

Is video game violence bad?

What a ridiculous question, says **Christopher J. Ferguson**. Surely there is a way out of this endless cycle of research and moralising?

Though the introduction of video games has co-occurred with substantial reductions in societal violence, not increases, politicians and some scholars often appear to react to tragic news events, such as mass shootings, by linking video games to aggression and criminal violence. In reality, conclusive links remain elusive.

The field has arguably become mired in a cycle of quasi-religious adherence to and correspondingly harsh criticism of beliefs in the 'harm' of violent video games. Within reason, honest arguments could be made both for and against certain types of video game influences on aggression; but the field may be best served by focusing more on the user rather than content and idiosyncratic rather than general effects.

Violent video games and mass shootings are two phenomena that have become invariably linked in much of the public consciousness. Whenever shootings are committed by a young male, whether in the US, such as the 2012 Sandy Hook shooting, or elsewhere, such as the 2011 shooting by Anders Breivik in Norway, they trigger predictable cycles of vehement protests about a downward spiralling media culture of dark aggression.

The notion often raised by politicians and some scholars alike is that, although violence is certainly multidetermined, at least some of these events might simply never have happened if not for the crucial trigger of violent video games. Adam Lanza might have remained just some lonely, disturbed youth, and Anders Breivik a wild-eyed but small-scale Islamophobe. In this way mass shootings and violent video games begin to appear correlated in society... so long as you ignore the cases that don't fit (such as a spate of high-profile gun violence by elderly men in the months following Sandy Hook culminating with the 2013 Serbian shooting by 60-year-old Ljubisa Bogdanovic, who killed 13).

Among some scholars, that video game violence is linked not only with mild acts of aggression but also with societal violence up to and including mass shootings is taken as an absolute fact. Those who question such beliefs or produce failed replications are loudly criticised as not 'true experts', the equivalent of global warming deniers or flat Earthers. Indeed, one scholar recently collectively referred to all scholars who

disagreed with him as 'industry apologists' (Anderson, 2013). Supporters of the video game violence hypothesis have taken to claiming that media violence effects constitute a public health crisis on a par with smoking and lung cancer, or that 30 per cent of societal violence is due to the media.

But a curious thing happened during the era in which video games became both much more popular and more graphic: violent crimes, including among youth, went steadily down cross-nationally, not up (Ferguson, 2013). This trend is most dramatic in the US, given the relatively high violence rates there for an industrialised nation, but the same basic pattern holds elsewhere. How did this subfield of social psychology come to be so mismatched with the data on societal violence, and why do so many scholars in this field insist on such dire warnings about violent games when the data suggest more conservative statements are warranted?

A (quick) overview of the data

Of course, just what the data does or does not say is an area of considerable contention. It is not uncommon to hear some scholars claim that there is consistent evidence for negative effects, although that's easily falsifiable nonsense. But here is a brief, general, overview of what is available.

First, there is a relatively large pool of research on video games and 'aggression' defined rather broadly, perhaps over a hundred such studies (Anderson et al., 2010, although see Ferguson & Kilburn, 2010). Results from these studies have been difficult to interpret. There are certainly some high-quality studies that find evidence that violent game play may increase at least mild aggression or hostility in the short term (e.g. Ballard & Lineberger, 1999; Markey & Markey, 2010; Williams, 2013). Yet there have been high-quality studies that have failed to replicate these findings (Ballard et al., 2012; Charles et al., 2013; Tear & Nielson, 2013).

questions

Has the time come to consider new theoretical models of media effects that focus on user motivations rather than content?

Is it possible to fully separate empirical science from cycles of moral panic that historically target new media?

resources

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Certain methodological flaws have predominated in much of the literature as well. For example, one common error was to assign participants to video games that systematically differed on variables other than just violence. Several recent studies have suggested that properly matching games on issues such as competitiveness (Adachi & Willoughby, 2011) or difficulty (Przybylski et al., 2010) causes violent content-related effects to vanish. Many aggression measures used in such studies were also unstandardised, potentially allowing for researchers to pick outcomes that best supported their hypotheses and ignore those which did not. For instance, using the popular 'noise blast' test of aggression, in which participants give a consenting competitor non-painful bursts of noise when losing a reaction time game, Elson (2011) demonstrated that violent games could be shown to increase, decrease or have no effect on aggression depending upon how aggression was extracted from this measure. Meta-analysis demonstrates that these measures produce

measures such as the noise blast test and filling in the missing letters of words (so that 'kill' is more aggressive than 'kiss') have been specifically rejected in *Brown v. EMA* (2011) as inconsequential by the US Supreme Court, having little to do with public health. The issue with such studies is as media researcher Frank Farley has stated (2012), that they are attempting to answer 'Big V' (societal violence) questions using 'little v' (laboratory aggression studies) data, where such an overextension risks damaging the credibility of the field (see also Hall et al., 2011).

