

# Blazing Angels or Resident Evil? Can Violent Video Games Be a Force for Good?

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Violent video games have been a source of controversy in the United States and elsewhere for several decades. Considerable concern has been raised in the public and scientific communities about the alleged deleterious effects of violent games. These concerns may coincide with periodic moral panics about media's influence, particularly on youth. This paper argues that the negative effects of violent games have been exaggerated by some elements of the scientific community, fitting with past cycles of media-focused moral panics. By contrast, potential positive effects of violent video game play have been ignored in the debate on violent games. The current paper considers research in several areas, including aggression, but also the nascent research fields of visuospatial cognition, social networking, and use as educational tools. It is argued that the debate on video game violence should be broadened to include both potential negative and positive effects.

*Keywords:* mass media, computer games, aggression, violence, visuospatial cognition, health psychology, social skills

The era of the modern video game began in the 1970s with the advent of arcade features such as *Space Invaders* and *Asteroids* and the quick launch of the Atari 2600 home game console. Debates immediately emerged about the moral and social implications of video games featuring violent content, as well as their potential positive use in education and other settings. Arguably, from *Death Race* to *Grand Theft Auto*, the greater part of the debate has focused on the negative effects of violent content. Such a debate is understandable. Social science has come to understand aggression as primarily socially learned (Bandura, 1965; Bandura, Ross, & Ross, 1961). The American Psychology Association's pamphlet on youth violence asserts that violence is learned rather than genetic, although violence may interact with other variables with strong heritability, such as learning disorders, impulsivity, and low IQ (American Psychological Association [APA], 1996). Although evidence from molecular and behavioral genetics (Caspi et al., 2002; Ferguson, 2010; Rhee & Waldman, 2002) suggest that significant genetic factors may either directly influence or interact with environmental factors to cause aggression, the "learning only" view remains the dominant paradigm for understanding aggression (Buss & Shackelford, 1997). Fears about video game violence also fit into a sociological and historical context of fears of new media, particularly in United States culture, but also in a broader historical world context (Cumberbatch, 2008; Grimes, Anderson, & Bergen, 2008; Gauntlett, 1995; Kutner & Olson, 2008; Trend, 2007). However there is a risk that such concerns could move beyond objective scientific examination and into the

realm of ideology, dogma and moral panic (Gauntlett, 1995; Grimes et al., 2008). An overemphasis on the potential deleterious effects of violent games, whether real or imagined, also preempts discussion of the strategic use of violent games as a positive force in cognitive development, education, psychological treatment, and health care. In this article, I attempt to bridge the gaps in the current discussion of violent video game effects, and open a wider discussion of the potential benefits and risks of video game playing among youth.

Before discussing the issue of video game violence, it is important to operationalize relevant terms such as aggression and violence. In this article, aggressive behavior is defined as intentional behavior produced to cause physical harm or humiliation to another person who wishes to avoid the harm (Baron & Richardson, 1994). Aggression can thus be distinguished from aggressive play in which two or more consenting individuals are wrestling, playing war, and so forth, but mutually enjoy the activity. Not all aggressive behaviors are violent, nor even necessarily maladaptive or socially discouraged. In this article, violence is defined as intentional behavior produced to cause considerable physical harm to another person who contrastingly wishes to avoid the harm (Ferguson, 2010). Violent behaviors inherently are aggressive, yet many aggressive behaviors that do not cause physical harm are not violent. Video game violence itself generally has been characterized by researchers as intentional acts within game play directed to cause physical harm to an animated character within the game (e.g., Pober, Thompson, Haninger, & Yokota, 2008).

## The Sociological and Historical Context of Violent Video Game Fears

Most psychologists are likely aware that debates and concerns about media—particularly media with violent, sexual, political, or antiauthority content—have raged in the social sciences and public arena across the 20th and 21st centuries. Before detailing the current controversies about violent video games, it is important to

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understand them in the sociological context of media controversies that historically have occurred in the United States and elsewhere.

The history of media violence debates have been covered in detail in several excellent reviews (Cumberbatch, 2008; Kutner & Olson, 2008; Trend, 2007). Concerns about the harmful and corrupting nature of media on consumers, particularly youth, have been recorded since at least the time of the Greeks and Romans. Plato cautioned that plays and poetry might have a deleterious effect on youth, and that society might be better off were they banned (Griswold, 2004). Plato is perhaps the first person on record arguing that children may be unable to distinguish fact from fiction (although research seems to suggest that, in fact, children as young as 3 years old can reliably distinguish fictional and factual events in at least a basic sense. See Woolley & Van Reet, 2006). His mentor, Socrates, is reported to have been suspicious even of the alphabet (McLuhan & Fiore, 1967) as a source of harm. Media violence, in a broad sense, likely reached the nadir of brutality during the Roman Games, which featured public executions, gladiatorial combats, and even plays in which prisoners were slain during death scenes (Coleman, 1990; Wells, 1995). Although the Games were vastly popular, early Christian theorists such as Tertullian (200) and Augustine (397), as well as pagan orators (e.g., Seneca, 64), worried about the moral effects of the Games on spectators. These early comments were largely polemics, as social science in the modern sense did not yet meaningfully exist.

The post-Roman Middle Ages arguably saw less hand-wringing over media influence. Most people could not read and there were few if any plays to see. War, famine, pestilence, and oppressive servitude were likely more imperative concerns. Yet jousting, less brutal than the Games, although still violent, was condemned by the Catholic Church (National Jousting Association, 2008).

During the Reformation the Catholic Church was concerned about non-Latin translations of the Bible which were thought to promote heresy, rebelliousness and sin. Unlike the purveyors of controversial media today who might fear lawsuits or angry e-mails, Renaissance purveyors of controversial media, including the Bible itself might find themselves burned at the stake (Daniell, 2004).

Not all fears about media effects originated from religion although conservative religious groups continue to this day to form one facet of causal hypothesis advocacy (the term "causal hypothesis" will be used to indicate the belief that media violence causes aggression). By the 18th and 19th century many of the concerns were secular in nature, usually originating from established authority figures or society elders concerned about youth, women or immigrant groups (Kirschenbaum, 2007; Trend, 2007). Common targets included novels such as *Don Quixote* and Samuel Richardson's *Pamela* (it was thought that women, in particular, couldn't distinguish fact from fiction; Kirschenbaum, 2007), penny-dreadfuls such as *Varney the Vampire*, political texts, and early films such as *The Great Train Robbery*.

The early film era saw the beginning of modern social science's involvement in controversies about media violence. Concerns about media violence dovetailed with other moral issues of the day including sexuality, temperance, immigration, and a general decline in cultural values, particularly among youth (Trend, 2007). It has been noted that these moral crusades were rooted in a sense of "inerrancy" vis-à-vis a particular set of beliefs. Some groups endorse attitudes that their moral beliefs are "factual," and those of

other groups are wrong (Sherkat & Ellison, 1997). In an effort to head off looming government censorship, the motion picture industry established the Hays Code (1930). Graphic depictions of violence, the techniques of murder or other crimes, smuggling and drug trafficking, the use of liquor (unless required by the plot), revenge, safecracking, train robberies, adultery (which was not to be presented as an attractive option), interracial relationships, sexually transmitted diseases, nudity, and even "lustful kissing" were all forbidden or strictly controlled under the Hays Commission.

