

**Statement of Christopher J. Ferguson, Ph.D.**  
**Before the Federal Commission on School Safety**  
**“The Ecology of Schools: Fostering a Culture of Human Flourishing & Developing Character”**  
**Eisenhower Executive Office Building**  
**Washington, DC**

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Thank you for this opportunity to offer testimony before the Federal Commission on School Safety. My name is Dr. Christopher Ferguson and I am a professor of psychology at Stetson University. I am a fellow of the American Psychological Association. I have been studying the impacts of media violence for about 15 years and have published hundreds of peer-reviewed research studies, chapters, and conference presentations on the topic. I am concerned that the Federal Commission on School Safety may be operating under a misimpression about the evidence linking violent media to aggression or violence. Further, I am concerned that indulging a moral panic over entertainment media could distract society and policy makers from actual causes of violence.

**Evidence from Aggression Studies**

First, it is important to understand that most psychological studies, particularly experiments, focus on mild acts of aggression, not violence. Aggressive behaviors include any intentional behavior that inflicts harm on another, however slight. Many such behaviors are considered permissible or even desirable by society. For instance, many of the rigorous debates which take place within politics, perhaps even today at this panel, would undoubtedly qualify as “aggressive” in a study done by media scholars.

Within laboratory studies, typical acts of aggression include behaviors such as putting another person’s hand in a bucket of ice water or giving hot sauce to a person who doesn’t like spicy food. Many studies have participants fill in the missing letters of words such that completing MU\_\_ER as “murder” rather than “mutter” would indicate “aggressive thinking” (what this means, if anything, continues to be debated).

These outcomes can be interesting, but there are also reasons to be suspicious of them. First, they obviously tell us little about the acts of criminal violence society is really concerned about. We are worried about school shootings, gang violence and other horrific acts of bloodshed; not whether youth gangs are roaming the streets with Wasabi sauce to inflict heartburn upon rival gangs. Our own research lab has confirmed these mundane measures of aggression do not predict more serious aggression well, and other analyses confirm there are reasons to be cautious in interpreting these measures (McCarthy & Elson, 2018; Mitchell, 2012.) More worrisome is that most aggression measures are unstandardized, meaning that researchers can pick and choose from among possible results, those that best fit their personal beliefs and alter findings so that it sometimes appears violent media might causes changes in these outcomes (Elson et al., 2014)

Given these problems it is not surprising that evidence for aggression effects has been inconsistent, with some studies finding effects, others not. Publication bias (wherein studies that

find effects are more likely to be published than those that do not) appears to have also caused an overestimation of effects, particularly from experimental studies (Hilgard et al., 2017).

Unfortunately, sometimes the evidence is presented as if it were more consistent than it actually is. This is a phenomenon called “citation bias” wherein scholars simply fail to mention studies that conflict with their personal views. It is considered poor scientific practice (Babor & McGovern, 2008) and appears to correlate with spurious findings (Ferguson, 2015a).

A related issue occurs when scholars run a study, find murky or inconsistent results, yet portray these as more consistent than they actually are. Typically this occurs by ignoring non-significant results or running multiple statistical analyses in order to convert non-significant results to those which are statistically significant, a poor scientific practice called p-hacking. In her work, criminologist Joanne Savage has documented a number of incidents in which research results have not been faithfully reported to policy makers and the general public (Savage, 2004). For example, Savage critiques the widely-reported cross-national study by Dr. Huesmann (Huesmann & Eron, 1986) for failing to use consistent methods across samples, reconfiguring variables when initial results for pure violent TV viewing proved non-significant (such as for boys in the United States) and miscommunicating a mixed bag of effects as if they consistently demonstrated aggression effects. Wiegman, Kuttchreuter and Baarda (1992) using a similar design in the Netherlands, were unable to replicate Dr. Huesmann’s study and found no evidence for long-term effects for television violence on youth aggression.

### **Does the Bible Also Cause Aggression?**

One issue the Commission should consider is that scholar advocates for media violence effects have included the Christian Bible among “violent media” that is thought to cause aggression (Bushman, Ridge, Das, Key, & Busath, 2007). Thus, if this evidence is to be taken at face value, which it should not be, any punitive efforts aimed at entertainment media would also need to be targeted toward religious texts with violence such as the Christian Bible or Hindu Ramayana.

