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Violent Video Games: Dogma, Fear, and Pseudoscience

Video games are at the center of a modern media-based moral panic. Too often, social scientists have fueled the flames and eschewed good scientific practices.

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School shootings at high schools and universities are rare but shocking events. Across the last decade or so we have come to understand a little about the psychology of the young men who carry out such horrific crimes. Most typically they are emotionally disturbed, often depressed, feel socially isolated and alienated from society, and are filled with rage and hatred. It is natural for society to want easy answers about how such a phenomenon can be controlled. Social scientists want to give those answers. Unfortunately, the human inclination to believe we can predict the unpredictable and control the uncontrollable often leads us to mask the language of fear and irrationality in the language of science.

In the case of school shootings and youth violence in general, some scientists have answered the public demand for a culprit. The alleged corrupting influence of violent video games has been identified by some as the root cause. Ignoring the youth-violence data, ignoring inconsistent data from multiple studies, even ignoring contradictory data from their own studies, some social scientists have presented the research on violent games as strong, consistent, and unequivocal. In truth, it is none of these things. There is a huge gap between the effects of violent video games as presented by some social scientists and the actual scientific data on the effects of violent video games. This gap between social science and reality has led to a moral panic regarding the effects of violent video games on youths.

Debates about the corrupting influence of media are nothing new. The history of media-based panics includes concerns about Greek plays (Aristotle liked them; Plato didn't), translations of the Bible (William Tyndale and others were harassed or executed for translating the Bible into English or other non-Latin languages), novels, jazz, rock and roll, rap, movies, comic books, Dungeons and Dragons role-playing games, Harry Potter, television, and now video games. Each of these panics hinged upon belief in a highly suggestible youth populace unable to distinguish fiction from reality, something children can actually do beginning by approximately age three (Woolley and van Reet 2006). In most of these cases we can look back and see how foolish these fears were. We'd love it if our children would read more, take an interest in classic Greek plays, or get more into the music that we older folks enjoy. Society's elders repeat the same cycle from generation to generation, becoming suspicious of new art forms that they don't use and have no use for. It appears to be part of human nature to disparage youth culture whenever it emerges, perhaps as a means of maintaining dominance in the guise of "protecting children." Activists such as former lawyer Jack Thompson and David Grossman might be forgiven for inflaming fears with exaggerated claims. Grossman, for instance, has promulgated the false notion that the military uses video games to desensitize soldiers to killing (they do use simulators for visual scanning and reaction time and vehicle training, but they seem more effective in reducing accidental shootings than anything else). These individuals have an activist agenda similar to many pressure groups, and we expect them to present only a single side of an issue to drive their point home far beyond the bounds of objectivity.

We expect more from scientists, for whom objectivity and skepticism are essential tools. Certainly science takes wrong turns, but objectivity, skepticism, and the willingness to challenge old paradigms allows science to self-correct over time. To be clear, the hypothesis that violent video games increase aggression and even violent criminal behavior is a reasonable empirical hypothesis. Given that data in social science tend to be murky since they continue to rely on rather flawed null-hypothesis significance testing, there also is nothing unacceptable in taking either position in an ongoing debate such as that on media violence or video-game violence specifically.

However, individuals with PhDs cease functioning as scientists when they make active efforts to stifle debate, ignore opposing research, ignore real-world data, fail to inform the public about the limits of social-science research, and make tenuous connections between survey questionnaires or consensual "aggression" games in the laboratory and mass-shooting incidents or the phenomenon of youth violence more generally in real life.

Scientists' Statements

When most people read about studies suggesting something along the lines of "eating plums increases aggression," they likely picture children or adults hitting, kicking, or screaming at one another after consuming plums. However, most aggression studies involve either individuals filling out questionnaires or engaging in mutually consensual game-like activities. Aggression measures used in laboratory studies of media violence have included everything from popping balloons, rating people's job performance (after those people have insulted participants), filling in the missing letters of words, and playing a consenting reaction-time game in which players can deliver non-painful noise bursts (like the white noise on a television set) to their opponent. Whether these "aggression" measures are valid predictors of real-world violence remains an open debate.

