

COMMENTARY

Manufacturing Consensus in a Diverse Field of Scholarly Opinions: A Comment on [Bushman, Gollwitzer, and Cruz \(2015\)](#)

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We appreciate the efforts of [Bushman, Gollwitzer, and Cruz \(2015\)](#) to provide new data describing parents' and professionals' opinions regarding effects of media violence. Unfortunately, we feel it is necessary to call attention to apparent errors and inaccuracies in the way those data are interpreted and represented in their article. The article includes flawed analyses, overstates the extent to which there is agreement that media violence has meaningful negative societal effects, and misrepresents the relevance of the study to an understanding of the effects of media violence on societal violence. In contrast, we call for a climate of research on media violence that better recognizes the diversity of findings and conclusions in an active and growing research agenda and eschews unwarranted insinuations about effects on criminal violence from research focused on aggression-related measures not assessing violent crime.

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We are a group of researchers from several academic fields with a shared interest in research on the societal effects of media violence. Our opinions vary regarding the possible relationship between exposure to media violence and aggression. Some of us are skeptical about the validity of findings linking media violence and aggressive

outcomes, while others among us are receptive to research linking exposure to media violence with at least some outcomes conceptually related to aggression, either in laboratory research or more externally valid settings.

Our varied opinions about the nuances of the literature on media violence and aggression not-

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withstanding, we agree on some substantial concerns about the way [Bushman, Gollwitzer, and Cruz \(2015\)](#) report and interpret their survey. Those concerns can be summarized in two general points: (a) Even taking these data at face value despite methodological shortcomings, [Bushman et al.](#) greatly exaggerate the extent to which a “consensus” exists on the effects of media violence among media researchers (described in their article as “media psychologists,” and “communication scientists”), and (b) Despite their acknowledgment that research measures conceptually related to aggression are different from actual criminal violence, [Bushman et al.](#) still reference acts of prominent mass violence to deliberately blur the line between them and inflate the apparent relevance of research on media violence to violent crime. Here, we emphasize that the survey shows not only that there is a diversity of opinion on the effects of media violence, but that research on media violence and aggression generally fails to inform an understanding of potential effects of media violence on violent crime.

The Distinction Between a Majority and a Consensus in Researchers’ Opinions About Effects of Media Violence: The Need for Acknowledgment of Opposing Views

[Bushman et al. \(2015\)](#) interpret their survey results as “major consensus among media researchers that violent media can increase aggression in children” (p. 211), as well as consensus among parents and pediatricians. There are other surveys on the topic with different researcher samples ([Bushman et al., 2015](#), sampled one of 55 divisions of the American Psychological Association to represent “media psychologists” and one of 26 divisions of the International Communication Association to represent “communication scientists”) that find lower rates of agreement among researchers that media violence has negative effects, such as preliminary findings shared by [Van Looy et al. \(2013\)](#). Also, other methodologies such as the Delphi method would be more robust for gathering consensus in controversial areas, facilitating communication between experts, and assisting formation of a well-informed group judgment. ([Adler & Ziglio, 1996](#); [Helmer, 1977](#); [McBride, Pates, Ramadan, & McGowan, 2003](#); [Okoli & Pawlowski, 2004](#)). Our primary

concern, though, is that even taking the findings from [Bushman et al.](#) at face value, their data do not support their claims of a consensus. Indeed, their results indicate a slight majority of “media psychologists” (61.1%) and “communication scientists” (56.3%) agreeing or strongly agreeing with a Likert-type statement that there is a causal link between media violence and aggression. That majority, though, is not a consensus, nor is it represented accurately by [Bushman et al.](#), who conveniently chose to ignore all neutral respondents in claims such as that “among those researchers who have an opinion, 8 of 10 media researchers agree that violent games increase aggression” (p. 211; see also [Bushman & Pollard-Sacks, 2014](#)).¹ We find this insincerity disappointing; why would researchers with a neutral position not be germane to the question of whether there is a consensus on effects of media violence? There are a number of reasons that respondents might express a neutral opinion, but a neutral opinion is not the absence of an opinion nor a nonresponse.