Second, there is a smaller pool of studies that do consider outcomes related to things like youth violence. Although these, too, are mixed, by and large they appear to be less likely to find evidence of 'harm' particularly when other relevant factors are controlled (von Salisch et al., 2011; Ybarra et al., 2008). For instance, on one recent study, my colleague Cheryl Olson and I (Ferguson & Olson, 2014) found that among children with elevated mental health symptoms, violent video

games were not associated with elevated bullying or delinquency. Some studies even fail to control for simple variables such as gender – boys both play more violent games and act more aggressively. In one excellent series of longitudinal studies (Adachi & Willoughby, 2013; Willoughby et al., 2012) the authors found that controlling for other variables reduced the effect size of violent game playing and youth aggression to trivial levels (accounting for just under 0.5 per cent of variance). In their follow-up analysis (Adachi & Willoughby, 2013) they discovered that even this was likely to be due to

competitiveness rather than violent content, suggesting that video game violence may be more similar than

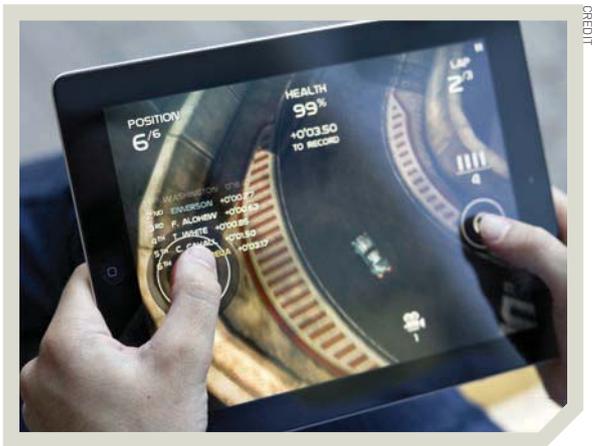
different to other competitive activities sanctioned by society.

Third, during the video game epoch, youth violence has dramatically declined cross-nationally, not risen (Ferguson, 2013). Similarly, nations which are the highest video game-consuming per-capita are amongst the world's least violent (Washington Post, 2012). There is no correlation between media culture and violent crime cross-nationally.

Last, data on mass murderers themselves largely rule out a link with media violence. The 2002 analysis of school shooters by the US Secret Service found no evidence that school shooters consumed high levels of media violence. The most recent and comprehensive analyses of mass homicide perpetrators make no mention of media violence whatsoever (Lankford, in press). With mass shootings, people tend to remember the cases that fit a particular narrative, such as Sandy Hook (although the official investigation report suggested Lanza was more interested in non-violent games than violent), while ignoring those that do not, such as Virginia Tech (where the shooter was found not to be a gamer). When young males are shooters the fact that some played violent video games is treated as a critical revelation, ignoring that almost all males in this age category play some violent games. When shooters are older males (or occasionally females), the issue of games is simply ignored.

The overextension of aggression research into violence

It is reasonable to suggest that cogent and honest arguments could be made both for and against the belief that violent games contribute to mild aggressive acts. But it is problematic when scholars try to overgeneralise their findings to mass homicides or other violent events. Careless scholarly statements became particularly pronounced in the months after Sandy Hook (e.g. KCCI, 2012). The pretence of researchers carefully distinguishing between aggression and



Anything from Pac-Man to Grand Theft Auto has been considered a 'violent video game'

higher effect sizes than better standardised measures of aggression (Ferguson & Kilburn, 2009). Further, common

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violence appeared to have been largely dropped.

