was the appropriate database to use because it includes more than 3 million records from psychology and many related fields (e.g., medicine, forensics, neuroscience). Indeed, Ferguson also used PsycINFO for his own meta-analysis of video game effects (Ferguson, 2007).

Some Disagreement in the Field Does Not Mean That the Field Is Evenly Divided

Ferguson (2013) stated, "At present we thus have two groups of scholars, approximately equal in number, who disagree vehemently about the data on video game violence effects" (p. 64), but he did not provide any data. We recently collected data from 371 media researchers and found that 66% agreed that violent video games increased aggression in children, 17% disagreed, and 17% were undecided (response rate = 46.3%; Bushman & Cruz, 2013). There is rarely, if ever, complete consensus on violent video game effects or on any other topic in science. However, just because there is some disagreement does not imply that the field is evenly divided. Our survey suggests the vast majority of researchers working in the area (80% of those who have an opinion) believe that violent video games increase aggression.

What Constitutes a Trivial Effect?

Most meta-analyses of violent media effects, including violent video game effects, find average correlations in the range of .15 to .30. Ferguson (2013) views these correlations as trivial in size, but that is not the consensus in the scientific community. According to Cohen's (1988) conventional values, a correlation of .1 is "small." A point-biserial correlation of .1 translates into a standardized mean difference of about 0.2, which many researchers do not consider trivial at all, particularly in lab experiments where the participants receive a very small "dose" of the treatment. In video game experiments, participants often play a game for only 15–30 minutes. It is impressive that playing a violent video game for just 15–30 minutes on a single occasion can have significant and measurable effects on human thought and behavior.

According to Lipsey's (1990) empirically based conventional values, a correlation of .075 is "small." But "small" is not the same as "trivial." For example, the average correlation obtained from 322 meta-analyses of more than 25,000 social psychology studies involving over 8 million participants was .2 (Richard, Bond, & Stokes-Zoota, 2003).

There are circumstances in which "small" effect sizes warrant serious concern: "When effects accumulate across time, or when large portions of the population are exposed to the risk factor, or when consequences are severe, statistically small effects become much more important . . . All three of these conditions apply to violent video game effects" (Anderson et al., 2010, p. 170).

Laboratory Measures of Aggression Are Not Trivial

Laboratory aggression paradigms are sometimes faulted for being "artificial" or "unrepresentative" of "real-life" aggression. Two different research teams using meta-analytic techniques have supported the validity of standard laboratory aggression paradigms. One meta-analysis found impressive levels of convergence across a wide range of laboratory aggression measures (Carlson, Marcus-Newhall, & Miller, 1989). Another meta-analysis found that "real" and laboratory measures of aggression are influenced in similar ways by situational variables (e.g., violent media, alcohol, provocation) and by individual difference variables (e.g., trait aggressiveness, sex, Type A personality) (Anderson & Bushman, 1997).

Conclusion

In conclusion, we argue that the divided and controversial U.S. Supreme Court opinion in *Brown v. Entertainment Merchants Association* (2011) informs us of the state of First Amendment jurisprudence, in which violent video games have been labeled "free speech," rendering them nearly impossible to regulate criminally under the prevailing strict scrutiny analysis. The Supreme Court decision, however, is a political opinion and tells us nothing about the scientific validity of violent video game studies or their effects on children.

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A Way Forward for Video Game Violence Research

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I thank Hoffman (2014, this issue) and Bushman and Pollard-Sacks (2014, this issue) for their comments on my original article (Ferguson, February–March 2013) on video game violence and the 2011 *Brown v. Entertainment Merchants Association (EMA)* Supreme Court case. I appre-

ciate their thoughtful tone and hope that this exchange may serve as a model for dialogue in this area to come. In this response I identify some continued areas of disagreement but also aim to sketch out some early ideas for how we might move this area of research forward.