It must also be noted that aggression and violence are not synonymous, and not all aggression is bad. Being aggressive in moderate amounts is likely adaptive. However, the limits of aggression measures used in studies of video games have not stopped some scholars from making claims, explicit or implicit, that their research can be generalized to extremely violent behaviors on the part of children or adults. Video game researchers commonly begin their research articles by raising the fears of mass school-shootings (Anderson and Dill 2000; Anderson 2004) or even the September 11 terrorist attacks (Bushman and Anderson 2002). There may be valid scholarly reasons for discussing such incidents, of course, although objective science would dictate the necessity of making clear the gulf between research procedures currently employed and these extreme events.

A more recent report (Anderson et al. 2008, funded in part by the activist group National Institute of Media and the Family) suggests that the weak correlations between playing video games and self-reported aggression can be generalized to youth violence. The authors fail to make clear that they didn't measure youth violence and also fail to note that youth violence has been experiencing a precipitous fifteen-year decline during the same period in which video games have soared in popularity. Although the data do not highlight violent-game sales as opposed to nonviolent-game sales, some authors have found at least mild violence to be common even in E-rated games (E is for "everyone," the lowest rating analogous to a G-rating in movies; Thompson and Haninger, 2001). Thus it seems safe to assume that average violent content in video games has either remained constant or increased during this time period.

[Insert Figure 1 about here]

A common element of these articles is the issue of "citation bias," the failure to honestly report on research results that don't support the authors' own hypothesis. As a result the study falsely communicates to the scientific community a greater certitude and unanimity of results than actually exists. The papers cited above all fail to note the considerable number of studies that do not find a relationship between violent video games and increased aggression. For instance, Bushman and Anderson (2002, p. 1679) claim, "Despite the recency of this genre and the relatively small size of the research literature, there is sufficient research to conclude that violent video-game exposure can cause increases in aggressive behavior and that repeated exposure to violent video games is linked to serious forms of aggression and violence," while failing to note that other scholars would disagree with this position (e.g. Guantlett 1995; Olson 2004; van Schie and Weigman 1997).

Of greater concern is when authors ignore their own results to drive home their *a priori* hypothesis. Anderson and Dill (2000) used four separate aggression measures in their laboratory study and found weak significance for only one of the four (had they employed a proper statistical correction for multiple analyses, even the fourth analyses would have been non-significant). Nonetheless they ignore the three non-significant results and focus on the single barely significant result to promote a hard causal view of violent game effects on aggression. Although I suspect that these authors are well-meaning and genuinely concerned about children, their statements do not always reflect a careful objective analysis of the data. In fairness, the authors do provide arguments for why one aggression measure should be highlighted over others. For instance, they suggest that two of the measures may have been compromised by their own instructions to participants to focus on the other two measures. Of the last two, they argue that provoked aggression (occurring after trials in which the participant loses) may be related to aggressive behavior more than unprovoked aggression (occurring after win trials). However, post-hoc reasoning could be used to support the precedence of any outcome over others, and other video game researchers have, indeed, made exactly the opposite argument, with unprovoked aggression taking precedence (Bushman, Baumeister, and Philips, 2001).

I believe that these authors have ceased functioning as scientists and have begun functioning as activists, becoming their own pressure group. Indeed, in at least one article, the authors appear to actively advocate censorship of academic critics and to encourage psychologists to make more extreme statements in the popular press about violent video-game effects (Gentile, Seleem, and Anderson 2007).