### **Media Violence Research has Experienced Replication Failures and Retractions**

There are also increasing reasons to suspect there may be serious validity issues across aggression studies. The inability to replicate the findings of Dr. Huesmann’s research is not a unique problem for studies in this area. Social science, in general, is experiencing a replication crisis in which many theories previously thought to be absolutely true have been discredited by newer, more rigorous research (Open Science Collaboration, 2015). This also appears to be the case for media violence research, in which newer preregistered studies have often failed to find aggression effects for violent media (e.g. Ferguson et al., 2017; McCarthy et al., 2016) or in which direct or conceptual replications have failed to confirm prior results (Przybylski et al., 2014; Sauer, Drummond & Nova, 2015; Tear & Nielsen, 2013; Zendle et al., 2018). There have also been incidents of study retraction (e.g. Whitaker & Bushman, 2017) and reanalyses that corrected or noted serious errors in original studies (e.g. Ferguson & Donnellan, 2017 reanalysis of Gabbiadini, Riva, Andrighetto, Volpato & Bushman, 2016.) Thus, this field has not provided a stable platform on which policy can be based.

### **Is There Evidence for Brain Desensitization?**

Data from functional magnetic resonance imaging (fMRI) studies also have not supported the belief that violence in media, particular games, cause emotional desensitization. Some early studies suggested that some links between gaming and brain function might exist (e.g. Hummer et al., 2010) although a potential conflict-of-interest funding source, the anti-media pressure group Center for Successful Parenting, reduces confidence in these results. Other studies that found effects were typically small (e.g. Gentile et al., 2016) and may have used analyses prone to false positive results. By contrast, other fMRI studies have failed to replicate these results (e.g. Kuhn et al., 2018; Regenbogen et al., 2010; Szyck et al., 2017). Thus, proposed effects of violent media on desensitization are unconfirmed and difficult to replicate.

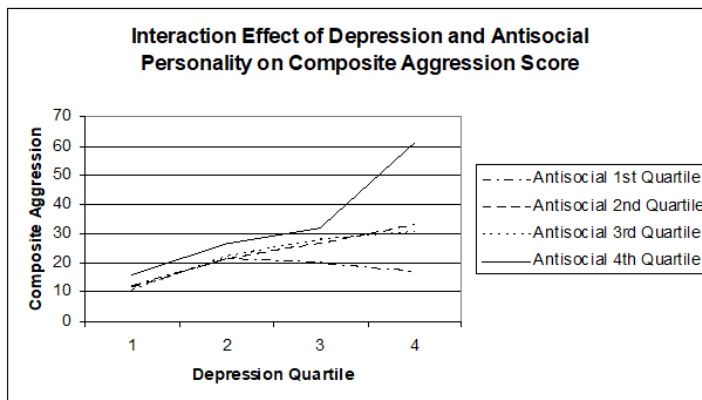
### Evidence from Studies of Violence

Although violence cannot be studied in the laboratory for ethical and legal reasons, correlational and longitudinal studies (the latter of which follow kids over time) can assess self or parent rated serious aggression, or outcomes such as arrests. Here, the evidence is clearer than for laboratory studies of aggression that have been mixed. Data generally do not find evidence that violent media viewing is a risk factor for later criminal violence, bullying, dating violence or arrests (e.g. Breuer et al., 2015; Ferguson, 2011; Lobel et al., 2017; Smith, Ferguson & Beaver, in press., von Salisch et al., 2011).

Some studies may find a “statistically significant” effect, but one that is so small it is likely noise rather than true effects (e.g. Anderson et al., 2008), what some psychologists call the “crud factor” of psychological research (Meehl, 1991). Generally, such studies are miscommunicated to the public who may not fully understand that a study can reach “statistical significance” by mistake, having no real evidence for practically valuable effects.

As indicated in the example below from Ferguson (2011), evidence suggests that issues such as mental health or family environment are risk factors for youth violence. Policy attention should focus on these issues rather than expending resources unnecessarily on entertainment media.

**Figure 1:** Serious aggression most common among youth with mixed antisocial traits and depression. Note entertainment violence was not a risk factor for youth violence:



### Meta-Analyses

Meta-analyses are studies that combine many smaller studies into a composite to examine for trends. Unfortunately, meta-analyses of media violence effects have themselves not reached

consensus on whether effects are real or not (e.g. Anderson et al., 2010; Bushman & Huesmann, 2006; Ferguson, 2015a; Furuya-Kanamori & Doi, 2016; Hilgard et al., 2017; Paik & Comstock, 1994; Sherry, 2007). In some cases (e.g. Anderson et al., 2010) meta-analyses have been reanalyzed as potentially having overestimated effects (Hilgard et al., 2017).