If the survey’s neutral respondents are considered, which it seems they should by any rationale, the results indicate that 61% of “media psychologists” and 56% of “communication scientists” agreed that there is a link between media violence and aggression, while 39% of “media psychologists” and 44% of “communication scientists” do not (either because they disagree or are neutral). We are not at all comfortable with the idea that a putative majority comprising fewer than two of three researchers in samples from two fields should be declared a consensus to stifle dissent. [Bushman et al. \(2015\)](#) make frequent attempts to draw analogies between the scientific consensus on climate change and researchers’ opinions regarding media violence; [Figure 1](#) indicates that according to the survey data, that comparison is inappropriate.

¹ Existing references to this study’s results, such as in [Bushman & Pollard-Sacks \(2014\)](#), may appear to report slightly different descriptive statistics because the initial analyses submitted and accepted for publication here included three groups rather than four: (a) a combined media researchers group combining the psychology and communication samples, (b) pediatricians, and (c) parents, but they are based on the same survey data; to our knowledge, the research design differs only in terms of how the groups were constructed.

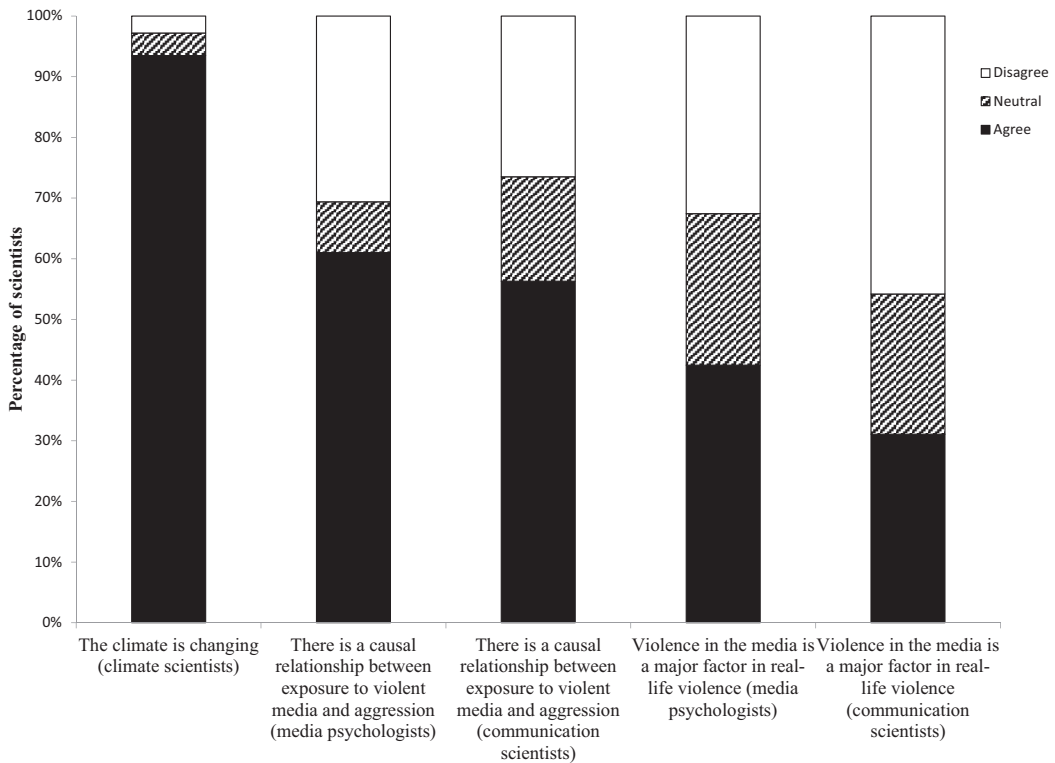


Figure 1. A comparison of researchers' opinions on climate change and effects of violent media. Results for opinions on climate change are from a survey of 373 climate scientists reported in [Bray and von Storch \(2010\)](#). Results for opinions on effects of violent media are from a survey of 132 communication scientists and 239 media psychologists reported in [Bushman et al. \(2015\)](#).

