What Is a Good Skeptic to Do? The Case for Skepticism in the Media Violence Discussion

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Abstract

The discussion about violent video games tends to engender extreme positions, each of which are deserving of deep skepticism. Ferguson's (2015, this issue) claim that humans can do something repeatedly with no effect on them should be examined carefully, especially as it violates most established psychological and learning theories. In this commentary, we examine three aspects of Ferguson's claim. First, it is a typical rhetorical trick to sow doubt, but it is valuable to examine the doubting claims. Second, it is good rhetoric to direct attention in only one direction, but it is valuable to examine that direction within its broader outlook. Third, it is good rhetoric to imply bias on the part of one position, but it is valuable to examine the potential biases on all sides. Good science definitely requires skeptics. The problem with the violent video game debate is perhaps that we have not been skeptical enough.

Extremes scare me. I tend to question both claims that something is monolithically devastating or that it can have no impact. This concern grows when the claim does not fit well with existing theory and data. Unfortunately, the violent video game debate seems to draw more of these types of extremes. Sometimes they are easy to spot, such as the now disbarred lawyer Jack Thompson who routinely appeared on television within hours of a school shooting claiming (long before any evidence had been collected) that violent video games were to blame. Sometimes, however, they are carefully crafted with gentle language to guide the reader past incongruities.

Sowing Doubt

One way to get a reader to miss incongruities is to cast doubt on multiple aspects of a literature. This was the approach of the cigarette industry, as admitted in an internal memo describing their goal of creating the appearance of a lack of consensus: "Doubt is our product since it is the best means of competing with the 'body of fact' that exists in the mind of the general public. It is also the means of establishing a controversy" (http://tobaccodocuments.org /landman/332506.html).

This approach to sowing doubt can be seen throughout Ferguson's article (2015, this issue), in which doubt is cast on almost every part of the scientific process, including variable selection, measurement, reporting, and even the integrity of researchers. I do not mean to suggest that we should not be skeptical, as this is of paramount importance in science. Instead, I mean to suggest that we should be skeptical even of the skeptics. Extraordinary claims require extraordinary evidence. Space is too limited here to respond to every point made in the article, but a few examples should illustrate the concern.

A claim has been made that violent game experiments either have not matched games across multiple dimensions (such as how frustrating, exciting, or fun they were) or did not pretest them. Sometimes this argument focuses on some specific aspect alleged to matter more than violent content, such as frustration or competition (e.g., Adachi & Willoughby, 2011; Przybylski, Deci, Rigby, & Ryan, 2014). There are at least three reasons to

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question these claims. First, most experiments conducted since 2000 have matched games on multiple dimensions. I know that Craig Anderson (whom I use as an example because he has published the greatest number of violent game studies) routinely matches violent and nonviolent games on multiple dimensions (frustration, excitement, fun, etc.). Some studies pretested games to match them. Because games change rapidly, and sometimes the perceptions of pilot study participants are different from later participants, researchers also have used statistical matching from the exact participants in the main experiments. This looks like appropriate science to me. Second, the claim that the effect is due to frustration or competition and not violent content ignores many studies that actually did control for these types of aspects. For example, Anderson and Carnagey (2009) used equally competitive sports games that differed in the amount of violence, finding that the violent content made a difference over and above the competitive aspects. Similarly, we knew 50 years ago that frustration alone could increase aggression (Dollard, Doob, Miller, Mower, & Sears, 1959), which is why many experimental studies have controlled for frustration. Third, these characteristics are not mutually exclusive. Violent content tends to co-occur with several of other aspects. In fact, violent content may have some of its effects precisely because violent games are frustrating, arousing, competitive, fast-paced, and so on. Teasing apart which of these aspects of violent games have the largest short-term effects is daunting, but controlling for them will reduce the short-term effect in experimental settings. Nonetheless, which aspect matters most is a different question than whether violent games as played *in situ* are a risk factor for aggression.

Misdirecting Attention

When a magician tells you to look one way, it is valuable to examine what you are not supposed to look at. Ferguson's article works carefully to stake out the space we should look at—only at children, only at clinical measures, and only at "societally relevant aggression"—and proposes that most of the theories are simplistic "hypodermic needle" theories. Yet many excellent studies conducted outside of these constraints are ignored by focusing only where directed by the author.