Social science research regarding the adverse effects of films began in roughly the 1930s. Perhaps most famous of these were the Payne Fund studies (Blummer, 1933) which purported to find a link between movie viewing and delinquency. Setting the stage for debates that would occur over the next century, critics of the Payne Fund studies noted the lack of control groups, difficulties in validly measuring aggression, and sampling problems as limiting their usefulness (Lowery, & DeFleur, 1995). Taking a "blank slate" approach on child development, the Payne Fund studies provided considerable fuel to the fire of belief that media exposure could harm youth.

The advent of TV triggered a new round of concern, particularly when some felt that crime increases beginning in the late 1960s might be attributed to the introduction of TV several decades before (e.g., Bushman & Anderson, 2001; Centerwall, 1989). Although an almost 20-year gap occurred between the mass production of TV and significant crime waves beginning in the late 1960s, the apparent correlation was likely too tempting for moral crusaders. Figure 1 presents murder/non-negligent homicide trends in the United States. It is apparent that such trends appear to form a sine wave pattern. Violent crime spikes of similar magnitude to those of the 1970s and 1980s occurred earlier in the 1930s and 1880s (Ferguson, 2002). Currently, homicide rates are returning to a low point as of 2010, their lowest in four decades. By focusing on a limited period in U.S. crime trends, it was possible to ostensibly link violent crime rates to TV despite that the period in which violent crimes spiked saw massive social upheavals in the United States, as well as a damaged economy and increased poverty and homelessness. The adoption of social learning theory (Bandura, 1965; Bandura et al., 1961/1963) as a leading paradigm by the psychological community beginning in the late 1960s

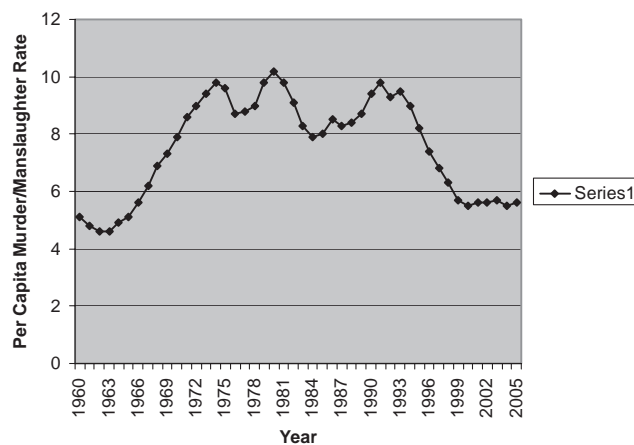


Figure 1. Per Capita Murder/Non-Negligent Manslaughter Rates in the United States by Year.

provided further framework for media violence research taking place in the latter half of the 1960s.

Beginning in the TV era, and continuing into the video game era, the causal hypothesis increasingly has been presented not only as one side of a reasonable debate, or a theory with some support, but rather as a fact (Bushman & Anderson, 2001; Cook, 2000) and a public health crisis on par with smoking and lung cancer (Bushman & Anderson, 2001; Huesmann, 2007), or even a scientific law (Huesmann & Taylor, 2003). Such rhetoric is, of course, quite rare in the social sciences, due to the limitations of social science research (Uttal, 2007), and even uncommon in the “hard” sciences. This rhetoric appears to have, if anything, increased in direct contrast to plummeting crime rates in the United States (Federal Bureau of Investigations [FBI], 1951–2009) and more frequent criticisms of media violent research (e.g., Freedman, 2002; Grimes et al., 2008; Olson, 2004; Savage, 2004; Savage & Yancey, 2008; Trend, 2007; Unsworth, Devilly, & Ward, 2007). Given the rarity of such rhetoric in the social sciences, concerns have been raised that psychology’s focus on media violence effects as “truth” may have less in common with the objective sciences of physics, chemistry, and biology, and more in common with moral advocacy crusades such as temperance and anti-pornography crusades (Gauntlett, 1995; Grimes et al., 2008).

### The Public Debate Regarding Video Game Violence

Arguably the debate regarding video game violence in the scientific community only indirectly influences the debate about video game violence in the public. The news cycle about video games tends to focus on three main phenomena: the release of controversial games such as the *Grand Theft Auto* series, unsupported statements by nonscientists such as Jack Thompson and David Grossman (two antivideo game violence activists), and efforts to tie individual real-life violent crimes to violent games, despite evidence that violent crimes, including youth crimes, generally are decreasing. Far from being independent, these three phenomena oftentimes intersect. For instance, the publicity and hyperbole over the release of *Grand Theft Auto IV* in 2008 seem to have provided a convenient excuse for the criminal behavior of some young men (e.g., Gamepolitics.com, 2008a), notwithstanding evidence that these young men often had prior criminal records. Naturally, criminal offenders oftentimes blame others for their crimes, whether their parents, their victims, alcohol use, society, a bad childhood, or the media. Most such excuses would not be given much credence, but claims by some offenders that video games motivated them to commit crimes sometimes are given unusual credibility.

Nonresearchers also fuel the debate at times by providing misinformation. For instance Florida attorney and antigame activist Jack Thompson claimed that the Virginia Tech Shooter, who killed 32 people at a university in Virginia in 2007, was an avid player of violent video games (Thompson, 2007). Yet the Virginia Tech Review Panel ultimately found that Seung-Hui Cho, the shooter, did not play violent games at all (Virginia Tech Review Panel, 2007). Similarly, psychologist Cooper Lawrence supported a Fox News claim that the game *Mass Effect* contained full frontal nudity and pornographic scenes, which it did not (Gamepolitics.com, 2008b). These public statements of misinformation naturally inflame an existing moral panic rather than inform the populace.

Perhaps most respected of the causal hypothesis activists has been David Grossman (1996) who has written several books on video games and refers to himself as an expert in “killology” who claims that violent games mimic combat simulators used by the military and ultimately desensitize and train youth to kill. Grossman’s main argument is that, as the military has begun using video simulators, modern troops are much more likely to fire at the enemy than troops during World War II (WWII). He neglects to note that comparing conscripted, poorly trained, nonprofessional WWII soldiers firing primarily the semiautomatic M1 with limited ammunition, to the modern volunteer, professional, highly trained soldier firing primarily the fully automatic M16 or M4 is a manifest example of comparing apples to oranges. Changes in training regarding selecting specific targets (in WWII) versus using “blind” suppressing fire (in the modern army) better explain differences in firing rates, than do any use of simulators. Why police organizations would use similar simulators to *decrease* bad shootings is also not well explained in Grossman’s treatise. Grossman also claims that media violence generally, and video game violence specifically, are powerful predictors of youth violence rates (Grossman & Degaetano, 1999) although this conclusion does not acknowledge the significant decline in youth violence rates since 1993.

Exactly what impact such debate has on the perceptions of individual news consumers or parents is unclear. Some research suggests that most parents express some concern that *other* children may be influenced by violent games, but that their own are not (Kutner, Olson, Warner, & Hertzog, 2007). Public fears about violent video games may be further assuaged by a recent study by the Pew Research Center (Lenhart et al., 2008) that found video game playing, including games with violent content, is nearly ubiquitous among youth with 97% of youth playing some form of video games and that negative impact appeared to be negligible. In fact, video game playing was found to offer significant opportunities for social interaction and civic engagement. Similarly, Durkin and Barber (2002) found that frequent video game playing children were better adjusted than nonplaying peers. It is this positive side of video games, violent games included, that receives little attention from either the public or the scientific community.

