



Subtracting From Scientific Knowledge About Media Effects

A Review of

Adolescents, Crime, and the Media: A Critical Analysis

by Christopher J. Ferguson

New York, NY: Springer Science + Business Media, 2013. 175 pp.

ISBN 978-1-4614-6740-3. \$129.00

<http://dx.doi.org/10.1037/a0034788>

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Within popular culture (and to a lesser extent, psychology), there has been hearty debate about the effects of media on youth and behavior. A goal of Christopher Ferguson's book *Adolescents, Crime, and the Media: A Critical Analysis* is to "cut past the agendas of both anti-media advocates and the media companies themselves" (p. 13). It should be noted that Ferguson himself is among the most vocal critics of media effects research (e.g., Ferguson, 2010; Ferguson, 2013; Ferguson & Brannick, 2012; Ferguson & Dyck, 2012; Ferguson & Hartley, 2009; Ferguson & Kilburn, 2010; Ferguson & Rueda, 2010; Ferguson & Savage, 2012), and therefore his goal of cutting past such agendas seems a particularly difficult task. Indeed, suggesting that mainstream media researchers are "anti-media" is itself quite revealing.

As the book's title suggests, much focus is on a critical (as in negative, not strengths-and-weaknesses) analysis of media effects research. Numerous criticisms are raised regarding the influence of media on behavior (and crime) in general. Importantly, many of these criticisms do indeed possess some validity, and for this reason several publications have been dedicated to addressing the limitations and generalizability of the literature related to violent media effects (Anderson et al., 2003; Anderson & Bushman, 1997; Anderson & Carnagey, 2009; Anderson, Lindsay, & Bushman, 1999; Bushman & Anderson, 1998; Bushman, Rothstein, & Anderson, 2010; Gentile & Bushman, 2012; Rothstein & Bushman, 2012). Unfortunately, despite numerous and thorough responses to such criticisms, they are nonetheless repeated throughout this book without acknowledgment of valid responses and solutions to them.

Our review focuses on some of the more prominent but specific criticisms that Ferguson presents. Word limits preclude an in-depth discussion of each described limitation. However, it is worth noting that Ferguson provides few citations in support of his statements. In fact, when research citations are provided, their conclusions are often misrepresented. These

misrepresentations of the media effects literature exist throughout the book and are not limited to those discussed in this review.

Prominent Methodological Criticisms

Psychological studies often possess limitations associated with the research method used. For this reason, scientific consensus regarding effect sizes does not rest on an individual study but on many studies using a variety of methods, a process called *triangulation*. Ferguson describes a number of studies, placing strong emphasis on the limitations of each. Here, we address several of the more common limitations discussed throughout the book.

Aggression measures are invalid.

Ferguson points out that Eron (1963) requested that children's peers nominate each other for aggressiveness, and these nominations were used as the dependent variable measure in his study. A concern is that these nominations could merely reflect a child's popularity rather than his or her actual aggressiveness. However, to suggest, as Ferguson does, that this measure of aggression is invalid relies on the assumption that such ratings possess absolutely no relationship with actual aggression. This is unlikely, given the large number of studies that have used such measures and have shown converging evidence of their validity. Moreover, it is not the only measure of aggression used in media violence research. Other studies measure aggression in the form of administration of unpleasant noises to another person (Carnagey & Anderson, 2005), providing hot sauce to someone known to dislike spicy food (Barlett, Branch, Rodeheffer, & Harris, 2009), or parent-, teacher-, and peer-reported aggression (e.g., Anderson, Gentile, & Buckley, 2007; Krahe, Busching, & Möller, 2012). Some use field observations with children during free play following violent television or video game use (Silvern & Williamson, 1987). All of these methods have been validated in numerous domains, both within and outside of media studies.

Similarly, well-validated objective measures of aggression-related concepts also are commonly used, such as counting the number of fragmented words completed to form aggression-related words (e.g., *explo_e* can be completed to form the word *explode* or *explore*) as a measure of the accessibility of aggressive thoughts (Carnagey & Anderson, 2005), judging the intention of ambiguously aggressive individuals described in a scenario as a measure of hostile attribution bias (Möller & Krahe, 2009), or reporting whether aggressive responses to a provocation example are justified as a measure of normative beliefs about aggression (Möller & Krahe, 2009). Violent media exposure has been found to increase a host of aggression-related measures. If the true effect of violent media on aggression were null, it is extremely unlikely that these findings would surface with such regularity and in such theoretically predicted ways.

Aggression measures are unstandardized.

Another criticism is that measures of aggression are unstandardized. Such freedom in selecting from a variety of dependent variables is said to provide opportunity to "cherry-pick" dependent variables that fit hypotheses while discarding the rest. A favorite target of this criticism is Anderson and Dill's (2000) study in which the violent media and aggression effect was observed for participants' noise duration selections for losing trials of a noise

blast game called Taylor's Competitive Reaction Time Task (TCRTT). What Ferguson fails to mention is that a priori hypotheses were made regarding this particular dependent variable, that other known predictors of physical aggression (sex and trait irritability) also affected only the duration settings, and that numerous subsequent experiments (by several different research teams, including Anderson's) that used all noise settings have replicated Anderson and Dill's original violent video game effect.