Interestingly, there is also evidence that the trajectory of researchers' opinion about the effects of media violence may be moving not toward the consensus claimed by [Bushman et al. \(2015\)](#), but away from it. In 1984, Murray assessed researchers' agreement with a National Institutes of Mental Health (NIMH) statement that ". . . the consensus among most of the research community is that violence on TV does lead to aggressive behavior by children and teenagers" (p. 1) and that "in magnitude, TV violence is as strongly correlated with aggressive behavior as any other behavioral variable that has been measured" (p. 1), reporting that 82% of media researcher respondents chose to "strongly agree" (69%) or "moderately agree" (13%). By any interpretation, the survey's results suggest migration away from a consensus on negative effects of media violence among researchers over the more than three decades

that have elapsed since [Murray's \(1984\)](#) survey. This trend may also indicate a "generational divide" in scholarly opinions on media effects, with possible differences in prevalence of opinions about negative effects of media among younger scholars than older scholars.

Aside from the above concerns about the study's descriptive results, a somewhat more technical concern regards the statistical analyses reported by [Bushman et al. \(2015\)](#). Readers should be aware that this is the second set of analyses we were provided by [Bushman et al.](#) which are purported to assess consensus. In the analyses originally submitted and accepted for publication, which have already been cited for some time ([Bushman & Pollard-Sacks, 2014](#)), [Bushman et al.](#) calculated deviation of respondents' average levels of agreement from the scale midpoint, falsely arguing that their significant results (i.e., average agreement scores sig-

nificantly higher than neutral) were evidence for consensus. Statistical tests examining the difference between a groups' average score for an item and the "neutral" scale midpoint do not provide information about the homogeneity of response patterns in the sample (i.e., consensus). For example, if 100% of the researchers ("media psychologists" and "communication scientists") surveyed indicated they were neutral in their level of agreement, the average level of agreement would be neutral, yet the consensus of agreement would be extremely high (unanimous, in fact). Similarly, if 98.3% of the researchers surveyed indicated a neutral level of agreement, the analyses conducted by Bushman et al. would produce a significant effect (i.e., the mean response significantly differing from neutral; $t(238) = 2.01, p < .05$) if as few as four researchers (1.6%) strongly agreed that violent media had an effect on aggression.

Bushman et al. agreed with us that this original analytic method does not actually assess consensus, and added new analyses to the published article using a different operational definition of consensus (see Bushman et al., 2015, p. 203, footnote 2). However, in the revised published article they also continue to present the results of the significance tests comparing responses to the scale midpoint in a confusing manner to make it appear as if they are measuring consensus among participants. Perhaps the most notable example of this obfuscation is their conclusion that, "On average, all participants agree that there is a causal effect of violent media on aggression" (p. 206). Although we don't understand the logic of the statement "On average, all participants agree," it seems that Bushman et al. are trying to suggest that such an analysis somehow provides insight into consensus when they have already acknowledged it does not.

After acknowledging the fundamental errors of the analyses described above (see Bushman et al., 2015, p. 203, footnote 2), Bushman et al. added a new author (Gollwitzer) and a new set of primary analyses purported to assess consensus. Specifically, in their revised analyses that appear in the published article, it is now claimed that low values for ω^2 and η^2 are sufficient evidence that there is consensus among participants in the study. However, these new analyses are as flawed as the previous analyses. Bushman et al. are correct that low ω^2 and η^2

values would indicate there was little difference between the group means, but such results provide no insight into the rate of consensus or agreement among the respondents. These authors are confusing the similarity of average scores across group means with the rate of consensus among the people within those groups. Analyses comparing groups' average scores tell us nothing about the homogeneity (consensus of agreement) among respondents.