Several things do appear to be clear from meta-analyses:

1. Effects range somewhere between very small to nonexistent.
2. Study results are influenced by systematic methodological flaws, such as used of poorly validated aggression measures, researcher expectancy effects and publication bias. These flaws tend to result in outcomes that are biased toward exaggerating effects.
3. The closer outcome measures approximate actual violence, the closer effects come to zero.
4. Controlling for theoretically relevant third variables (e.g. gender, personality, family environment, genetics) brings effect sizes close to zero. This suggests that violent media has little unique risk for negative outcomes.
5. Research evidence for effects has been getting weaker, not stronger, particularly over the prior 10 years. This is likely due to increased rigor in the field.

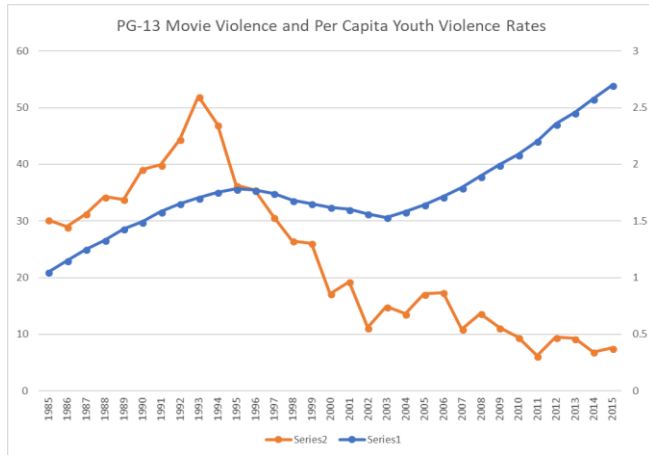
### **Linking Media Violence to Societal Violence**

It can be instrumental to examine links between society's consumption of entertainment media violence and actual violence in society. In previous decades, it was common for media scholars to claim direct associations, thus looking at these associations can be illustrative with the caveat that they are correlational in nature.

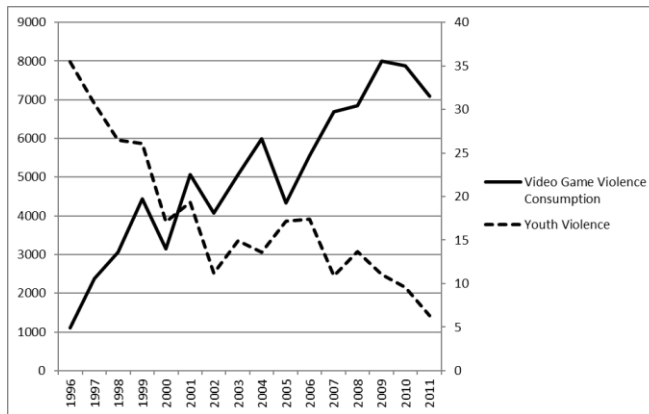
It is worth remembering that, according to National Crime Victimization Survey data, youth assaults are down over 80% from 25 years prior. Indeed, as Stephen Pinker (2011) compiles from multiple historical data sources, we are likely living in the least violent epoch of human history.

As can be seen from figures 2 and 3, both violence in PG-13 movies commonly enjoyed among youth and in video games is associated with declines in youth violence, not increases. This is true for any age category of youth. The notion that youth today are becoming more violent at earlier ages is entirely a myth:

**Figure 2:** Violence in PG-13 Movies and Youth Violence:



**Figure 2: Violence in Video Games and Youth Violence:**



Although this basic data is correlational, time-series analyses suggests that the release of popular violent movies (Dahl & Dellavigna, 2009) and video games (Cunningham et al., 2016; Markey, Markey & French, 2015) are associated with immediate declines in societal violence. This is a classic case of “if it ain’t broke, don’t fix it.” Policy tampering with a system already moving in a positive direction is more likely to cause unintended harms than benefits.

### Cross-National Comparisons

Cross-national data likewise does not support a link between violent media consumption and gun violence or assaults (Markey & Ferguson, 2017). In fact, analyses suggest countries which consume higher rates of violent media tend to be among the safest in the world even after controlling for other possible variables.

### Do Mass Shooters Consume More Violent Media?

One other piece of data worth considering is whether mass homicide perpetrators are particularly avid consumers of violent media. It’s worth noting that public perceptions are often susceptible to two phenomena. The first is *confirmation bias* wherein society often discusses violent media when a perpetrator is a younger male and ignores the issue when the shooter is an older male (such as the 2017 Las Vegas shooting perpetrated by a 64-year old male) or, more rarely a woman (such as the 2018 shooting at a YouTube office.) This allows the illusion of a

correlation to persist when one does not exist as the general public simply ignores cases that don't fit the belief.