Ferguson and I completely agree (personal communication, October 11, 2011) that there is very little evidence that media violence causes serious criminal level aggression (although more research is needed). I study everyday types of aggression, such as teasing, cyberbullying, relational aggression, and even some physical aggression such as hitting and bullying. All of these studies are eliminated from consideration once we claim that we need "clinical" level outcomes. Nonetheless, if your child comes home crying about being teased, it is serious to you and to your child (and the research shows that these types of lower level aggression can predict serious negative outcomes for the victims). Several experimental, correlational, and longitudinal studies show that violent video game play predicts these varied types of aggression.

The claims that aggression measures must be standardized, or that anyone who thinks that multiple measurement methods are scientifically valuable is "self-serving," seem puzzling. The use of multiple methods and triangulation is generally considered to be good science (e.g., Creswell, 2014). Similarly, the claim that the theories are of the hypodermic needle variety is not accurate—the theories used include several well-established social-cognitive models of learning and aggression (e.g., Anderson & Bushman, 2002; Anderson & Huesmann, 2003; Bandura, 1986; Huesmann, 1986; Huesmann & Eron, 1986; Huesmann & Guerra, 1997). What is more puzzling is that Ferguson does not seem to dispute that violent games can have effects on thoughts and feelings (e.g., Ferguson, 2007) but believes that they cannot affect behavior. A century of research, however, has made it pretty clear that thoughts and feelings can influence behavior. Indeed, cognitive behavioral therapy is based on the idea that changing one's thoughts and feelings can change behaviors. No theoretical explanation is provided to explain how this relation between thoughts, feelings, and behavior would be different in the video game domain.

Regarding the claim that measures are unstandardized or lack clinical validity, it is my contention that good science replicates and extends and does not examine aspects in only one way. By restricting the sample of studies measured in this meta-analysis to a subset that fit arbitrary criteria that are not standard practice, he is able to shift the sample in ways that one could question. For example, a footnote states that one of my studies (Gentile et al., 2009) was not included because of "concerns of multi-collinearity," that we found "bouncing beta" coefficients in opposing directions, and that we reported "variance inflation factor levels near 10, which tend to produce spurious multi-collinearity results" (p. 660). Each of these claims is false. We did not find changing beta coefficients—we found that prosocial games increase prosocial behavior but violent games decrease it. That is, we found opposite direction effects as predicted by theory. Regarding the variance inflation factors, what we actually reported was that "multi-collinearity did not unduly influence the regression coefficients (i.e., variance inflation factors were less than 10" (Gentile et al., 2009, p. 756).

That is, we conducted the appropriate test and found that none of the VIFs passed the generally accepted threshold for being suspect. Of course, errors can get made, even by the best scientists. This is what the peer-review process is supposed to help fix, and this error had been noted in some reviews but was never corrected.

Biases of Participants, Experimenters, and Authors

The refusal to correct errors discovered in the peer-review process brings up the question of bias, which Ferguson's article alludes to repeatedly in several ways. Experimenter bias is important, but participant bias may be more likely. Recent studies found that, if the cover story is not good enough, participants (gamers especially) will intentionally reduce their aggression to try to make the study show null results (Bender, Rothmund, & Gollwitzer, 2013), as well as other systematic biases in gamer reporting (Kahn, Ratan, & Williams, 2014). This is why we should be reporting not only what the cover story and procedures were, but also the suspicion rates (which have been going up dramatically in the past 5 years in my experience).

Ferguson claims that manuscripts that do not cite articles that disagree with their data must be biased. The meta-analysis shows that studies not citing the critics tend to show larger effect sizes, and Ferguson concludes that therefore the authors of those studies must be biased, claiming that "researcher biases, as evidenced by citation bias in literature reviews, do play a role in influencing the effect sizes of resultant publications" (p. 655). This is flawed and tautological logic at best, and it ignores a more parsimonious explanation. When writing a manuscript, one cites the articles that are most directly relevant within a theoretical framework. Journal space is limited, and it is more likely that authors tell the story that seems clearest and most directly related to their data than that they intentionally inflate their effect sizes. This explanation fits the data much better—authors who find large effects would be more likely to cite papers that fit with their data and theory, whereas authors who find small or null effects would be more likely to cite papers that fit with their data. To claim that these data prove researcher bias is not only fallacious, it's extraordinary. It is a far cry from not citing a paper to selectively reporting results or manipulating data. Why would one claim that other researchers who find different results must be biased? Because it is an excellent rhetorical tactic designed to instill doubt.

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The author declared no conflicts of interest with respect to the authorship or the publication of this article.

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