despite that her work made no such connection. Ironically, at approximately the same time this report was released, the anti-media watchdog group Common Sense Media (2013) released its own report on media violence, which much more honestly acknowledged inconsistencies and limitations in the research literature. It is reasonable to observe that it is an indication of serious problems within the field when an advocacy group whose funding depends on frightening people about media violence can release a report that is more honest and carefully construed than the National Science Foundation.

Belief in consistent evidence has also not fared well under objective scrutiny by neutral entities. Reviews that conflict with such claims now include not only the U.S. Supreme Court but the Australian Attorney General's Office (2010), the Swedish Media Counsel (2011), the U.S. House of Representatives (Gun Violence Prevention Task Force, 2013), and the State of Pennsylvania (Advisory Committee on Violence Prevention, 2013). Thus—unlike professional advocacy reviews comprised of scholars invested in ideological beliefs and organizations arguably promoting their self-interest rather than objective science—neutral reviews have generally debunked the claim of consistent evidence.

It may also be difficult to determine whether some individual studies support or do not support media effects theories. Anderson and Dill (2000) is one classic example. In their analysis of aggressive behavior, the authors

run four separate statistical tests and find significance for only one of the four. Had a proper Bonferroni correction been applied, even this fourth would have been nonsignificant. Ambiguous research findings such as this may often be interpreted in the direction of the individual scholar's a priori opinions. It is certainly well within the realm of honest scholarly debate to question the meaningfulness of individual studies from the perspective of either side of that debate. However, scholars would do well to be more cautious in their public statements. Overstatements of consistency (which can be easily fact-checked as false) risk damaging the credibility of the field (Hall et al., 2011a).

Talking Point Two: Mentally unstable children are more influenced by violent video games

This is actually not an unreasonable hypothesis. It is mainly problematic in that it has proceeded as if true despite a lack of evidence. In this sense the argument is often presented as a classical case of *begging the question* in which the premise of the question is assumed, a priori, to be true.

Little data exists on this question mainly because of difficulties in gaining access to large samples of children with preexisting mental health problems. Some work with college students (e.g., Markey & Markey, 2010) has suggested that college students with preexisting angry personalities may be prone to becoming angrier after playing violent games, although the behaviors in question are very mild and should not be generalized to societal violence. However, a recent correlational study of children with elevated symptoms of depression and attention deficit found no links between violent game exposure and bullying or delinquency (Ferguson & Olson, 2014). Thus, although it may be reasonable to hypothesize about this issue, evidence for it remains lacking at this juncture.

Talking Point Three: The interactive nature of violent video games makes them more harmful

The notion that the interactive nature of video games might make them more harmful than television or books was also a reasonable hypothesis (the same argument had been raised for the role-playing game *Dungeons & Dragons*), albeit one that never gathered much evidence. Even by the early 2000s, data from meta-analyses (Sherry, 2001) indicated that this belief was

false. Nonetheless, this argument was raised repeatedly during the *Brown v. EMA* case and is frequently mentioned by politicians and scholars.

One study that is sometimes mentioned in relation to this belief is Polman, de Castro, & van Aken (2008). In this study small groups of children were randomized to either play or watch someone play a violent video game, or play a nonviolent video game. Children were then given a mild aggression task. Children who played violent video games were more aggressive than children who watched violent video games. As such, proponents of the interactivity hypothesis often cite this study as evidence violent video games are more harmful than television or movies.

However, there are several problems with this use of this study. First, there were no television or movie conditions in the study, thus there are no grounds for making comparisons between video games and television or movies based on this study. Although watching video games was probably used as a control condition for equivalence reasons, it is a fundamental misuse of the medium and thus fails as a meaningful control. Put simply, few people actually *watch* video games for entertainment. Unlike television, video games are not meant to be watched. Thus the boring experience of watching a video game cannot be compared to the experience of watching a narrative television show or movie. Secondly, the experiment found no differences between violent and nonviolent video games. Thus none of the differences found in the study can be attributed to violent content at all.

Few other studies have directly compared video games to other entertainment medium. One prospective study to do so (Ferguson, 2011) found no evidence for prospective links between either video game or television violence with later youth violence. Similarly, in a correlational study, Ybarra and colleagues (2008) found that, with other factors controlled, neither television nor video game violence contributed to youth violence. It is, thus, probably time to retire this belief.