The second issue is the *Follow-through Failure Effect*, wherein news media fail to inform the public of the resolution of a case. The public and policy makers may speculate over a perpetrator's media habits before anything is officially known (as has been the case for recent shootings). Later, when an official investigation report is released that downplays media effects, this gets little news media coverage. As such the public believes, falsely, that the anecdotal case is still "evidence" for media effects. The 2012 Sandy Hook shooting is an example of this, wherein months of speculation about violent games percolated before the official investigation report revealed the shooter was mainly obsessed with the non-violent game *Dance, Dance Revolution* (State's Attorney for the Judicial District of Danbury, 2013).

However, both a report by the by the US Secret Service and US Department of Education (2002) and more recent analyses (Markey & Ferguson, 2017) both found that only about 20% of school shooters had an interest in violent video games compared to 70% of average high school students (see Figure 4). In other words, school shooters are about 3-times *less* interested in violent video games than is normative for males of their age. Criminologists who study mass homicide have likewise referred to link between mass homicides and violent media as a "myth" (Fox & DeLateur, 2014).

**Figure 4:**



### **Why Do False Beliefs about Media Violence Keep Coming Up?**

From the existing data, we can see it is very difficult to substantiate links between violent media and societal violence. Why, then, does the issue continue to be brought up after mass homicides or other acts of violence?

One theory called *Moral Panic Theory* speaks to this issue. Briefly, moral panic theory suggests that society tends to search for scapegoats on which to blame real or perceived social problems. Given that older adults hold more financial and political power (given that they vote, buy newspapers and control grant funding), society tends to focus blame on new technology or media that older adults don't like, understand, or use. Older adults, in effect, become the "audience" for a moral panic, with policy makers, news media and scholars performing for that audience.

Related to this, there is a clear historical pattern of moral panic regarding media stretching back to the ancient Greeks (some of whom were concerned about Greek plays, and

who are recorded complaining about the declining moral values of the newest generation of youth, just as older adults still do today.) During the 20<sup>th</sup> century, moral panics focused on everything from comic books (thought to cause delinquency and homosexuality), Elvis Presley, 80s rock music (including Cyndi Lauper, Prince and Tom Petty), Harry Potter, Dungeons and Dragons and, of course, video games and movies. Usually, the moral panics look foolish in hindsight, but that can be difficult for people to realize in the moment.

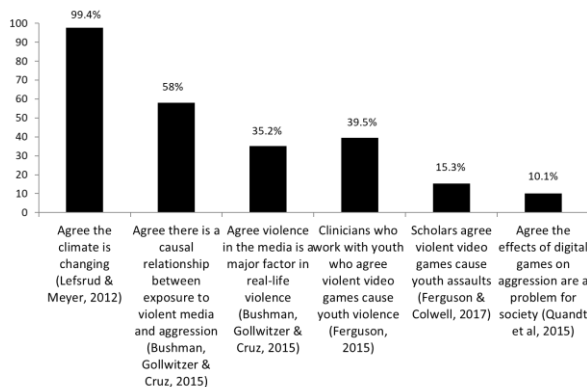
Supporting this, research evidence suggests that older age and low experience with new media are strong predictors of negative attitudes toward that new media. For instance, older adults have more hostile attitudes toward video games than younger adults. This is true for the general population (Przybylski, 2014), among clinicians (Ferguson, 2015b) and among scholars who study video games (Ferguson & Colwell, 2017.)

Interestingly, along with age, the other strong predictor of negative attitudes toward new media is negative attitudes toward youth themselves. Although attempts to regulate new media are often couched in language regarding “protecting” youth, this suggests the motivation may be more complicated.

### Is There a Consensus Among Scholars on Media Violence Effects?

It is not uncommon to hear some scholars who advocate for media violence effects to claim they have a “consensus” among scientists in this area. However, research evidence does not support this claim. In Figure 5, data on the consensus among scholars who study climate change (about 99.4%) is compared to agreement among scholars who study media effects. These surveys were conducted by scholars on both sides of the debate so it is difficult to argue there is any bias in these data. As can be seen, across all these surveys of scientists in this area, significant disagreements exist even for mild aggression (hot sauce, filling in the missing letters of words, etc.), but the view that links between media violence and societal violence exist is, in fact a minority view.