### The Etiology of Moral Panic

A moral panic may be defined as a quest by some members of society to impose their moral beliefs on the greater society through the tactic of fear (Cohen, 1972; Gauntlett, 1995). Most commonly, established or “elder” members indulge in moral panics as a means of maintaining the status quo, maintaining control over communication in the society, and reigning in the independence of youth. Politicians, scientists and scientific organizations, religious figures and advocacy groups may all contribute to moral panics. Moral panics may also serve to distract society from concerns that are intractable, complex or embarrassing to established authority, replacing them with “folk devils” which can deflect blame for societal problems (Cohen, 1972). One essence of a “moral panic” is that previous “folk devils” are often forgotten, particularly when dire consequences do not seem to originate from their existence, with new “folk devils” taking their place (Cohen, 1972). Examples of recent moral panics include “rainbow” oral sex parties among teens (Lewin, 2005) and ritual Satanic abuse (Jewkes, 2004).

Gauntlett (1995) developed a model for how media violence has functioned as a moral panic. An updated version by Ferguson (2008) is presented here in Figure 2. The moral panic wheel is spun, if you will, by general societal beliefs. These societal beliefs may be cultural, or religious, or may be supplied by society's authoritarian political, scientific, and activist elements. In modern mass-media society, society's concerns about a particular phenomenon are reported in news media, oftentimes implying that the concern is "fact." Research is called for which will support the initial concern. Given the relatively low threshold for supporting theory in psychology and an emphasis on positive statistical significance over null results, including the "publication bias" effect (Cohen, 1994; Loftus, 1996; Rosenthal & Rosnow, 1991; Uttal, 2007), research that supports the original concern is obligingly provided. The claims of such research may match poorly with the data; indeed the claims made may be excessively conclusive yet largely escape scrutiny. By contrast, findings that disconfirm the moral panic, or that criticize the supportive social science are largely ignored. Findings that reinforce the panic are dutifully reported in the media, then borrowed by politicians for political gain, perpetuating the wheel. In this model, the authority figures of society all generally "win" through the promotion of fear. Politicians and other "elders" of society retain authoritarian control over the masses. The youth are portrayed as dangerous, in need of a firm but caring adult hand to keep them from moral decrepitude. Scientists benefit both by perpetuating existing dogma, and by securing grant funding (it is difficult to attain grant funding by arguing that something is *not* a pressing social concern, after all). It has been observed that scientists have participated in moral panics at various times including over sports fan violence (Moorhouse, 1991; Ward, 2002), belief in emerging juvenile superpredators (Muschert, 2007) or the "death" of childhood (Darbyshire, 2007). Perhaps more famously, a meta-analysis that questioned the harmful effects of child sexual abuse (Rind, Tromovich, & Bauserman, 1998) kicked off a protracted if understandable emotional reaction

from both the political and scientific community (see Dallam, 2001 for a time-line) that may be akin to a moral panic (this was a rare time in which a peer-reviewed scientific article was censored by congress).

### The Predominance of Social Learning Theory and Its Consequences

In the early 21st century, the paradigm of social learning arguably retains dominance in psychology. This paradigm is based upon the work of Bandura purporting to demonstrate that aggression, in particular, can be modeled (Bandura, 1965; Bandura et al., 1961/1963). Briefly, the famous Bandura studies involved children watching videos of adult models hitting a bo-bo doll. Children were then frustrated by the experimenters, and placed in a room with a bo-bo doll. Children who had seen the video of the adult acting aggressively toward the bo-bo doll were more likely to mimic the behavior themselves, unless they witnessed the model get punished for those actions. The Bandura experiments are not beyond reproach (see Gauntlett, 1995). For instance, far from being a powerful lasting influence, modeling effects witnessed appear to be small and evaporate quickly. It is unclear whether the children were necessarily more motivated to engage in aggression in general, as opposed to mimicking *specific* aggressive acts. In other words, overall aggressive behaviors may not have changed much, but the style of the aggressive behaviors might have been altered due to the novel kinds of aggressive behaviors presented. It is also unclear that the children were necessarily motivated by aggression, as opposed to aggressive play or even the desire to please the adult experimenter. Children are quite used to being given instructions by adults and they may arguably have simply viewed the models (who were adults) as instructors telling them what to do. In other words, the children may have even believed that they might be scolded or punished if they didn't follow the model's lead. Lastly, in a subsequent paper, Bandura (1965) found

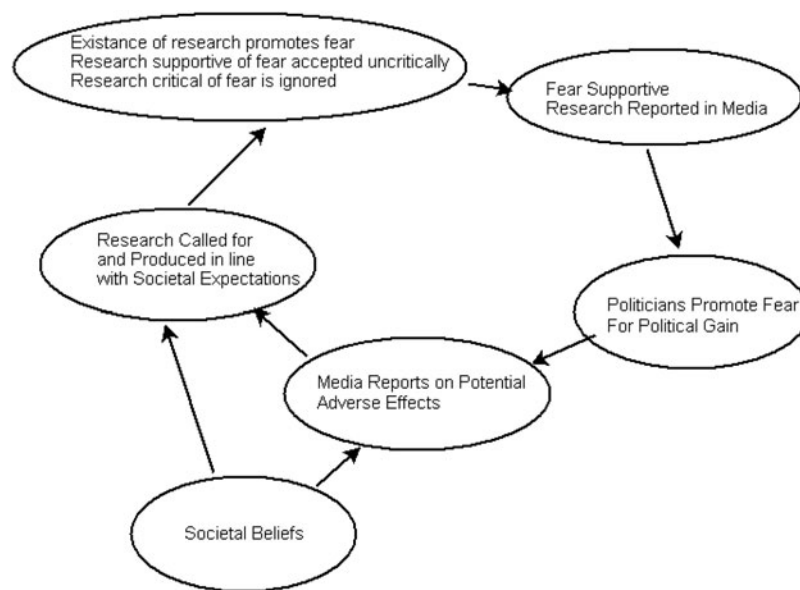


Figure 2. The Moral Panic Wheel.

that showing the model being punished for attacking the bo-bo doll decreased modeled behaviors in child participants. Yet the punishments themselves appeared to involved considerable aggressive behavior. As described in the original text (Bandura, 1965. p. 591):

For children in the model-punished condition, the reinforcing agent appeared on the scene [this occurs after the children watched the model hit the bo-bo doll] shaking his finger menacingly and commenting reprovingly, "Hey there you big bully. You quit picking on that clown. I won't tolerate it." As the model drew back he tripped and fell, and the other adult sat on the model and spanked him with a rolled up magazine while reminding him of his aggressive behavior. As the model ran off, cowering, the agent forewarned him, "If I catch you doing that again, you big bully, I'll give you a hard spanking. You quit acting that way."