Although some studies certainly do appear to indicate at least weak correlations between violent games and increased aggression, many other studies, including several of my own (e.g. Ferguson et al. 2008; Ferguson and Rueda in press) as well as research by John Colwell, Cheryl Olson, Lawrence Kutner, Grant DeVilly, and others, find no such effect. In a recent meta-analysis of video-game and other media-violence studies published in the *Journal of Pediatrics*, my colleague John Kilburn and I found that these studies effectively add up to zero (Ferguson and Kilburn 2009). More worrisome is that many studies employ unstandardized measures of aggression that may allow researchers to pick from among multiple outcomes, potentially picking outcomes that best support their hypotheses. Such measures were found to produce higher effects than better standardized and well validated aggression measures. The closer that measures came to actual violent behavior, the closer to zero the effects became.

Video-Game Violence, Smoking, and Lung Cancer

One argument some scholars have used to promote the view that video-game violence is a looming problem is the argument that media-violence effects are similar in magnitude to those of smoking on lung cancer (Bushman and Anderson 2001). The effects of smoking on lung cancer rates are well known to be among the strongest in medical science. Among male smokers, the risks of lung cancer are twenty-three times stronger than for non-smokers. Lung cancer among non-smokers is very rare (and almost always related to clear genetic risk). Instead of using the standard "relative risk" effect size estimate for smoking and lung cancer used in medical research Bushman and Anderson (2001) tried to convert it to a correlational coefficient. The resulting coefficient is surprisingly low and, indeed, not much larger than the statistic they report for media-violence effects. Such a lofty claim for media-violence effects should have invited skepticism, particularly with violent crime rates plummeting to 1960s levels while media violence is soaring in popularity. These same statistics, in fact, have been used to support the importance of ESP research (Bem and Honorton 1994) among other psychological (or apparently parapsychological) findings. Unfortunately, skepticism was in short supply, and this comparison has been repeated multiple times, including in testimony to the U.S. Congress.

The statistics upon which this comparison is based are flawed, however. First, the authors chose a statistic for the effect size of media violence that most meta-analytic reviews (indeed including the authors' very own meta-analysis in the same paper) find to be too high. It also turns out that it's not so easy to convert medical "relative risks" into correlational coefficients. In a paper due to come out in the American Psychological Association journal *Review of General Psychology* in June **[CF: Has this paper come out yet? If so, references to it in this paper need to be updated—JL]**, I note that the statistics on which these calculations are based are deeply flawed and tend to grossly underestimate medical effects. I'm not the first to point this out—Block and Crain (2007) pointed out much the same issue—but I suspect our cautionary voices have largely been ignored.

What about Those School Shooters?

It is certainly true that most (although not all) school shooters played violent video games. So do most other boys and young men. Concluding that a school shooter likely played violent video games may seem prescient, but it is not. It is about as predictive as suggesting that they have probably worn sneakers at some time in the past, are able to grow facial hair, have testicles, or anything else that is fairly ubiquitous among males. Michael Moore, in his famous documentary *Bowling for Columbine*, noted that the Columbine shooters were avid bowlers. Perhaps a propensity for bowling as a hobby is correlated with aggression? As such, playing video games is an illusory correlation as far as aggression is concerned and explains nothing. This was highlighted best during the 2007 Virginia Tech shooting. Soon after the shooting, pundits such as Jack Thompson and Phillip McGraw ("Dr. Phil") speculated that the shooter must have been a frequent violent gamer (McGraw 2007; Thompson 2007). This was an easy guess given that most young males play violent games, however the official investigation revealed quite the opposite: the shooter, Seung-Hui Cho, did not play violent games at all.

This is a cautionary note for the research on violent video games. Any two variables that are gender-based are likely to display some correlation. Males play more violent games than females and are also more aggressive on average. Thus we expect to see a correlation between violent games and aggression . . . but also between aggression and growing beards, participation in sports, wearing pants rather than skirts, and a preference for dating women—in short, anything else that is a male-dominated activity. Sadly, many studies of video games miss this important issue and fail to control for something as obvious as gender. Such correlations should be interpreted with caution. Since school shooters don't often volunteer for participation in psychology research, little is honestly known about their media consumption relative to normal males. However, a report by the U.S. Secret Service (2002) suggested that violent-media consumption, including video-game violence, is surprisingly low, not high, among this population.