Continued Areas of Disagreement

The first area of disagreement concerns the continued need to clarify the issues we are discussing. As for violence as an outcome, Hoffman (2014) claimed, "Violence (regardless of its etiology and cause) is now reaching epic proportions within both urban and rural communities" (p. 305). However, in fact, violence has been declining rapidly cross-nationally and we appear to be in one of the least violent epochs in human history (Pinker, 2011). Youth violence rates in 2011 were only 12% of what they were 20 years previous (Federal Interagency Forum on Child and Family Statistics, 2013). Little evidence of violence reaching "epic proportions" can be found. As for aggression as an outcome, Bushman and Pollard-Sacks (2014) argued that contested aggression measures used in laboratory research should be considered valid. They mainly cited studies showing intercorrelations among measures of aggression as evidence, but such studies should not be mistaken as evidence of construct validity, particularly when such intercorrelations may be explained as due, in part, to unstandardized uses of these measures, allowing researchers to select outcomes whose results best fit their hypotheses and ignore those that fail to confirm. Moreover, a recent replication of the work Bushman and Pollard-Sacks cited found the validity of laboratory aggression measures to be low (Mitchell, 2012). Recent research has indicated that the unstandardized measures substantially influence effect sizes (Elson, Mohseni, Breuer, Scharkow, & Quandt, 2014). Even what is meant by a "violent video game" is unclear. In terms of predictors, one scholar acknowledged in a recent murder trial in Beason, Illinois (the Christopher Harris trial) that even games such as *Pac Man* might be considered violent video games, a notion most of the general public would likely consider absurd. Perhaps use of the term "violent video game" is too provocative for objective scholarly research and a more neutral, operationally defined term might be adopted. In either case, greater care is needed in defining our terms.

The second area of disagreement concerns whether a consensus exists among scholars. Hoffman (2014) listed several points that "leading experts" (p. 305) agree on, and Bushman and Pollard-Sacks (2014) cited some of their own work to conclude that the majority of scholars agree with their position and that those who do not are less qualified. However, a recent survey of 544 media scholars and psychologists (Van Looy et al., 2013) came to very different conclusions, wherein only 10.1% of scholars either agreed or strongly agreed with the belief that digital games' influence on aggression represented a problem for society (27% were undecided). And recently a consortium of over 230 psychologists, criminologists, and media scholars wrote an open letter to the American Psychological Association asking them to retire their media policy statements and refrain from making broad-reaching policy statements in an area of developing research (Consortium of Scholars, 2013). Thus there appears to be little consensus on a consensus. As to whether skeptical scholars are less experienced, Bushman and Pollard-Sacks stand by their original analysis, which is fair enough. But let us imagine, for the sake of argument, that Bushman and Pollard-Sacks are correct and that they are more experienced than their critics. What does this prove? Their critique of the amicus briefs did not address the substance of either brief, merely the resumes of those who signed them, thus constituting an *argumentum ab auctoritate* logical fallacy. One has to wonder where science would be today were such arguments allowed to hold sway given that *arguments from authority* are commonly employed in resisting new scientific ideas (Walton, 2008).

Bushman and Pollard-Sacks (2014) argued that the *Brown v. EMA* case was decided on First Amendment grounds, not on the basis of the science. U.S. Supreme Court decisions are, of course, made on constitutional grounds. However, as Bushman and Pollard-Sacks themselves noted, the science was specifically scrutinized and criticized in the majority opinion (which also rejected the dissenting arguments of Justice Breyer). My point is that our field is experiencing a credibility crisis in the eyes of the courts. I invite readers to read the 2011 *Brown v. EMA* decision (and the decisions of lower courts such as in the 2005 *Entertainment Software Association, Video Software Dealers Association, and Illinois Retail Merchants Association v. Blagojevich, Madigan, and Devine* case) and decide for themselves whether our field is experiencing such a credibility crisis.

Bushman and Pollard-Sacks (2014) maintained that the small effects seen in much of media violence research are not trivial. Across meta-analyses, bivariate effect sizes are closer to $r = .15$ (not $r = .30$, see Sherry, 2007), but evidence suggests even these effect sizes are inflated by publication bias and use of unstandardized measurements (Ferguson & Kilburn, 2009). And focusing on bivariate results is itself deceptive, because it fails to note that small effects may be due to uncontrolled variables such as gender or personality. As I noted in my original article, studies which control for more variables are more likely to return null results. Contrary to the argument that small effects accumulate over time, the weakest results have been obtained in longitudinal studies (Anderson, Shibuya, Ihori, Swing, Bushman, et al., 2010). In Bushman's own meta-analysis (Anderson et al., 2010), with only Time 1 aggression and gender controlled, longitudinal effects were only $r = .07$. Whether it is video games or some other topic, how much closer to zero need we get before we are willing to label an effect trivial?