In one such recent event, a non-clinician researcher was called to testify at the trial of a man accused of killing a family of five, Christopher Harris. Forensic evidence against the individual was considerable. Thus, the defence attempted to argue that it was not Harris who killed the family, but that he walked in on the 14-year-old son killing his family and Harris had to kill the teen in self-defence (by bludgeoning him 52 times with a tyre iron). The researcher, though not a clinician, testified that the teen had risk factors for aggression and violence, including that he had played violent games such as *Mortal Kombat*. On the stand, the researcher was forced to admit he was not a clinician, had not conducted a proper risk assessment, could provide no evidence violent games are linked to homicides, and even suggested *Pac-Man* could be considered a violent video game (Rushton, 2013). Cases such as this point to the actual danger of an ideological view of media violence, which can foster more misunderstanding than information about violent crime, including in this case the potential to release a mass murderer (Harris was subsequently convicted on all counts) based on faulty information.

The sociology of media violence

Given that it's not possible to use available evidence to make conclusive statements linking violent games to aggression, let alone violence, why do such statements persist? To answer that, I believe it is necessary to take a broader sociological perspective to examine how both society at large and the culture of scholars invested in media research contribute to rigid views.

From the perspective of society at large, we know that most new media go through periods in which society, particularly older adults, tend to worry about the influence of this new media on youth. In the 1950s, comic books were thought to cause delinquency as well as

homosexuality (because it was believed that Batman and Robin were secretly gay). In the 1980s the US Congress held hearings about the alleged harmfulness of musical bands ranging from Cyndi Lauper to Twisted Sister. Other media from various books to Elvis Presley to *Dungeons and Dragons* have similarly been worried over. Some scholars even consider religious books such as the Bible to be violent media with harmful effects on a par with video games (Bushman et al., 2007).

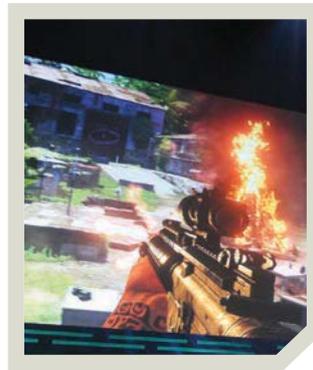
From the perspective of what is called 'moral panic theory' politicians, news professionals and scholars all contribute to these moral panics because it is in their short-term interest to do so. Society calls for research, not to study an issue objectively, but rather to support an *a priori* conclusion. This was most evident in the months after the Sandy Hook shooting, in which members of the US Congress such as Senator Rockefeller and Congressman Wolf called for research while clearly stating what they wished the results of that research to be. Researchers, then, may find it easier to get both grant funding as well as headline attention by arguing that video games present a pressing social problem, even if the data is not conclusive.

Research fields themselves can become ideologically rigid over time as well. In their sociological analysis of the media violence field, Grimes et al. (2008) have argued that this is what has happened to the media violence research field. Put simply, media violence researchers have often begun to act more as advocates for a particular belief, rather than objective scientists. New ideas can be discouraged through harsh reviews of submitted work or *ad hominem* attacks against most

sceptical scholars (it is often implied that sceptical scholars are either shills of industry or 'gamers', typically without providing any evidence of such claims). The willingness of scholars to imply direct links between video games and mass shootings (despite the lack of evidence to support such claims), false claims of consistent research or a scholarly consensus, and continued favourable comparisons to medical research such as smoking and lung cancer (something the Australian Attorney General's 2010 report on video game violence specifically asked scholars to stop doing because it is 'misleading') all constitute evidence of the ideological rigidity of video game research.

Such an atmosphere may be unwittingly supported by professional advocacy groups such as the American Psychological Association (APA). As groups intended to advocate for their profession, they may be incentivised to identify problems their members might fix, as well as getting their own political influence, rather than promoting nuanced science.

Further, once these groups have staked out a rigid position of 'truth by committee' in a policy statement, it may be difficult for them to change course on that policy statement no matter how problematic it proves to be. Problems with and errors in previous policy statements by the APA and other groups have been clearly identified (Ferguson, 2013). Although the APA is reviewing its current policy statements, the committee assigned to do so include a majority of scholars who have taken public anti-media positions in the past (for example, two members signed an amicus brief supporting efforts to regulate violent video games during the *Brown v. EMA*, 2011 case). Thus, the APA has, once again, missed the opportunity to appear non-partisan on this issue. Recently a group of over 230 psychologists and media scholars wrote an open letter to the



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APA requesting that they retire their media policy statements due to a mismatch between such statements and the available data.

Where do we go from here?