From this description it is reasonable to wonder what we can conclude when it appears that children are willing to imitate nonviolent aggression against an object, but viewing violence against an actual person inhibits their aggression. We may conclude that perhaps the children were able to judge the context of the aggression, unreal bo-bo doll or live human, or violence which was rewarded in one case and punished in the other. Of course following logic down this past might lead to speculation that modeling is more complex than the mechanistic law portrayed by some (e.g., Huesmann & Taylor, 2003). Indeed, modeling might be something that humans *can* do rather than something that they *must* do. Modeling may be motivated by intrapsychic factors (motivation, biological instincts, personality, perhaps even agency) rather than controlled by immutable laws of external cause and effect.

Modeling certainly can occur and can be a good way for humans to learn procedures such as tying shoes or driving a car. It is less clear that modeling *must* happen or that it can fundamentally change the brain and personality of the viewer. Nonetheless social learning theory in various forms retains prominence in most subfields of psychology, including aggression, sometimes pushing out competing theories such as evolutionary and genetic theories that may be considered solid in other fields (Buss & Shackelford, 1997). Unlike evolutionary and genetic theories, which are the product of the biological and medical sciences, social learning is a unique creation of psychology itself. Social learning is psychology's one true contribution to modern understanding of human behavior. Thus, it may not be surprising to see social learning deified and presented as more consistent, more mechanistic and applied to behaviors and constructs beyond its natural reach. The APA's youth violence brochure (1996) provides one such example, downplaying the influence of genes and promotes the influence of social learning. The pamphlet has not yet been updated despite evidence that the interplay between learning and genetics is much more complex (Caspi et al., 2002; Ferguson, 2010; Rhee & Waldman, 2002, see also Ferguson, 2009; 2009a).

The risk of such a deification of social learning is that it becomes a dogma to be protected, as opposed to a science to be corrected, at least in some subfields. This is not meant to say that social learning is universally *wrong* just that it is not so much universally *right* as some psychologists may assume. Indeed it may be a weaker explanation for some phenomenon, including aggression, than are evolutionary and genetic explanations (Ferguson, 2010). For instance the U.S. Department of Health and Human Services (2001) concludes that risk markers for youth violence that

many psychologists would likely consider quite strong, such as media and family violence exposure, are in fact weak predictors at best of youth violence.

As scientific dogma and moral panic are both intended to protect the established order from change (Gauntlett, 1995; Kuhn, 1970) they can dovetail easily within social science. Scientific dogmas, like moral panics, rigidly defend established beliefs and present opinion as "fact," oftentimes resting on impressive looking but ultimately weak, distorted and unreliable data (Uttal, 2007). The millennia-long dominance of humoral theories of disease, as well as the related miasma theory of disease transmission provides examples of both the difficulties in overthrowing established scientific dogma, and the harm to scientific inquiry and society generally that dogma can do (Johnson, 2006). The question before us is whether the concerns among psychologists about violent video games' alleged negative effects are real or dogma, and what effect such concerns have had on the potential utility of violent games as force for potential societal benefit.

### Violent Video Games and Aggression: State of the Research

If social learning, in a mechanistic lawfully determined form, has become a dogma in social science, it has almost certainly permeated media violence studies. In media violence studies, claims of causal certainty are unprecedented (e.g., Anderson et al., 2003; Bushman & Anderson, 2001; Huesmann, 2007). These claims, such as that the impact of media violence on viewers approaches that of smoking and lung cancer, should have invited close scrutiny and skepticism from the scientific community. Instead they have been embraced by professional scientific organizations with minimal oversight. For instance the American Academy of Pediatrics (AAP, 2009; Cook, 2000) in support of media violence effects research has claimed that 3,500 studies have been conducted on media violence with only 18 finding null effects. In fact most meta-analyses and reviews (Bushman & Huesmann, 2006; Freedman, 2002; Paik & Comstock, 1994) can only find between 200 and 300 articles including unpublished and non peer-reviewed studies. Indeed this 3500/18 statistic is apocryphal and now well-known to be an "urban legend" (Freedman, 2002), yet was restated verbatim by the AAP 10 years later (AAP, 2009) despite that it could only still be true after 10 years if no research on media violence had taken place in the interim.

Similar claims of certainty have emerged within the violent video games effect literature (Anderson, 2004; Carnagey & Anderson, 2004). Once again oversight and adequate peer review appears to have been minimal. The 2005 American Psychological Association resolution on video game violence (APA, 2005) was written by a committee of causal hypothesis scholars commenting largely on their own work and ignoring that of skeptics or research with opposing findings (e.g., Colwell & Kato, 2003; Durkin & Barber, 2002; Freedman, 2002; Olson, 2004; Savage, 2004; Williams & Skoric, 2005). It is concerning that a more balanced panel of experts was not convened, calling into question the objectivity of the resolution.

A close look at the research on violence in video games reveals that findings are far less consistent than have been reported by some sources. For instance, while some research does find an effect for violent game playing on aggression (e.g., Anderson &

Murphy, 2003; Bartholow, Bushman, & Sestir, 2006) others clearly do not (e.g., Baldaro et al., 2004; Durkin & Barber, 2002; Ferguson & Rueda, in press; Ferguson, Rueda, et al., 2008; Ferguson, San Miguel, & Hartley, 2009; van Schie & Wiegman, 1997; Wiegman & van Schie, 1998; Williams & Skoric, 2005). Some find that exposure to violent games is related to *reduced* aggression (e.g., Barnett, Coulson, & Foreman, 2008; Colwell & Kato, 2003; Unsworth et al., 2007) and others claim to have found effects, but a close examination of their results demonstrates that they have not (Anderson & Dill, 2000; Gentile, Lynch, Linder, & Walsh, 2004). For example, a correlation between video game exposure and aggression all but disappears once gender is controlled in one study (Gentile et al., 2004) and, in a common problem among media violence studies Anderson and Dill (2000) focus on one out of four aggression outcome measures that was significant and ignore the other three that were not in interpreting their results (a Bonferroni correction, if correctly applied, would have rendered all four results nonsignificant). Indeed the Anderson and Dill (2000) paper provides an example of the limitations of the peer-review process that has overseen the violent video games effects literature. The error in interpretation is readily apparent upon reading the results section of their experimental study, but appears to have escaped notice during peer-review and the paper continues to be influential if misleading.

Meta-analyses of video games similarly produce weak and inconsistent results. Two early meta-analyses claimed to find small but significant effects for video game violence on aggression (Anderson & Bushman, 2001; Anderson, 2004), although a subsequent review of these meta-analyses during a court case revealed that the authors may have simply ignored research that didn't fit with their hypotheses (ESA, VSDA and IRMA v. Blagojevich, Madigan, & Devine, 2005). The Illinois case cited above mentions this issue, noting that:

"With regard to their conclusions, Dr. Goldstein and Dr. Williams noted that Dr. Anderson not only had failed to cite any peer-reviewed studies that had shown a definitive causal link between violent video game play and aggression, but had also ignored research that reached conflicting conclusions. Dr. Goldstein and Dr. Williams noted that several studies concluded that there was no relationship between these two variables. They also cited studies concluding that in certain instances, there was a *negative* relationship between violent video game play and aggressive thoughts and behavior (e.g., initial increases in aggression wore off if the individual was allowed to play violent video game for longer period)." (ESA, VSDA, and IRMA v. Blagojevich, Madigan, & Devine, 2005, pp. 14–15).