Delineating Science and Pseudoscience

The main arguments raised in favor of the correlation between video-game violence and aggression include the argument that the research is consistent and strong; that aggression measures used in the research predict real-life aggression and violence; and even that the effects of video-game and other media violence approach the strength and consistency of the research found linking smoking with lung cancer. Such comments should invite careful inquiry and even normal scientific skepticism. Indeed a careful examination of the evidence finds these claims to be false. Aggression measures used in much of the research do not seem to predict real life behavior very well. Indeed most of these measures are not used clinically to predict violence, and even with well-validated clinical measures violence prediction remains dicey. The research is inconsistent at best, weak where relationships are concerned, and nowhere near the strength, consistency, or quality of the research on the link between smoking and lung cancer (which benefits from the unambiguous outcome of death as opposed to proxy aggression measures of questionable quality). At the extreme, the pseudoscience of media-violence effects has resulted in outlandish claims even from professional organizations. For instance the American Academy of Pediatrics (AAP) claimed that "Since the 1950s more than 3,500 research studies in the

United States and around the world using many investigative methods have examined whether there is an association between exposure to media violence and subsequent violent behavior. All but 18 have shown a positive correlation between media exposure and violent behavior" (Cook 2000). This statement is a blatant misrepresentation of the facts. Not a single meta-analysis of media violence has located more than a couple hundred studies, including all forms of media violence, not just video games, and these have included many unpublished studies of questionable quality. Freedman found about 200 studies in his 2002 review (including non peer-reviewed studies), which were about evenly divided between those that did and those that did not find effects. That the AAP could raise such a fallacious claim unchallenged is remarkable.

The investigation of video-game violence effects is not, in and of itself, pseudoscientific; the hypothesis is legitimate. My concern is the manner in which some scientists have tried to protect rather than objectively study this hypothesis, oftentimes distorting or miscommunicating the facts to the scientific community and the public at large. In some cases this may be driven by activist/lobbying groups such as the National Institute of the Media and Family (NIMF), which has funded several anti-game studies. In one recent study supported by the NIMF (Gentile 2009) the author suggested that approximately 8 percent of youths exhibited "pathological" patterns of video-game play (commonly referred to as video-game "addiction"). The author (the director of research for the NIMF) stated multiple times (including in the title) that this sample was nationally representative, and he claimed to have used a "stratified random sample." A close read reveals that the "stratified random sample" is in fact taken from a larger convenience sample of youth who self-selected into the sample online. Youth who are online so often as to volunteer for online surveys may, naturally, be particularly prone to pathological Internet use and certainly don't represent "average" youth. Taking a smaller random sample from a larger convenience sample does not convert a convenience sample into a random sample. Even if the study had no other problems, this 8 percent figure taken from a convenience sample of potentially heavy Internet users is meaningless and uninterpretable. However, this issue was not well communicated to either the scientific community or general public. Activist/lobbying groups regularly inflate statistics to sell books or speaking engagements and thus may have a profit motive for inflating fears about a particular issue. That the scientific community outsources science to such organizations is a worrying trend.

That said, I acknowledge that a rational, objective argument could be made for violent video-game effects. I would likely disagree with many of those conclusions based on the evidence, but debate is normal and healthy in the social sciences; I would respect an objective and honest appraisal of the evidence that differed from my own. Such appraisals already do exist. As one example, Patrick Markey (e.g., in Markey and Scherer 2009) has been examining whether certain individuals with preexisting mental health problems may be prone to violent game effects, even if most individuals are not (violent video games may thus be like peanut butter—a harmless diversion for the vast majority but potentially a problem for a tiny minority). Although I may disagree with some of Markey's conclusions, he exhibits caution, objectivity, and balance in his reviews of current literature and in his measured conclusions from his own research. Although we may differ, I respect the views of such scientists who are careful to note the limitations of the science and encourage scientific discourse rather than attempting to discontinue debate in favor of their own view. Too often such scientists are drowned out by the voices of others who have promoted a radical ideological view, indulged in "citation bias" (citing only research favorable to one's own view), and implied any views differing from their own are the product of the video game industry rather than legitimate scientists (I am, in fact, unaware that the video game industry funds or supports any behavioral research on aggression, and I, for one, certainly have no affiliation with them).