The non-ending debates over video game violence (and television... and even book violence: see Coyne et al., 2012) are undoubtedly frustrating for many. They have been going on since the time of the Ancient Greeks, and, so long as they remain wrapped in moralistic and culture war narratives, they are unlikely to end soon. Hoping for a final declaration of 'Video game violence is/is not (pick one) bad for children' is futile. Such a final decision is simply never coming. Even declarative statements such as by the APA (2005) have little real influence, particularly given their historical tendency to be error prone and politicised (see Ferguson, 2013). In part, I argue this is because the question was framed all wrong from the beginning and, in acknowledging this, we may begin to see a way out.

Media psychology has an unfortunate tendency to drift into moralism in which objectionable content is assumed to be bad and that automatic imitation is the road to ruin. In essence, a rather simplified vision of the relationship between media and consumer is promulgated, often encouraged by societal moral panics, in which consumers are considered as passive victims of media. In hammering this question over and over 'Is media violence bad?', the field does little more than travel in circles. Some scholars find effects, to be followed by more sceptical scholars, a replication crisis ensues, but eventually failed replications are ignored due to the moral issues involved. Around and around it goes. Violent (or sexual or otherwise naughty) media has to be bad because... well... it's naughty. The collection and dissemination of data is secondary to a moral crusade. I suspect, to be frank, expressing displeasure over naughty media has a sanctimonious element to it as well,

giving scholars an opportunity to demonstrate their relative worth vis-à-vis a corrupting media industry.

To get out of this cycle we need to re-envision media effects altogether. First, we need to divorce media psychology from its reflexive moralism. And second, we must recognise that individual consumers are active in selecting, processing and shaping their media environment, not merely passive recipients. This approach, the Uses and Gratifications approach (Sherry et al., 2006), posits a media relationship driven by the consumer, not media content. People actively select media in order to obtain a particular desired effect for their mood or behaviour. Under such an approach, media effects would be expected to be subtle and idiosyncratic, rather than profound and predictable. A single source of media could have varied influences on individuals. Consider the Bible: many people seek out the Bible for positive purposes, whether to learn how to love others more, to be inspired, to find comfort in difficult times, to learn how to love God, etc. But others may use the same text to disparage those who are different from themselves, or to find vindication for their anger or hatred, etc. The relationship between media and the consumer is thus more subtle, user-driven and idiosyncratic than often implied in standard social science and communications by scholars to the general public.

This approach does not rule out that the media may influence us, but such influences probably do not rise to the kinds of headline-grabbing or fear-mongering public health crises that have often been favoured in discourses on video game violence. So the answer to the question of 'Are violent video games bad for people?' may be something along the lines of 'Not necessarily, but why the person wanted to play a violent video game in the first place may be important to know!' Most probably play because it is fun and exciting, indeed, playing violent video games appears to be socially normal, particularly for boys (Olson, 2010). But if someone plays to get out rage may tell us

something irrespective of whether the game makes the rage worse or better. Or put another way, someone isolating themselves to (stereotypically) play violent games 18 hours a day in a darkened basement may certainly be indicative a problem, a problem unlikely to be solved if we were to wave a magic wand and make violent games go away.

As a fundamental issue, if we are serious about understanding video games, violent or not, we have to understand them as an integral part of society, not a corrupter of society (Quandt & Kroger, 2013). That has, frankly, appeared to be difficult for a generation of scholars who too often appear unfamiliar with the games and have a tendency toward nannyism that has cost the field significant credibility (Hall et al., 2011). With more sophisticated approaches we may begin to truly understand the interaction between video games, individuals and society.

This high-quality research of the future will be more sensitive to the need to control for other variables in correlational and longitudinal designs and be more cautious in interpreting small effects. Experimental designs that employ carefully matched control video games are also cutting-edge. But more than anything, research that focuses more on user experiences and motivations will likely prove more fruitful than the older effects models. Also, it has probably been unhelpful to consider 'violent video games', a category so broad and poorly defined it includes everything from Pac-Man to Grand Theft Auto, as a unitary and meaningful construct. Research designs that focus more on game structure and narrative may be more valuable than those that stumble, too bluntly, into morality-based content issues such as violence.

Such research is already under way, although it has a tendency to produce fewer headlines than frightening messages about the alleged dangers of video games. If, as scholars, we truly wish to obtain a sophisticated understanding of video game influences on behaviour, we may need to accept that we will have to forgo big conclusions, frightening headlines, and moralistic pronouncements. As with much of human behaviour, things are much more complex than that.



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