Talking Point Four: There is a scientific consensus on video game violence

The notion of whether a consensus on violent video game (or other violent media) effects exists has, largely, tended to depend on the degree to which advocates of media effects theory have held power within the scholarly field and in professional organizations such as the American Academy of Pediatrics of American Psychological Association. It is probably the case that,

particularly between the 1970s through the 1990s, the majority of scholars who did research on media effects believed in media effects, at least to some degree, but whether this reflects a meaningful consensus or simply a selection effect is less clear. Similarly, the degree to which such a consensus was based on actual data as opposed to conforming to social and political narratives, as well as the short-term best interest of the profession in regards to issues such as political influence and grant funding, also remains unclear. It is difficult to get grant funding for one's research by arguing that the issue at hand is not a pressing social problem.

Data to support beliefs in a consensus are generally lacking, however. The closest to such data probably comes from Murray (1984) who, in a nonrandom poll of scholars who had written articles on television and aggression, found that 82% agreed with a statement by the National Institute of Mental Health conclusively linking television violence to aggression in children. By Murray's acknowledgement, the sample is nonrandom and the potential for sampling bias exists, but this is probably evidence that the majority of scholars who did research on television violence and aggression believed in the potential for effects in 1984. Further, it may very well be the case that scholars who already worried about television violence were prone to becoming active in the field, rather than having assessed the data objectively. Within those limits, however, it was probably true to suggest, at least, that the majority of researchers who did research in the area agreed on the topic of media effects.

By 2013, that situation had changed, and in effect, there was no longer a consensus on whether there even was a consensus. Advocates for the media effects position continued to argue that a consensus existed. Bushman and Cruz (2013), for example, in a sample of media scholars, found that 66% agreed or strongly agreed that video games could increase aggression. Similarly, 90% of pediatricians who were members of the Council on Communication and Media at the American Academy of Pediatrics agreed with the same statement. However, the survey was not actually open to all media scholars, but only to certain select groups that tended to be heavy with supporters of media effects theory such as the aforementioned Council.

Perhaps more critically, the Bushman and Cruz (2013) study immediately experienced a failed replication in the form of van Looy and colleagues (2013) who sampled 544 media researchers. They found that only 10.1% agreed or strongly agreed that the effects of video games on aggression were

a problem for society. This would seem to suggest that the consensus is *against* media effects theory. Further, in September of 2013, a group of approximately 230 media scholars wrote an open letter to the American Psychological Association, asking them to retire their policy statements on media violence and desist in releasing any further policy statements (Consortium of Scholars, 2013). Overall, it is probably difficult at present to support the belief that a scholarly consensus exists on this issue.

Beliefs in a consensus may have been promoted by policy statements by the American Academy of Pediatrics (2000; 2009) and American Psychological Association (2005) linking video game and other media violence to aggression. However, these policy statements have been found to contain multiple and serious errors (Freedman, 2002; Ferguson, 2013). For instance the AAP's 2000 statement repeated the urban legend that 3500 studies of media violence existed with only 18 not finding evidence for harmful effects. The AAP's source for those figures was a book of pop psychology, not a credible scientific source (Grossman & DeGaetano, 1999). By its 2009 policy statement, the AAP had quietly downgraded the number of studies to 2000, a net loss of 1500 studies over 9 years, but even this number appeared based on a single database of studies. A perusal of this database reveals that many of the studies were unrelated to media violence, so this number still appears to be off. In his analysis of the media violence field, Freedman (2002) found approximately 200 to 300 studies, including unpublished studies, a number consistent with that reported by most meta-analyses. Thus, the AAP had something as simple as the number of studies wrong by a factor of over ten.

Other basic errors and repetitions of scientific urban legends such as comparisons with medical effects like smoking and lung cancer and repetitions of the interactivity hypothesis despite it having little evidence permeate policy statements by both the AAP and APA. Both policy statements employ blatant citation bias, failing to cite numerous studies disconfirming the organizations' positions. In one related report from 2011, the American Academy of Pediatrics claimed that youth might suffer from a "Facebook Depression" due to too much social media use. Scholar Larry Magid (2011) discovered that not only had the AAP failed to cite studies not supporting its position but at least one of the scholars they did cite specifically disavowed the AAP's conclusions on her website (Davila, 2011). Thus, both organizations have a very poor track record when it comes to the issue of media policy statements.

One issue is that both groups, when selecting committee members for their media policy statement task forces in the past, have chosen only from among individuals heavily invested in anti-media views and have ignored skeptics (Ferguson, 2013). These scholars reviewed their own work and declared it beyond further debate. Thus, these policy statements have not constituted objective or balanced reviews. In a forthcoming review of their policy statements the APA appears to be repeating this mistake, including four of seven task force members with clearly identifiable anti-media views. Two task force members signed the Gruel Brief in *Brown v. EMA*, two others have worked closely with anti-media researchers and have made anti-media statements in the popular press, and one was a coauthor on the National Science Foundation report mentioned earlier in this article.