The court also expresses similar concerns about cherry-picking of data by politicians involved in enacting antigame legislation, noting (ESA, VSDA, and IRMA v. Blagojevich, Madigan, & Devine, 2005, p. 16):

Finally, the Court is concerned that the legislative record does not indicate that the Illinois General Assembly considered any of the evidence that showed no relationship or a negative relationship between violent video game play and increases in aggressive thoughts and behavior. The legislative record included none of the articles cited by Dr. Goldstein or Dr. Williams. It included no data whatsoever that was critical of research finding a causal link between violent video game play and aggression. These omissions further undermine defendants' claim that the legislature made "reasonable inferences" from the scientific literature based on "substantial evidence."

A contemporary meta-analysis by Sherry (2001) found only weak effects, and Sherry concluded that any effects were weaker than for TV, contradicting concerns that the active nature of video games may produce higher effects on aggression. In a follow-up analysis Sherry (2007) concluded that the video game violence research currently available did not support the social learning view of aggression effects questioning "Further, why do some researchers (e.g., Gentile & Anderson, 2003) continue to argue that video games are dangerous despite evidence to the contrary?" (p. 244). This conclusion was supported by two meta-analyses by Ferguson (2007a/2007b) who concluded that video game violence effects research had been deeply affected by publication bias problems which inflated reported effect sizes, and the misuse of unstandardized unreliable measures of aggression, which allowed researchers too much latitude in picking results that supported their hypotheses. A third recent meta-analysis of media effects generally (Ferguson & Kilburn, 2009) reached similar conclusions for both TV and video games. Neither form of media was associated with increased aggressive behavior, and better validated measures produced weaker results than poorer measures. Also, the claim that video games may produce higher effects due to their interactive nature was not found to be supported. From the social learning perspective, it would have seemed plausible that the interactive nature of video games might have produced higher effects for aggression than for other forms of media. Effect sizes, even from meta-analyses that claim support for violent game effects, do not offer support for this conclusion. For instance in the recent Ferguson and Kilburn (2009) meta-analysis, there effectively was no difference in the (negligible) effect size for video games and TV on aggressive behavior.

Publication bias, the selective publishing of positive effects in journal articles is also well demonstrated in the Anderson (2004) meta-analysis in which a plurality of published studies demonstrate statistically significant (if weak) effects, yet almost all unpublished studies included demonstrate null effects. Yet in their most recent update (Anderson et al., 2010) Anderson's team are critical of publication bias analyses (e.g., Ferguson & Kilburn, 2009), suggesting that including unpublished studies is a better fix for publication bias. Yet as noted by an accompanying commentary (Ferguson & Kilburn, 2010) Anderson et al include a number of their own unpublished articles while failing to solicit those from other groups, particularly from researchers skeptical of their own findings (apparently replicating the issues identified in the ESA, VSDA, and IRMA v. Blagojevich, Madigan, & Devine, 2005 court case). Such a biased sample of unpublished studies highlights the potential pitfalls of including unpublished studies in research. This most recent Anderson et al meta-analysis also contains significant flaws: including numerous studies with measures which are unstandardized or have poor validity, focusing on bivariate correlations rather than better controlled estimates of video game effects, failing to include some published work from authors critical of the causal hypothesis and including an unreliable evaluation of "best practice" studies that conflates measurement error with "best practices" due to the failure to consider the impact of unstandardized assessment measures. Despite a number of flaws, all likely to inflate effect size estimates, the authors nonetheless find only weak effects for video game violence ( $r = .15$ ). Unfortunately in arguing against a comprehensive statistical analysis of publication bias in

favor of a clearly flawed and biased effort to include unpublished studies, the authors argue against rigor rather than for it.

Taken together these meta-analyses range from those which argue against meaningful effects (Sherry, 2001, 2007; Ferguson 2007a; 2007b; Ferguson & Kilburn, 2009) to those which find weak effects (e.g., Anderson, 2004; Anderson et al., 2010). Thus the debate on video game violence has been reduced to whether video game violence produces no effects, or almost no effects.

### Methodological and Theoretical Problems That Limit Our Interpretation of Video Game Violence and Aggression Research

For a research field to have addressed the level of certainty claimed by some researchers (e.g., Anderson et al., 2003; Bushman & Anderson, 2001; Huesmann, 2007) it must rest on solid methodology, firm theoretical footing, and highly invariant findings with a high degree of predictability in relation to real-world phenomenon (Uttal, 2007). This high level of consistency is what has allowed certain physical phenomenon to reach "law" status, whereas there have not been any behavioral equivalents in psychology. Other scholars have pointed to vast methodological and theoretical problems with media effects research in general, and violent video game effects specifically (Freedman, 2002; Gauntlett, 1995; Grimes, Anderson, & Bergent, 2008; Olson, 2004; Savage, 2004; Trend, 2007). These issues are described briefly below:

**1) Many aggression measures used are invalid.** Put simply, many measures used in video game studies claiming to represent "aggression" in fact don't correlate well with actual real-life aggressive acts or violent behaviors (Ferguson & Rueda, 2009; Ferguson, Smith, Miller-Stratton, Fritz, & Heinrich, 2008; Ritter & Eslea, 2005; Tedeschi & Quigley, 2000). Although this appears to be a near-universal problem for laboratory measures of aggression, many survey instruments such as peer and teacher nominated aggression also appear to obtain weak validity coefficients (e.g., Henry & Metropolitan Area Child Study Research Group, 2006).

**2) The "third variable" effect.** This concern is that other variables such as gender, family violence, genetics, and so forth, may account for any small relationship between violent video game exposure and aggression (Ferguson, 2007b; Freedman, 2002; Savage, 2004). Univariate statistics may be overinterpreted at the expense of multivariate statistics. For example, as noted above Gentile et al., (2004) overinterpret bivariate correlations between violent video games and aggression, and fail to note that controlling for gender alone removes most of the overlapping variance. As boys are both more aggressive and play more violent video games than do girls, any bivariate correlation may simply be masking an underlying gender effect. A recent paper by Anderson et al., (2008) provide another example in which video game violence account for only half a percent to 2% of the variance in aggression in a longitudinal study of children, whereas other relevant factors such as family environment, peer environment, poverty, and genetics were not controlled nor even discussed as alternate explanations for such a weak correlational link. Antgame research is not unique in this respect; Kutner and Olson's (2008) research on video games, although finding few meaningful effects, largely relies on small bivariate correlations, most of which would likely have evaporated altogether had gender, trait aggression or family violence been well controlled. In support of this concern,

Ferguson, Rueda et al., (2008) in a multivariate analysis, found that trait aggression and family violence were predictive of violent criminal acts, whereas violent game exposure was not. Ferguson, San Miguel, and Hartley (2009) similarly found the video game and TV violence held no predictive value for serious youth violence, once other factors, primarily peer delinquency, antisocial personality, and depression were controlled.

**3) Citation bias.** Numerous critics have noted that media effects scholars ignore work, even from their own results, which contradicts their hypotheses (Freedman, 2002; Gauntlett, 1995; Moeller, 2005; Savage, 2004). As noted earlier in studies such as Anderson and Dill (2000) and Gentile et al., (2004) this issue continues to be a concern for video game violence effects.