Hoffman (2014) suggested that research on media and prosocial behavior is less contested than research on media and aggression. However, this argument neglects both the issue of failed replications in the prosocial field (Tear & Nielson, 2013) and evidence that violent video games increase prosocial behavior in at least some contexts (Granic, Lobel, & Engels, 2014). This argument also makes two logical errors. First, it implies that a clear delineating line exists between prosocial and "violent" media, which is unlikely. Second, Hoffman's argument also makes the error of *false equivalence* in assuming that prosocial and aggressive behaviors are equivalent. They may be, of course, but they may not be. That is an empirical question, not one that can be assumed.

A New Path Forward

Hoffman (2014) wondered whether it is time for psychological science to offer a "mea culpa" on media violence research. Perhaps that time has indeed come, but perhaps much of the acrimony in debates such as this one comes from what I believe is the mistaken perspective that an old perspective must "lose" if it is being replaced by a new one. Science is about advancement upon previous beliefs. Forgetting such may cause both sides to dig in their heels. As scientists we are destined to be proven wrong. Beyond simply continuing the debate, in this section I hope to offer a few thoughts on how we might move this area of research forward.

1. I argue that media psychology has focused, and overly so, on media content as a driver of behavior. This is understandable, although it may also be influenced by

societal "culture wars" over offensive content and, thus, be more prone to bias.

2. Increasing research suggests that user motivations are more critical than content in understanding media effects. I submit that media psychology would do well to learn from our neighboring field of communication, wherein users and their motivations are thought to drive the user/media experience more so than content (Sherry, 2007). I also echo the eloquent arguments of Granic and colleagues (2014), who argued for a balanced approach to understanding media effects.

3. Media, even with objectionable content, may have both positive and negative influences. Where objectionable content is concerned, it is too easy to adapt scholarly inquiry to social "culture wars," which can distort both the development of our hypotheses and the interpretation of our data. If media effects are indeed user driven, then these effects are idiosyncratic. That is, the same form of media may have very different influences on different individuals. Perhaps this is uncontroversial, but media psychology has tended to communicate in the language of general, global effects when it would be better served by use of a more nuanced, conditional language.

4. A *sociology of media research* approach highlights the view that science is imperfect and does not exist in an objective social vacuum. We are all human and respond to social narratives, political pressure, funding opportunities, emotional reactions to objectionable content, and a desire to obtain moral high ground and social influence. Understanding how these processes influence the science itself, I argue, will help us to avoid repeating mistakes of the past.

I conclude by saying that I believe we can move past debates on media violence and that scholars on both sides of the issue must be part of this advancement. We can't be satisfied with the way things were, particularly as new data emerge to reveal ways in which our thinking can be improved. This does not mean we should lambast the past, but neither should we reify it. It is time to move forward with a more sophisticated perspective on media effects that focuses less on moral objections to certain content and more on media consumers and their motivations.

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Racism Inflation

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Millions of Americans will be outraged, and rightly so, by Neville, Awad, Brooks, Flores, and Bluemel's (September 2013) assertion that "racial color-blindness is . . . actually an expression of ultramodern notions of racism among White Americans" (p. 455). These authors have expropriated the concept of color-blindness and have distorted it beyond recognition.

First, Neville et al. (2013) included within the concept of color-blindness the beliefs that racism no longer exists in America and that being White in America does not bestow certain advantages. What is the justification for this other than to make it easier to find correlations between color-blindness and measures of racism? Their construct of color-blindness is not what most Americans have in mind when they speak of color-blindness.

Contrary to their assertion, color-blindness, in its essence, means that in many, perhaps most, contexts, an individual's race ought not to be taken into account. Examples include deciding whom to hire for most jobs, who can vote, and decisions before the law (e.g., guilt or innocence at trial). In other contexts, however, considerations of race are appropriate. This is the case, for example, in treating an individual in psychotherapy, in selecting a representative sample of Americans for a survey, or when trying to fully understand someone. It certainly does not mean lack of awareness of racism in contemporary America.

Second, even if this lack of awareness of the state of race relations is nevertheless incorrectly included in the concept of color-blindness, such ignorance is not the equivalent of racism. Whereas Neville, Lilly, Duran, Lee, and Browne (2000) seemed to understand that color-blind ideology and racial prejudice were "distinct concepts" (p. 66), although perhaps correlated, 12 years later, Neville et al. (2013), in contrast, have equated the two. The latter position means that anyone endorsing their concept of color-