

## Media Violence and the American Public Revisited

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We thank Mark S. Kiselica (2002, this issue), Christopher J. Ferguson (2002, this issue), and Richard W. Bloom (2002, this issue) for their thoughtful comments on our article (Bushman & Anderson, June/July 2001). Our main point was that there is little correspondence between scientific knowledge about versus news reports about media violence effects. Since 1975, there has been no scientific doubt that viewing violence increases aggression. However, as scientific evidence grew stronger from 1975 to the present, news reports moved in the opposite direction.

Kiselica (2002) correctly noted a directional error in Figure 2 (Bushman & Anderson, 2001, p. 481). We caught and alerted journal staff about it but apparently were too late. Ferguson (2002) and Bloom (2002) raised several commonly voiced criticisms that we address below.

### The Cigarette Smoking Analogy

Ferguson (2002) argued that the cigarette smoking analogy is inappropriate. The analogy is not necessary to the main point of our article, but three main criticisms of the media violence-aggression link apply equally to the lung cancer-smoking link.

1. Some nonsmokers get lung cancer; some smokers do not. Therefore, smoking is not a necessary and sufficient cause of lung cancer.

2. Leading medical researchers decided that smoking causes lung cancer long before the exact mechanisms were well understood.

3. There are differences in susceptibility to smoking-induced cancer.

In some ways, the media violence literature is stronger than the smoking literature. Media violence research includes true experiments with human participants. To our knowledge, there are no true experiments with humans being randomly assigned to smoking or nonsmoking conditions for a sufficient period of time for cancer (or precancerous states) to develop.

### Three Critiques

Ferguson (2002) raised three additional critiques: (a) violent media are not necessary

precursors to violence, (b) violent media are not sufficient to cause violent behavior, and (c) media violence effect sizes are small. Media violence researchers agree with the first two points. However, the implication that media violence therefore cannot cause violence requires an unusually restrictive view of causality. Factors that are neither necessary nor sufficient are frequently deemed causal by scientists, public policy makers, and lay people alike. Adopting this view would require major changes in beliefs, social action, and public policies. Drinking a large number of alcoholic beverages prior to driving does not always lead to fatal accidents. Then should driving while under the influence of alcohol not be illegal? Similarly, by this logic, smoking is not a cause of cancer. Media violence researchers consistently note that more extreme forms of aggression are relatively infrequent and require the confluence of many causal factors, most of which are neither necessary nor sufficient by themselves (Anderson & Bushman, 2002b; Bushman & Huesmann, 2001; U.S. Department of Health and Human Services, 2001). The third point is irrelevant to whether media violence effects are real and to whether news media reports change appropriately as evidence accumulates. Furthermore, our article demonstrated that media violence effects are equal to or greater than many other effects that modern society has decided are very important (e.g., effect of asbestos on cancer).

### Media Violence Research and Causality

How does one determine whether a specified factor is causal? The scientific community typically considers the following four questions.

1. Is there a good theoretical (i.e., causal) explanation? The basic processes underlying short- and long-term media violence effects are well understood (e.g., Anderson & Bushman, 2001; Bushman & Huesmann, 2001).

2. Do the data fit the theory? The data fit the theory quiet well. Numerous meta-analytic review articles using varying definitions of *aggression* and varying subsets of empirical studies have all found a significant positive relationship between media violence and aggression (e.g., Anderson & Bushman, 2001, 2002a; Hearold, 1986; Paik & Comstock, 1994; Wood, Wong, & Chachere, 1991). Furthermore, data on the underlying causal mechanisms fit well with current theory.

3. Do different methodologies yield consistent findings? We used our media violence data set to estimate media violence effect sizes from four different methodologies. Table 1 shows a significant relationship between exposure to media violence and aggression regardless of the methodology used.

4. Are there plausible alternative explanations of the entire set of findings? No plau-

**Table 1**  
Average Effect Size ( $r^+$ ), Confidence Interval (CI), Number of Independent Samples ( $k$ ), and Total Sample Size for Four Types of Media Violence Studies

Methodology	Effect size ( $r^+$ )	95% CI	Samples ( $k$ )	N
Cross-sectional	0.18	0.17–0.19	86	37,341
Longitudinal	0.17	0.14–0.20	42	4,975
Lab experiments	0.23	0.21–0.26	124	7,305
Field experiments	0.19	0.15–0.23	28	1,976

sible alternative explanation can handle more than a subset of existing data. For example, the possibility that aggressive people like violent media more than do nonaggressive people is relevant only to cross-sectional studies. Longitudinal and experimental studies have controlled for this factor and still find a significant positive media effect.