**4) Publication bias.** Although this is certainly not an issue specific to video game violence, studies of video game violence appear to be deeply influenced by publication bias (Ferguson, 2007a/2007b). Studies with statistically significant effects, no matter how small in practical effect, are more likely to be published than those with null results. Although a problem throughout published research, in an atmosphere of moral panic, publication bias effects are likely to become greater in magnitude. This has been demonstrated both through statistical publication bias analyses (Ferguson 2007a;2007b) as well as through examining differences between published and unpublished studies (e.g., Anderson, 2004). Ioannidis (2005) notes that publication bias and bias more generally are highly likely for research fields with small effect sizes, small studies, great flexibility in designs, measurement and analysis, and "hotter" and more political issues raised, all clear issues for the violent video game field.

**5) Small effect sizes.** Estimates on the size of effect for violent video games on aggressive behavior range from (using  $r^2 \times 100$ ) effectively zero through 2.5% (Anderson & Bushman, 2001; Anderson, 2004; Anderson et al., 2010; Ferguson 2007a/2007b; Sherry, 2001/2007). Many scholars have argued that these effects, even if assumed to have been produced by methodologically perfect research, are too small to be meaningful (Ferguson, 2002; Freedman, 2002; Gauntlett, 1995; Olson, 2004; Savage, 2004; Sherry, 2007). Some scholars have countered that these effects are similar to those found in smoking and lung cancer research or other medical effects (e.g., Anderson, 2004; Huesmann, 2007) although these claims appear to be based on faulty statistics which underestimate medical effect sizes (Block & Crain, 2007; Crow, 1991; Ferguson, 2009b; Hsu, 2004).

**6) Absence of clinical cut-offs.** In clinical psychology, to make a claim such as "eating avocados causes schizophrenia" it would be necessary to demonstrate that avocado eaters were pushed over a clinically meaningful score on a clinical measure of schizophrenia with documented sensitivity and specificity for detecting the disorder. It wouldn't be enough to demonstrate that avocado eaters had a t-score of 52 on the MMPI 8-Scale, when nonavocados had only a t-score of 49 (these score differences are about on the magnitude of aggression score differences seen in violent video game effects research). A t-score of 52 or 49 are both well within the normal range (t-score means are 50 with  $SD = 10$ ). Yet this is exactly what occurs in video game violence research. The absence of clinical cut-offs on aggression measures make it impossible to document whether a particular variable influences *pathological* aggression. Instead it is assumed that aggression has no adaptive function and is always pathological and undesirable.

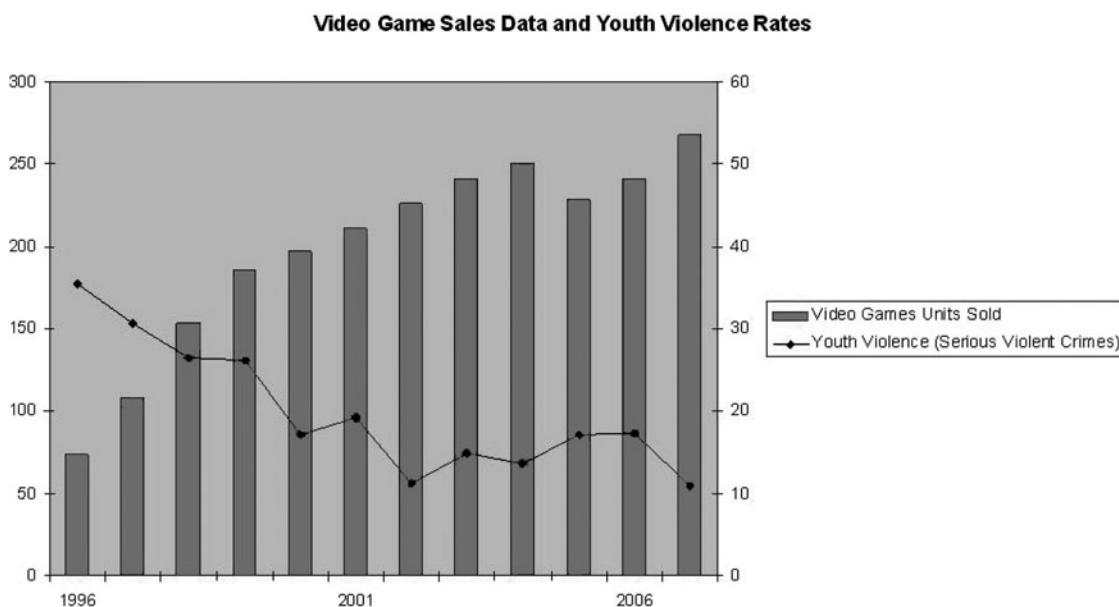
This would appear to be naïve, and at best is an assumption. In moderate doses, aggression may very well be adaptive, guiding individuals toward many behaviors approved of by society including standing up for one's beliefs, assertiveness, defending others in need, careers in law enforcement, the military, business, legal affairs, and so forth, sporting activities, political involvement, debate and discourse indeed including scientific debate (Hawley & Vaughn, 2003; Smith, 2007). Particularly as most video game research uses individuals who may be expected to be below average in aggression, such as college students or healthy children, we should be wary of regression to the mean effects. In the absence of clinical cut-offs aggression scores remain difficult to interpret.

**7) Unstandardized use of aggression measures.** As noted in Ferguson (2007a) one significant concern is that some measures of aggression, such as the modified Taylor Competitive Reaction Time Test have been used in an unstandardized way. Different studies use this test to measure aggression in far different ways. Sometimes even the same authors use the measure differently between studies. Ferguson found that measures used in such an unstandardized way resulted in higher effect sizes, likely as authors were free to choose outcomes that supported their hypotheses and ignore outcomes which did not.

**8) The mismatch between violent video game consumption and violent crimes.** Put simply, this issue notes that both public and scientific concern (Anderson, 2004, refers to violent video game effects as "somewhat alarming" p 120) is not matched by violent crime data, which for both adults and youth (Childstats.gov, 2008; FBI, 1951–2007) has plummeted at the same time as video games have increased in popularity. Figure 3 presents these data for all the years available for both sets of data on video game

sales and youth violence (Entertainment Software Administration, 2007; Childstats.gov, 2008). As can be seen, as video games sold increased in number, violent crimes among youth have declined. The statistical relationship between these two variables is  $r = -.95$ . It should be carefully noted that video game consumption is unlikely to be responsible for this decline (e.g., ecological fallacy), even in part. Just as the alleged correlation between TV introduction and violent crime increases 20 years later proved to be illusory, so too is this correlation most likely due to other factors. However, we can be sure that violent video games have not sparked a violent crime epidemic because there is no violent crime epidemic. The violent video game issue is a crusade in search of a crisis. Some causal hypothesis scholars may claim that this real-world data does not matter, but the same scholars often pointed to violent crime trends when they appeared to work in the favor of their hypotheses (e.g., Bushman & Anderson, 2001) or raise the issue of youth violence while ignoring youth violence data (e.g., Anderson et al., 2008). Claiming that video game effects theories need never relate to actual world phenomenon is a pseudoscientific claim, one that is more akin to a moral panic than objective science.

