

The Evolution of Scientific Skepticism in the Media Violence “Debate”

Douglas A. Gentile*

Iowa State University

After 60 years of research on media violence and aggression, the scientifically skeptical positions have evolved. This article examines some of the reasons why this issue continues to be a difficult one, and argues that, after examining the preponderance of data, the interesting skeptical questions are: (1) How can we put the pieces of the puzzle back together, given humans’ inherent complexity? (2) How do differing perspectives affect the interpretation of the data? Questions such as these may help to move past the polarizing rhetoric that has characterized much of the recent debate.

Psychological research on media violence effects is routinely contextualized within the frame of current events and political rhetoric. Few other bodies of research generate such strong opinions from both the public and the research communities, with extreme statements such as blaming school shootings on video games (Tassi, 2012), or disparaging the collected research as “pseudoscience” (Ferguson, 2009). Although we should all strive to be good scientific skeptics, ultimately the data are what they are. So after almost 60 years of data, how should we approach this “debate”?

One reason the data have been so hotly contested is based on psychological research methods and the (sometimes inappropriate) assumptions that come with them. For example, a well-conducted experiment randomly assigns participants to violent and nonviolent conditions whose stimuli are matched as closely as possible. Such experiments allow for strong causal interpretations to be drawn, given that all variables except for the manipulated one are assumed to be held constant. Results are, therefore, focused specifically on the difference between violent and

*Correspondence concerning this article should be addressed to Douglas A. Gentile, Iowa State University, W112 Lagomarcino Hall, 901 Stange Rd., Ames, IA 50011 [e-mail: dgentile@iastate.edu]. The author states that he has no conflicts of interest associated with this manuscript.

nonviolent content. Unfortunately, this often gets incorrectly interpreted by critics as illustrative of the researchers' presumed belief that media violence alone is a strong and monolithic influence. Many researchers, including the authors of the target article, have been calling for a recognition that human behaviors such as aggression are multicausal insofar as any one causal factor (including media violence exposure) needs to be considered within the broader net of risk and protective factors (Anderson, Bushman, Donnerstein, Hummer, & Warburton, 2015; Browne & Hamilton-Giachritsis, 2005; Centers for Disease Control and Prevention, 2010; Dodge & Pettit, 2003; Ferguson, San Miguel, & Hartley, 2009; Gentile & Bushman, 2012; Gentile, Saleem, & Anderson, 2007; Gentile & Sesma, 2003; Prot & Gentile, 2014; U.S. Surgeon General, 2001). Nevertheless, critics continue to claim that media violence researchers are suggesting a simplistic "hypodermic needle" type of effect with a passive viewer (e.g., Ferguson, 2015b). This is surprising, given that the primary theories used to help predict and explain the effects are complex and dynamic social-cognitive learning models (e.g., Anderson & Bushman, 2002; Bandura, 1986; Huesmann, 1986). Moreover, even the strongest critics of the literature find that media violence has effects on thoughts and feelings (e.g., Ferguson, 2007a). This alone should make the complexity apparent. We all know that our thoughts and feelings are related to our behaviors, but not in a simple mechanistic way. This is why the effect of media violence, or almost any experience for that matter, does not relate in a simple mechanistic way to any given outcome.

We generally seek to be affected by media. If we choose to watch a violent film or play a violent video game and it truly has no effect on us, we call it "boring" and stop watching it. We want to be affected by the media—we listen to sad songs to process feelings of loneliness, we watch a scary movie when we want to be scared, and we play violent video games when we want a rush of adrenaline. It is hard to reconcile the idea that we seek to be affected by media with the dissonant claim that media have no effects on us.

Furthermore, researchers "triangulate" across multiple study designs (i.e., experimental, cross-sectional, field experiments, longitudinal, and meta-analyses) and across multiple methods (e.g., different ways of operationalizing aggression), finding converging evidence for media effects (American Academy of Pediatrics, American Psychological Association, American Academy of Child & Adolescent Psychiatry, & American Medical Association; Anderson, 2004; Anderson & Bushman, 2001; Anderson et al., 2010; Bushman & Huesmann, 2006; Comstock & Scharrer, 2003; Ferguson, 2007a, 2007b; Ferguson & Kilburn, 2009; Sherry, 2001). I argue, therefore, that there really is not much serious disagreement about whether there is an effect of media violence on aggression. The disagreement must be about something else.