To illustrate the negligible value of the authors' new methodology for determining consensus, imagine a scenario in which media psychologists, communication scientists, pediatricians, and parents responded to the item "media violence causes aggression" with completely random responses on the 1-to-5 Likert-type scale. This is a situation in which no rational person would ever say there is "broad consensus" among people about this item. However, using the exact same analytic methodology proposed by Bushman et al. (2015), the expected ω^2 and η^2 in that scenario would essentially be 0 because the average levels of agreement among the groups would be equivalent. Similarly, if participants within each group in the survey conducted by Bushman et al. had been evenly and sharply divided in their opinion, with half the respondents strongly disagreeing and half strongly agreeing, results would again produce ω^2 and η^2 values close to zero, meeting the flawed criteria for consensus provided by Bushman et al. Thus, according to the operational definition of "consensus" now used by Bushman et al., both random data and data in which there are extreme disagreements among respondents would produce a near perfect "consensus!" In short, these new analyses presented by Bushman et al. again provide no information concerning whether or not there is consensus among researchers about whether violent media cause aggression.

The problematic nature of the above hypothetical scenarios dealing with ω^2 , η^2 , and one-sample t tests illustrates the flawed logic behind the analytic strategies Bushman et al. (2015) have continued to use in order to purportedly assess consensus, even when they have published new analyses in acknowledgment of the methodological problems we previously identified. As we suggested in the initial draft of this comment that prompted a revision of the article, the most effective indicator of consensus avail-

able from the survey data is simply the percentage of survey participants who agree with their survey's Likert-type statements related to effects of media violence. Again, as Figure 1 indicates, a substantial minority of the media researchers sampled do not agree that violent media cause aggression, and a majority do not agree that violent media are a major cause of serious violence. Regardless of whatever erroneous statistical analyses Bushman et al. continue to utilize in an attempt to construct a flawed definition of consensus based on patterns of variation in groups' average scores, the descriptive response frequencies to their own survey items vividly illustrate that contrary to the misleading title and conclusions of the Bushman et al. article, there is not "broad consensus" among researchers.

We implore scholars conducting research in this area to continue to acknowledge and engage the diversity of findings and opinions regarding effects of media violence rather than indulging a quixotic quest for a consensus that does not at present exist. One survey using narrow samples and relying on a problematic analysis strategy that fails to support its conclusion should not be misinterpreted by the research community as conclusive evidence that there is one canonical viewpoint on the effects of media violence. Instead of prematurely "nailing the coffin shut" on an open discussion about media effects and casting aspersions on the character and motivations of dissenters (Anderson, 2013; Huesmann, 2010), an open-minded approach to an active and growing research agenda will be the pathway that best represents the principles of scientific dialogue and that will be most likely to uncover truths in this challenging and controversial area. Disagreement and critique are valuable aspects of scientific dialogue, but ignoring alternate views is not.

The Distinction Between "Aggression" and Criminal Violence as an Effect of Media Violence Exposure: The Need for Clarity and Candor

The survey's findings regarding opinions about the effects of media violence on real-life violence are also important to the research debate about effects of media violence. Here, the survey data indicate that a minority of media researchers, 42.5% of "media psychologists,"

and 31.1% of "communication scientists" (see Figure 1) agreed that media violence was a major factor in real-life violence; nearly two thirds either disagreed or were neutral. This low rate of agreement with the notion that media violence causes violent crime should discourage exaggerated claims about the relevance of research on aggression measures to an understanding of causes of societal violence.

Bushman et al. (2015) rightly note that "much of the confusion about the 'media violence debate' arises over the distinction between aggressive and violent behavior" (p. 201). However, we disagree with their claim that there is a "dearth of research studies on the link between media violence exposure and real-world violence" (p. 201). There are a number of relevant studies that have examined the topic and have found little evidence that media violence as a useful causal predictor of criminal violence (Farrington & Loeber, 2002; Felson, 1996; Ferguson, 2011; DeCamp, 2014; Hawkins et al., 1998; Markey, Markey, & French, 2014; Messner, 1986; Savage, 2004, 2008; Savage & Yancey, 2008; Surette, 2013). The few studies that have found small correlations, either find that these disappear once other factors are controlled for (Ybarra et al., 2008) or have utilized weak methodologies, such as reliance on youth self-reports, that introduce demand characteristics (DeLisi, Vaughn, Gentile, Anderson, & Shook, 2013). Among criminologists, research exploring media violence as a potential risk factor finds little cause for concern (Felson, 1996; Messner, 1986) and reviews of risk factors for violence tend to exclude media violence as a useful predictor (Farrington & Loeber, 2002; Hawkins et al., 1998).