This is unfortunate considering that such policy statements are often thought to reflect objective reviews of a research field. However, it is often forgotten that groups such as the APA and AAP are professional *advocacy* organizations for the welfare of their professions. This is a necessary undertaking, of course, but does mean that these groups have certain social and political biases that influence their policy statements. Put simply, arguing that there is a pressing social problem that their members can help fix is in the short-term interest, politically and financially, of these organizations. Identifying media violence as a problem increases these organizations' political influence as well as sources of grant funding for their members. Given the highly problematic nature of these policy statements from a scientific standpoint, it is probably better to view them as statements that reflect the groups' own vested interests rather than objective truths. Further, once these groups have gone out on a limb, making bold and conclusive statements linking media violence to aggression, it becomes more difficult for them to retreat from these policy statements without losing respectability (Hall et al., 2011a). Speaking of news journalism, James Poniewozik (2013) commented, "Once someone stakes out a tough or controversial position, there's a buy-in: from that point on, they're conditioned to look for reasons to support a position they're already invested in." This would appear to be as true for professional advocacy organizations as it is for journalists. And it is particularly true given that these organizations also are responsible for publishing science, a potential conflict of interest if they are heavily invested in particular policy positions. The concern is that such organizations will selectively publish research supporting their policy positions and squash research conflicting with them.

Talking Point Five: All mass shooters are video game players

This belief is based on simply ignoring cases that do not fit the stereotype. Given that about 90% of young men play violent video games, finding that a young crime perpetrator played violent video games is hardly surprising. But this neglects that many older perpetrators of gun violence, such as (recently) Douglas Harmon, William Spengler, or Jimmy Lee Dykes, were not gamers. Neither, in fact, was the Virginia Tech Shooter, often erroneously linked to video games (Virginia Tech Review Panel, 2007). And as mentioned, Adam Lanza's video game playing history appears to have been unremarkable aside from his favoring nonviolent games. In fact, a recent review of mass murders specifically called links between video game violence and mass shootings a "myth" (Fox & DeLateur, 2014).

Talking Point Six: Video games award points for antisocial acts

This belief is a leftover from the *Atari* age when video games visibly racked up scores for eating ghosts or shooting space ships. Now, this type of statement usually reflects ignorance on the part of the speaker. Most modern commercial games stopped awarding points in the traditional sense, a decade or more ago, and focus instead on complex narratives. Commentators are advised to avoid such comments unless they wish to signal their unfamiliarity with commercial video games.

Talking Point Seven: The military used violent video games to desensitize soldiers to kill

This talking point is little more than an urban legend, never having had actual data to support it. The origin appears to be the books of David Grossman (1996; 1999) who argued that the military used human-shaped targets and video games to desensitize soldiers from their natural aversion to killing. However, these statements are little more than speculation. During an APA Division 48 task force investigation into another matter (weaponized drones), a military psychologist specifically disavowed the use of video games in this manner. The military does use video game-like simulators to train vehicle handling and team performance, but there is little basis for the claim that video games are used to desensitize, or that they would even be effective for this purpose.

CONCLUSION

Video game science has experienced a long and largely unfruitful involvement in the law, both from a public policy and criminological standpoint. Much of this involvement has relied on fundamental misunderstandings and misrepresentations of video game science. By this juncture, the intersection of video game science and the law has done great damage to video game science by highly politicizing it. Video game science would likely benefit from time out of the limelight with fewer low-hanging political fruit to grasp for.

Similarly, legal and public policy fascination with video games has largely been unproductive. Undoubtedly, politicians and lawyers have practical short-term motivations for singling out video game violence, although in the long term they also risk damage to their credibility. One need only look to the history of past moral panics over media and the reputations of individuals such as Fredrick Wertham (1954/2004) or Tipper Gore (Reesman, 2010), who have promoted moral crusades over media only to see such media subsequently become mainstream. In general, there is always some risk in basing the law or public policy on the shifting sands of social science, but perhaps more so in this field than others. There are many scholars on both sides of the video game debate doing excellent work, but too often the intrusion of culture war and moral panic in this field has done great damage to its credibility. This is particularly tragic given that focus on video games likely has distracted society from more serious (but perhaps less “sexy”) issues that actually influence societal violence such as poverty, mental health, and educational disparities.

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