If the skeptical question is not whether there is or is not an effect, then what is a useful skeptical question currently? Is it about bias? It is sometimes claimed

that researchers who find significant effects are biased (Ferguson, 2015a), and sometimes it is claimed that researchers who find null effects are biased (Greitemeyer & Mügge, 2014). It seems unlikely that so many researchers worldwide are biased. Most researchers simply get convinced by their own data. Therefore, questioning the bias of researchers is unlikely to be a useful skeptical question. Similarly, given that there is no such thing as a perfect measure or study, are methods the best target for skepticism? Certainly, there have been a lot of bad studies conducted in this domain, but most of them get weeded out by the peer-review process. Although one should always be careful about methods, when the preponderance of evidence points the same way across multiple methods, then this also ceases to be a particularly useful skeptical stance.

Given the preponderance of converging evidence and converging consensus across multiple professional scientific review panels, a reasonable skeptical view should not argue that there is no effect, but rather that the documented effects are only part of a much bigger picture. This is precisely where some of the most valuable recent contributions have been made to the field, reminding us that “violent content” is not necessarily a unidimensional construct (e.g., Adachi & Willoughby, 2011), and that players have motivations that may interact with content (e.g., Ryan, Rigby, & Przybylski, 2006). Other contributions have focused on testing underlying mechanisms linking media violence to aggression (e.g., Gentile, Li, Khoo, Prot, & Anderson, 2014) and on examining parallel questions with other aspects of media content (e.g., Green & Bavelier, 2007; Prot et al., 2014). One valuable question, therefore, is how to put all the pieces together in a way that allows for predicting trajectories and outcomes (Prot & Gentile, 2014). Are some combinations of risk and protective factors particularly potent to predict aggression? Reductionistic methods focusing on one or two variables do not easily lend themselves to fitting all of the variables together to understand how they influence each other.

A second valuable skeptical question is: “So what?” Even if we admit that media violence is a risk factor for aggression, it is only one of many scientifically documented risk factors (Anderson et al., 2003; U.S. Surgeon General, 2001). It is neither the largest nor the smallest. Much of the disagreement in the debate seems to be about what outcomes we ought to care about. Criminologists and clinicians tend to focus on rare criminal-level or “clinically significant” physical violence, a perspective from which the effects of media violence are very small indeed. In contrast, child psychologists (like myself) focus on everyday playground-level aggression, such as teasing, ostracism, rumor-spreading, and bullying behaviors. From this perspective, the effects of media violence seem much more substantial. Although this type of aggression does not show up in crime statistics, when your child comes home crying because of it, it is hard to claim that it is not “serious” aggression.

Framed this way, the debate about media violence effects is not a matter of who is right or wrong. People of good conscience will naturally disagree about what level of outcome is worth caring about. The problem comes when we do not recognize that researchers approach the topic from these differing frames, causing them to ask entirely different questions about media violence effects. This problem is exacerbated by mass media that clumsily interpret research solely within the context of finding the solitary cause of a tragic school shooting. This polarizing context makes it more difficult to recognize that the issue of media violence is not simple and, instead, that there are multiple reasonable views, most of which are neither particularly controversial nor competing, but are complementary. The theories and data regarding media violence have evolved over the past 60 years. The skeptical stances should too.

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DOUGLAS A. GENTILE, Ph.D., 1998, University of Minnesota, is an Associate Professor of Psychology at Iowa State University. His research expertise focuses on the positive and negative effects of media on children and adolescents interna-

tionally. He was honored with the Distinguished Scientific Contributions to Media Psychology Award from the American Psychological Association (Division 46), and was named one of the Top 300 Professors in the United States by the Princeton Review.