The Pseudoscience of Video Games

In summary, some scholars of video games, and perhaps the psychological community in a larger respect, have ignored data that conflicts with the alarmist view of violent game effects and have made extreme, hyperbolic statements that are not supported by data or are based only on faulty statistics. Some scholars have invalidly also made comparisons in their work between violent video games and school shootings or other violent incidents. How has this irresponsibility been allowed to go on for so long?

One possibility is simply that social scientists are prone to the same human foibles as everyone else, including making emotional rather than objective evaluations of their own research and that of others. It is very easy for social science to blur into activism, politics, and social engineering. Media violence and video games is certainly not the first example and surely won't be the last. Even in regard to the cyclical media-violence panics, the involvement of social science in promoting unrealistic fears across the nineteenth and twentieth centuries is well documented. Grimes, Anderson, and Bergen (2008) have an excellent book that discusses how politics and scientific dogma (e.g., the tabula rasa belief that we are *forced* to imitate anything that we see in the media) have done much to damage the media-violence field as an objective science. Indeed, they express concern that psychological science has become an industry that produces factoids on demand to an agitated populace. This concern is echoed by Gauntlett (1995).

The murkiness of social science research makes it very easy for objectivity to be lost. The "standards of evidence" in psychological science are much lower than for the hard sciences. This is a simple reality that psychologists must bear but that many choose to deny, hurt by the implication that we are something lesser than the hard scientists. The weaknesses of social science allow long-outdated theories to continue to haunt the graveyards of psychology far past their prime. Witness the immortality of psychoanalysis or tabula rasa, for instance. Unfortunately, the willingness of psychologists to stray into activism and social engineering confirms the very concerns that hard scientists have regarding our discipline.

Summations of the research on video-game violence find that they have either no effect or only very weak effects on aggression (defined very broadly, ranging from approximately 0–4 percent; Anderson 2004; Ferguson 2007; Ferguson and Kilburn, 2009; Mitrofan, Paul, and Spencer, 2009; Sherry 2007). Data from youth-violence statistics reveal that the explosion in popularity of video games, including those with violence, has clearly not produced a youth-violence epidemic, as youth violence has declined precipitously during the same period (I am careful to note that this should not be taken to imply that video games have *caused* this reduction in youth violence; this data is merely correlational). It has become fashionable for some media-violence scholars to ignore this youth-violence data. It doesn't matter, the argument goes, because violence is caused by so many different factors that video games could still be having a negative effect masked by numerous other, yet unexplained, positive factors. This argument fails on two counts. First, it is essentially a concession to what critics have been arguing: that video-game violence effects are so trivial that they are unimportant in the grand scheme of things. If this is true, then social scientists should be taking greater care to ease the fears of parents and policymakers, not inducing greater amounts of unnecessary fear. Second and perhaps more important, this argument brings the hypothesis about media violence and video-game violence to an unfalsifiable level. In other words, the theory need never actually match with real-life data, and such data can be simply ignored or wished away. It is forgotten that many of the same researchers happily referred to violent-crime data while it was *increasing* (e.g., Bushman and Anderson 2001; Centerwall 1989); now when the data is unfavorable, it is unimportant. Yet an unfalsifiable theory, one immune to data, is a nonscientific theory. Could we imagine worrying about smoking if lung cancer rates *declined* among smokers? Or would the scientific community be as concerned about global warming if it were snowing in Florida in July? **[CF: I'm not sure this is the best analogy. Aren't strange weather phenomena actually symptoms of global warming? JL]** This is how far the media-violence hypothesis has fallen. A perfectly reasonable empirical question has largely been disproven by real-world data. The only place for it to go is pseudoscience. □

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Figure 1
Youth Violence and Video Game Sales Data.