**9) Low standards of evidence.** As noted above, one issue that is oftentimes raised (Freedman, 2002; Ferguson, 2007b; Olson, 2004; Savage, 2004) is that video game violence effects, like those in media violence more broadly, are small, but that some authors exaggerate their significance promoting unnecessary concern. This is likely not unique to video game studies, as the standards of evidence for psychological theories are universally low (Uttal, 2007). Specifically, demonstrating "statistical significance" is not an adequate method for determining the utility of theories in



Video game sales data source: The NPD Group, Inc./Retail Tracking Service

Youth violence data source: Childstats.gov

Figure 3. Youth Violence and Video Game Sales Data.



explaining events (Cohen, 1994; Loftus, 1996) particularly when increasing sample sizes can make almost any effect statistically significant. Some researchers (e.g., Anderson, 2002) have cynically encouraged researchers to chase statistical significance by increasing sample sizes, in tacit acknowledgment that effects are small. Given that null results are easily dismissed as type-II errors, it is not practically feasible to falsify psychological theories. Thus a weak theory, such as the causal hypothesis may remain influential despite frail evidence (Grimes et al., 2008).

One other issue that bears mentioning is that some studies do not clearly distinguish violent from non violent video games in making contrasts. For example, Konijn, Nije Bijvank, and Bushman (2007) examined the impact of playing violent video games on young boys. The authors found that boys who played a violent game were more likely to select these supposedly damaging noise levels on the Taylor Competitive Reaction Time Test (TCRTT). However, when a personality variable, namely identification with aggressive role models was controlled, the direct impact of video games became negligible. Of greater concern however, was that several of the “nonviolent” games that the authors test (*The Sims 2*, *Tony Hawk’s Underground 2*, *Final Fantasy*) have actually received Entertainment Software Ratings Board (ESRB) content descriptors for violence. As such, the authors appear to have unsatisfactorily distinguished nonviolent from violent games. Thus any alleged difference between game conditions cannot be due to violence, since the researchers inadequately controlled for violence in the “nonviolent” game condition. It is possible, instead, that participants may have picked up on demand characteristics for their performance. Other studies may compare relatively primitive and outdated games such as *Tetris* against complex, graphical, story-driven games with violent content. Such game conditions vary on multiple levels (complexity, characterization, storyline, presence of dialogue, etc.) that it is not possible to attribute any differences to violence. Such studies lack internal validity.

### The Positive Effects of Violent Video Games

To many, the very idea that violent video games can have a positive impact will seem absurd. Yet I’d argue that this is an emotional reaction, not an objective one, particularly if it rises up before even the evidence is fully heard. A better skepticism would be, if violent video games *don’t* teach aggression, why would they be able to teach anything else? In other words, if social learning of violence doesn’t occur for video game effects, why would we expect that other forms of information can be transmitted via video games?

One possible answer is simply that not all information is the same (Ferguson, 2009a). This is readily apparent, of course, as children can be watched eagerly modeling fun tasks, while ignoring modeling opportunities for chores. As discussed earlier, the Bandura studies themselves find that even very young children are selective in what they model. However in the following section, this paper will discuss a different kind of information than that assumed to transfer via aggression. Namely, for the media effects on aggression hypothesis to work, viewers must learn to shape their internal goals, motivation, and core personality as a result of media exposure. As already argued, the evidence that this happens appears to be quite thin (although it remains plausible that some small segment of the population may be at risk, whereas the

majority are unaffected, see Markey & Markey, 2010). By contrast, other kinds of learning, such as visuospatial cognitions, information about medical diseases, science and math skills, and so forth, don’t require internal shifts in a largely stable personality. Put more simply, video games may be effective in communicating raw data or information, but they aren’t effective in transmitting moral beliefs, personality traits, and so forth. Information transfers but personality traits such as aggressiveness do not. A few of the most promising positive developments regarding video games, including those with violence, are outlined below. It should be noted at the outset that, like the research on aggression, none of these research fields are without flaws. Indeed the merits of all research, positive or negative, should be subject to more intense scrutiny than has previously been the case. However, an intelligent discussion of violent game effects needs to consider both sides of the coin if it is to escape the realm of moral panic.

**Visuospatial cognition.** Broadly defined visuospatial cognition involves intellectual and cognitive processes related to attending, scanning, selecting, processing, and mentally altering visual information (Ferguson, 2007a; Green & Bavelier, 2007; Shah & Freedman, 2003; Spence & Feng, 2010). These tasks are oftentimes thought to relate to the performance subtests on common intelligence tests such as the Wechsler scales (e.g., Atkinson, et al., 2003; Kramer, Kaplan, Share, & Huckleba, 1999) and may be important for career paths involving visual acuity and processing.

Currently, a number of studies, both experimental and correlational, have found that playing violent video games is associated with higher visuospatial acuity, perception, processing, visual memory, and mental rotation (Castel, Pratt, & Drummond, 2005; Feng, Spence, & Pratt, 2007; Ferguson, Cruz, & Rueda, 2008; Green & Bavelier, 2007; Green & Bavelier, 2006; Green & Bavelier, 2003; Rosser et al., 2007). For reasons that are not well understood, results for nonviolent games such as *Tetris* are considerably weaker (e.g., Quaiser-Pohl, Geiser, & Lehmann, 2006; Sims & Mayer, 2002). A recent meta-analysis of studies examining the effects of violent video games on visuospatial cognition (Ferguson, 2007b) found that, after controlling for publication bias effects, video games still had a moderate effect on visuospatial cognition ( $r = .36$ ) where as no effect was found in studies of aggressive behavior ( $r = .04$ ). These results appear to support the earlier suggestion that cognitive information can be transmitted more easily via video games than can moral information or personality traits. It is not clear that the violence, per se, is the primary agent of increased visuospatial cognition. Rather, it may be the type of fast action commonly found in a first-person-shooter game that increases visuospatial cognition rather than violence per se (see Spence & Feng, 2010 for an in-depth discussion). Nonetheless these game engines may be of practical value. Smith and Trencholme (2009) recently found that a simulator based on the Source Engine used in *Half-Life 2* was effective in improving participant performance during evacuation fire drills.

It should be noted that, in comparison to the research on video games and aggression, research on violent video games and visuospatial cognition is much more consistent, yet at the same time generally smaller regarding the number of studies. Many of the studies use relatively small sample sizes, oftentimes of convenience samples (although as stated earlier, this occurs commonly for aggression studies too). The generalizability of laboratory studies of visuospatial cognition, particularly those that don’t use

well validated measures such as the Wechsler scales, to real-life visuospatial tasks may also be limited, much as the generalizability of many aggression measures is limited. In short, this is a relatively new research field. Thus far promising and interesting results have been found, yet more research is needed, particularly on ecologically valid visuospatial tasks. At the same time it should be noted that one recent study (Boot, Kramer, Simons, Fabiani, & Gratton, in press) found that although expert gamers were better at visuospatial tasks than were nongamers, this effect was difficult to replicate through practice in the lab with nongamers. We must be cautious in not rushing to judgment regarding visuospatial skills related outcomes, as has happened in the debates on aggression effects. As a side issue it is interesting to note that most publications in this research area prefer the term “action” game rather than “violent” game (notwithstanding that many of the same or similar games are used). Choosing the term “violent” as opposed to “action” (or vice versa) appears to be a clear effort by researchers to frame the debate in prosocial or antisocial terms which once again might be expected in the atmosphere of moral panic. “Action” games are violent games, although much of the research detailing “action games” has focused on the first-person-shooter genre of violent game.