**Figure 5:** Agreement Among Scholars on Global Warming and Media Violence Effects



### Problems with the APA, AAP Statements

Beliefs about media violence effects are also undoubtedly reinforced by professional guild organizations such as the American Psychological Association (APA; full disclosure: I am a fellow of the APA and sit on their Council of Representatives at the time of this writing) and

American Academy of Pediatrics (AAP). It is important to note that these are not objective scientific bodies but (legal status aside) function somewhere between guilds that promote the interests of their members and as business marketing a product (in the case of the APA, psychological science or even more specifically the peer-reviewed journals that they publish and for which they charge subscriptions.) The revenues from the publishing aspect of the APA and AAP given them an incentive to publish research that garners newspaper headlines, then defend that research against criticism by declaring it beyond debate. These organizations also have an incentive to create problems for their members to fix, promoting grant funding for their members.

The APA's 2015 resolution on video game violence is illustrative. Although this resolution claimed that research could link video games to minor acts of aggression, to their credit, the APA acknowledged that evidence could not link games to societal violence. However, even the claims about aggression have proven controversial. When the task force for that resolution was announced in 2013, over 230 scholars wrote an open letter to the APA asking them not to release these types of policy statements (Consortium of Scholars, 2013). In 2017, the Media Psychology and Technology Division of the APA released their own policy statement correctly noting that the research evidence for even mild aggression is inconsistent, and asking policy makers to stop linking violent media to societal evidence given research noting such links do not exist (News Media, Public Education and Public Policy Committee, 2017)

The AAP policy statements have likewise been widely known for serious errors, citation bias (failing to cite studies conflicting with the policy statements), and repetitions of urban legends. For instance, a 2000 policy statement by the AAP claimed that 3500 studies of media violence existed at that time with only 18 not finding effects. These figures appear to be entirely apocryphal and were debunked as off by a factor of 10 (Freedman, 2002) in terms of the sheer number (Freedman, 2002 reports about 200 studies existing at that time) as well as misreporting their consistency. In the same policy statement, the AAP claimed that media effects were similar to those for smoking on lung cancer, a claim that has similarly been debunked as due to a statistical error (Block & Crain, 2007).

Related to social media effects, which I believe another panel will discuss, the AAP once (2011) claimed that "Facebook Depression" existed, mainly by citing news media reports of psychological studies. As a result, one of the researchers whose work had been covered in those news media reports publicly noted her work had been misrepresented by news media and could not, in fact, be used to support the AAP's claims (Davila, 2011.) Frankly I would fail an undergraduate student in my courses for referencing news reports rather than original sources. That a professional guild would produce such sloppy work goes to their credibility as a source of scientific information.

It is also worth noting that most government reviews have been more skeptical of media effects. These include reviews by the governments of the UK, Australia, Sweden, as well as the US Supreme Court in the *Brown v EMA* (2011) case which considered video game regulation and, in a 7-2 decision, ruled it unconstitutional and unnecessary. Even the anti-media advocacy group Common Sense Media (2013) acknowledged that research evidence could not substantiate links between media violence and societal violence despite it would have clearly been in their interest to say otherwise. We need to ask our professional guilds for more honest and accurate



overviews of media violence and other media research even if the resulting summaries don't provide easy sound bites that are of benefit to the organization or profession.

### **The Risks of Focusing on the Wrong Thing**

My concerns about the Commission's focus on violent media are not merely academic. There are real risks that focusing on a red herring distracts society's attention and resources from other, more pressing issues.

First it is worth noting that the US Supreme Court's decision in *Brown v EMA* (2011) provides broad constitutional protections to what we have taken to calling "violent media." I am not a constitutional lawyer, but my lay-reading of the decision is that it would prohibit any government regulation or taxation of media, or government involvement in ratings systems. Thus, it is unclear what the government could do under the First Amendment even if there were a real issue, which there does not appear to be.

More at hand is that moral panics over media often serve to distract society from more pressing, but less morally grandstanding issues such as poverty, mental health reform and educational disparities. Progress on these issues will be hard and require sustained attention. We can't get distracted by culture wars over media and the agendas of moral crusaders.

Speaking as a media violence researcher, I want to be clear. We do not need your research funding. We do not need your support or attention. The notion that violent video games or violent movies are a cause of horrific acts of violence doesn't deserve anymore newspaper headlines. Aside from esoteric debates on mild aggression (e.g., the administration of hot sauce) only a very vocal minority of scholars still believe violent video games or movies are a cause of school shootings or horrific acts of violence. We cannot fix the problem of societal violence with our research field. The research we have now is abundantly clear: media violence does not play a role, not even a small role, in societal violence. If anything, messing with the relationship between media and society may actually *harm* society rather than help distracting society from real issues. I ask the Commission to help put this issue to rest once-and-for-all by focusing its attention on real issues that influence violence in schools and society.

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