As Bushman et al. (2015) stress, there are studies suggesting that exposure to violent media affect a number of responses related to aggression, from questionnaire measures to laboratory behavioral measures. There are, of course, studies that fail to find such effects, and an increasing number of alternative explanations for effects that have been found (Adachi & Willoughby, 2011; Elson, Mohseni, Breuer, Scharkow, & Quandt, 2014). Even if we assume that research studies demonstrating the effects of media violence on aggression are consistent and valid (some of us do not), media violence can then be placed among several other cues that have been found to influence similar ag-

gression measures, such as references to weapons (Anderson, Benjamin, & Bartholow, 1998), black competition uniforms (Frank & Gilovich, 1988), words and symbols reminiscent of America (Ferguson & Hassin, 2007), and low blood glucose levels (Bushman, DeWall, Pond, & Hanus, 2014).

It is unsurprising, then, that researchers studying possible effects of media violence on aggression-related measures frequently allude to prominent episodes of violent crime such as mass shootings, terrorism, and other homicide (Markey, Markey, & French, 2014) to give the impression that their research is germane to issues of much greater societal immediacy than laboratory aggression measures, self-report questionnaires, voodoo dolls, and administration of noise blasts and hot sauce to strangers. Media violence researchers have made claims such as that up to 30% of societal violence can be attributed to media violence (Strasburger, 2007), that media violence exposure is a major contributor to societal violence (Anderson, 2000), that the public health effects of media violence on society can be compared with those of smoking on cancer (Bushman & Anderson, 2002), and that even weak effects of media violence can be expected to produce a rise in murders at schools and other violent acts (Bushman & Anderson, 2002). These unsupported claims about links between aggression research and prominent extreme societal violence are mirrored by researchers' speculative claims in the popular media that violent media use may have played a role in specific mass shooting events (Bushman, 2013; KCCI News, 2012). Given the survey's finding that most researchers did not voice agreement that media violence is a major influence on societal violence, and given the authors' assessment that speculative attempts to implicate violent media as a cause of prominent violent crimes are "unwarranted claims" (p. 201), we hope Bushman et al. will agree that continued references to mass crimes in research articles about aggression measures and unwarranted speculation in popular media should be avoided. In short, they serve only to mislead readers and cloud the distinction between aggression and violence.

To this point, we end our commentary where Bushman et al. (2015) began their ar-

ticle—with a powerful quote from U.S. President Barack Obama in the wake of the horrific mass shooting that occurred in Newtown, CT, in December 2012. Considering that Bushman et al. note the distinction between aggression-related measures and actual violent crime, discouraging extrapolating findings from research dealing with effects of media violence on aggression to actual violent crime, and criticize "unwarranted claims regarding the relationship between violent video games and violent criminal behavior" (p. 201), this opening quote seems extremely inappropriate. If Bushman et al. are truly concerned that violent crime and aggression get unintentionally confounded, why open the article with such a quote at all?

We strenuously appeal to Bushman et al. and others to reconsider their use of such emotionally laden, but irrelevant, quotes to add an air of urgency and gravity to research that does not directly inform causes of mass murder and other violent crime. The strategy of draping research findings about media violence and aggression measures in a context of references to unrelated prominent mass shootings and other violent crimes to frame research dealing with effects of media violence on noncriminal aggression measures is frequently used but entirely inappropriate (Markey, Markey, & French, 2014). At best, the tactic confuses and distracts readers; at worst, it exploits these tragedies and their victims. We urge our fellow scholars to ensure that the continuing dialogue about the potential effects of media violence appropriately interprets the potential scope of the implications of our research rather than referencing our society's most tragic moments to give the false impression that media violence research tells us much of anything at all about the causes of those tragedies.

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