**Social Involvement.** One common concern is that video games impair social connections for youth (e.g., Putnam, 2000). It is feared that video games may lead to reduced social skills and fewer friendships among youth players. However, beginning in the 1990s, easy access to the Internet expanded games into a new realm for potential social interaction (Olson, 2010). Players could now play first-person-shooter games such as *Medal of Honor* against (or in cooperation with) other players online. Some games allowed for entire social communities to develop online. Collectively called Massively MultiPlayer On-Line Role Playing Games (MMORPG), games such as *Everquest* and *World of Warcraft* allowed for complex social interactions to occur within the game world (Barnett & Coulson, 2010). Current evidence suggests that social connections formed through such online games can be very deep and meaningful to those involved (Murphy, 2007; Yee, 2006).

The Pew Research Center’s study on youth and video games (Lenhart et al., 2008) found that video games, far from being an isolationist activity, were highly social activities for most children. Children who engaged in highly social interactions while playing video games were also highly likely to take an interest in civic involvement (see also Bers, 2010). Violent and nonviolent games appear to be equally predictive of such involvement. Somewhat by contrast Williams (2006) and Smyth (2007) found something of a trade-off, with online social and civic engagements replacing some off-line engagements.

Although research evidence in this field remains thin, there appears to be little concern that video game play necessarily impairs social functioning in other realms. Perhaps a more pertinent question is whether interactive video game play may influence the social functioning of those already low in social skills, particularly shy individuals who find in-person social interaction to be anxiety provoking. Some early research suggests that some gamers may indeed use online MMORPGs to meet social needs (Hussain & Griffiths, 2008; Williams et al., 2006). Other research has suggested that positive social experiences online can encourage social experiences off-line (Kobayashi & Ikeda, 2006) and that

video game play is effective in meeting needs for relation with others (Ryan, Rigby, & Przybylski, 2006).

At present, however, there are no controlled trials demonstrating the potential impact of video game play, either socially in real life, or via online play on the social skills and quality of social interactions for youth or adults with social skills deficits. However, such controlled trials may form a fruitful avenue for future research.

**Use in education.** As noted earlier, violent video games appear to be poor conveyors of personality traits related to aggression itself. But can violent video games form a medium in which other kinds of information may pass more freely? Put another way, given that violent video games are quite popular (Lenhart et al., 2008), can they be used as a medium to convey purely informational content? If the violence in the game secures players’ attention, will they then prove able to learn material that may be boring or difficult to attend to in another context?

There has been some evidence to suggest that video games may provide a useful platform for education (Annetta, 2010; Durkin, 2010). Some of the most promising research in this regard has come out of health psychology, where specifically targeted video games have promoted the health of young medical patients (see Kato, 2010). In one remarkable recent study, researchers found that a first-person shooter game *Re-Mission* improved self-efficacy, cancer knowledge, and treatment adherence in teen and young adult cancer patients (Kato, Cole, Bradlyn, & Pollock, 2008). In the game *Re-Mission* players play as a microscopic female robot who is injected into the bodies of cancer patients and blasts cancer cells and infections with a variety of weapons. Arguably the game succeeds because it presents a lively action-oriented platform that holds players’ attention, allowing the educational components of the game greater opportunity for impact. *Re-Mission* takes advantage of the existing, popular first-person shooter format and applies this format for a prosocial purpose.

It might be reasonable to ask whether a nonviolent game wouldn’t be just as successful. Indeed, violence in a game is probably tangential to any educational content (except perhaps in military or police simulators). However, the strategic nature of using violent games for educational purposes is in drawing in video game audiences who intrinsically prefer violent games. Given that boys particularly seem to prefer violent games (Lenhart et al., 2008; Olson, 2010; Olson et al., 2007), ignoring the use of this medium out of hand may be short-sighted. To be direct, educational games are inherently at a disadvantage regarding the time demands of potential child and teen players, vis-à-vis commercial games. To refuse to incorporate design elements that make commercial games successful, at times including violence, places educational games at further disadvantage.

Not surprisingly, given the controversy over violent video games, there have been many educational games that have been developed with minimal violent content (e.g., Lim, 2008; United Nations World Food Program, 2008). Those that include violence such as *Re-Mission* tend to include relatively mild levels of violence. Both nonviolent and mildly violent educational games have demonstrated short-term efficacy for specific educational goals in controlled settings (Asakawa & Gilbert, 2003; Reiber, Smith, & Noah, 1998), yet little research has expanded outcomes to longer-term, global, and ecologically valid results. It is not well understood how nonviolent games and violent games compare in regards

to their potential educational value. One challenge for any educational video game is that it must compete with commercially available games in a medium where short shelf-lives are quite common.

The use of video games directly in educational settings faces several practical constraints, including time commitment limitations and teacher prejudices against video games (Rice, 2007 see also Ceranoglu, 2010 for a discussion of similar issues in play therapy). However, the use of violent video games in informal settings may also promote some cognitive development, although this is usually an unintended element of game play. For instance research in this area has typically focused on *World of Warcraft* (WoW) a MMORPG that has enjoyed an unusually long active life. WoW is a fantasy role-playing game with violent content, for which many players actively participate in message boards and blogs related to the game (Barnett & Coulson, 2010). Some early research has suggested that WoW may promote reading and writing achievement, including among boys who previously had little interest in such activities (Steinkuehler, in press; Steinkuehler & Duncan, 2008; Steinkuehler & Williams, 2006). Similarly VanDeventer and White (2002) found that children who displayed expertise at mildly violent games were likely to display higher-ordered thinking skills.

Research on the use of violent video games in promoting educational agendas remains in infancy. Yet the promise of *Re-Mission* directly, and *World of Warcraft* somewhat indirectly has led to calls for increased use of video games, including those with violent content, to promote educational agendas. For instance, NASA has begun development of an MMO to promote science education (National Aeronautic and Space Association, 2008). The adoption of violent games as potential educational tools will naturally need to take place in the framework of a larger discussion of positive and negative effects of violent games. Yet, given the appeal and staying power of violent games, it may be worth having this discussion.

## Conclusions

Research regarding the impact of violent video games on aggression is inconsistent and hampered by poor methodologies and the intrusion of ideology and scientific dogma. Particularly in light of increased video game consumption and declining youth violence, at present time, there appears to be little reason for speculation that violent video games are a significant factor in promoting youth violence. Unfortunately, by maintaining a myopic view on negative issues related to video game violence, a broader discussion of the benefits and risks of violent game playing is prohibited. It is argued here that if psychology is serious about understanding violent video games from an objective rather than ideological view, a broader and less activist stance must be taken.

Although the research on violent games and aggression appears to be much less solid than some scholars have indicated, it must also be emphasized that research on the positive effects of violent games is nascent, but entirely incomplete. It may still be that, on balance, an objective examination of violent games finds that they cause more harm than good, or that they have little impact on human behavior whatsoever. By failing to conduct a thorough examination of violent game effects, we risk losing a potentially valuable tool for promoting human welfare, and risk expending

scientific, social, and financial efforts on promoting a 21st century folk devil.

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