In summary, the violent media–aggression domain meets all the criteria that scientists typically consider when deciding whether one variable causes another.

### Media Violence and Societal Violence Across Time

Ferguson (2002) raised another common argument: Because U.S. violent crime rates have decreased in recent years, exposure to media violence cannot cause increases in societal violence. Such reasoning might be valid if (and only if) three assumptions are true: (a) Exposure to media violence was increasing during this time, (b) violent crime rates among youth were actually decreasing, and (c) media violence is the only factor that causally contributes to societal violence. The first assumption is probably true (e.g., Comstock & Scharrer, 1999). The second assumption is highly debatable: The rate of self-reported acts of violence by youth increased during this time (U.S. Department of Health and Human Services, 2001, Figures 2–8, p. 27). The third assumption is clearly false. Many factors contribute to changes in societal violence and might well account for the recent overall decline in violent crimes in the United States. Four possibilities are (a) the U.S. population was getting older, (b) U.S. residents were being imprisoned at record rates during this time span, (c) unemployment and poverty rates were low when overall crime rates were declining, and (d) the number of young people carrying guns declined during this time span.

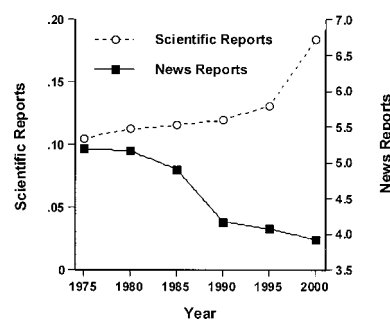
### Misreading and Misunderstanding

Bloom's (2002) criticisms seem to come from misreading or misunderstanding our article.

For instance, he alluded to the fact that a correlation of .3 accounts for only 9% of the variance. He then said, "Would not a discerning newspaper reader conclude that the media are being accurate in using the term *weak*?" (p. 448). Regardless of what reporters think about the magnitude of .3 correlations, the key question is, How can news reports of media violence effects make significantly weaker statements over time while the scientific evidence is getting stronger?

Suppose that news reporters adopt a very conservative approach to evaluating the strength of research evidence. Such an approach suggests that instead of comparing news reports with the average effect size estimates, we should be comparing them with the lower confidence interval (CI) boundaries (see Figure 1). Regardless of how reporters generated media violence news reports in the early to mid-1970s, a conservative mindset does not explain the divergence across time. Astute statistical readers will surmise that the positive slope between lower CI estimates and time displayed in Figure 1 would necessarily get even steeper

**Figure 1**  
Conservative Scientific (Lower Boundary of 99.9% Confidence Interval) Versus News Reports of the Effect of Media Violence on Aggression



Note. Adapted from "Video Games and Aggression," by C. A. Anderson, in D. Ravitch & J. P. Viteritti (Eds.), *Children and the Popular Culture*, Johns Hopkins University Press, in press. Adapted with permission.

if even more conservative criteria were adopted (e.g., 99.99% vs. 99.9% CIs).

Bloom (2002) also implied that the decline in news report scores from 1975 to 2000 is nonsignificant. However, we reported that this decline was statistically significant, " $t(521) = 2.79, p < .01, d = 0.31$ " (Bushman & Anderson, 2001, p. 483). The downward shift of more than one full scale point also seems important, especially when the scientific data are moving in the opposite direction.

A final point highlighted by Bloom (2002) concerned the difference between the results of experimental versus correlational studies. Three comments are warranted. First, it would be useful to know why different patterns have emerged over time, but we have no relevant data. Second, if Bloom is suggesting that reporters know about the important differences between these two types of studies, he must speak to very different reporters than we do. Third, the correlational studies are hardly irrelevant.

## Conclusion

These further considerations highlight our original point. Although we do not know what is driving news reports on media violence effects, we can say with confidence and disappointment that it is not primarily the scientific literature.

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