In the expansion of this discussion beyond individual studies, it is important to consider the broader implications of the possibility of such widespread dependent variable "cherry-picking" that Ferguson suggests. Specifically, numerous researchers worldwide would have to independently engage in blatant malpractice; all should be insulted by such a charge. It is interesting to note that Ferguson fails to mention that a meta-analysis test of whether violent video game TCRTT experiments yield bigger effect sizes than do experiments using other measures of physical aggression found exactly the opposite, a nonsignificant tendency for TCRTT experiments to yield slightly smaller effects (Anderson et al., 2010). Furthermore, numerous studies have found high internal and external validity for the TCRTT (e.g., Anderson & Bushman, 1997; Carlson, Marcus-Newhall, & Miller, 1989; Giancola & Parrott, 2008; Giancola & Zeichner, 1995).

Demand characteristics influence findings.

Another criticism that Ferguson raises is that demand characteristics may fuel effects in media violence studies. Of course, demand characteristics are a major concern for many research domains, which is why creating effective cover stories, measuring suspicion, and excluding suspicious participants from analyses are common practices (e.g., Saleem, Anderson, & Gentile, 2012). Furthermore, the demand characteristics claim has been addressed many times, with the general conclusion being that, on average, participants prefer to look good rather than to confirm the experimenter's hypothesis. Thus, suspicious participants (e.g., those who believe that their aggressive behavior is being assessed) tend to be relatively unaffected by experimental manipulations and to display abnormally low levels of aggression, (e.g., Anderson et al., 2004; Berkowitz & Donnerstein, 1982; Carlson et al., 1989; Kruglanski, 1975; Weber & Cook, 1972). In other words, transparent cover stories are more likely to reduce violent media effects than they are to increase them.

Using Societal Data as Evidence Against a Psychological Effect

Ferguson notes that societal violence rates are negatively correlated with video game sales, just as pornography sales rates are negatively correlated with rape rates in the United States. Additionally, several countries possess lower crime rates than the United States but also have similar levels of violent media consumption. The logic is that if pornography were to affect rape rates or if violent media were to affect aggression, then these correlations should not exist or should be positive. He states, "Recent suggestions that the decline in societal violence rates is unimportant for the media violence hypothesis are both hypocritical and scientifically lazy" (p. 85). Ferguson also states, "When violent crimes appeared to work in favor of such theories [i.e., media violence effects] they were readily invoked, as late as 2001 (Bushman & Anderson, 2001)" (p. 85).

Ferguson conveniently fails to note two very important facts. First, Bushman and Anderson (2001, p. 478) used the rapid rise in violent crime as “one possible reason for the early interest in a link between media violence and societal violence”; they did not state or imply that violent media caused this increase. Second, in fact, they explicitly stated that “such comparisons of demographic trends are not proof of any causal relationship between violent media and violent crime” (p. 479). There is nothing hypocritical or lazy in noting that societal trends sparked interest in media violence effects while also noting that societal trends do not provide good tests of the causal effects of violent media. Thus, we find Ferguson’s erroneous claims in this regard to be ironic.

Problems inherent in drawing conclusions about psychological-level processes from sociological-level data even have their own name, the *ecological fallacy*. Critically, Ferguson’s claim in 2002 that this correlation is important when one is considering the effect of violent media on aggression rests upon the assumption that violent media must serve as the most important (or at least as a predominant) factor in predicting societal violence. This is simply not the case, and this point was made long ago (e.g., Anderson & Bushman, 2002). In sum, using societal-level data on crime rates to support his argument of no harmful media violence effects represents an impoverished view of the complexity of behaviors such as violence and rape.

Violent Media Effect Sizes Are Small (and Thus Negligible)

Throughout the book, media effects on behavior are described as “small.” However, Ferguson fails to note that these effects are comparable to many others found in social psychology. Notably, they are larger than many others seen in research fields in which societal action follows the release of research findings of small effects such as the effects of asbestos on laryngeal cancer, calcium intake on bone mass, and exposure to lead on IQ scores in children (Bushman & Anderson, 2001). In Table 6.1 (p. 96), Ferguson reports that the influence of violent video games on aggression is $r = 0.04$ (reduced from $r = 0.14$ after inappropriately correcting for publication bias). However, these types of corrections should not be interpreted as a true effect size because they are based on imputed data. Further, if a relationship exists between effect size and sample size (e.g., longitudinal effects tend to be smaller and have larger sample sizes), then this can lead to erroneous adjustments of the size of the effect (Anderson et al., 2010). Still, Ferguson selects the $r = 0.04$ effect size to list rather than the $r = 0.19$ effect size found across 140 studies in Anderson et al.’s 2010 meta-analysis (compared with 21 analyzed in Ferguson, 2007).

It should be noted that the Anderson et al. (2010) meta-analysis publication was available at the time that this book was written, yet Ferguson selects the smallest possible, and inappropriately corrected, effect size located across all meta-analytic publications dedicated to violent video game research. Furthermore, even very small effects (in terms of effect size) can be very important (e.g., Abelson, 1985; Prentice & Miller, 1992; Rosenthal, 1990).

Extreme Violent Acts and Actors

Ferguson describes several cases of extreme violence in which violent media use was presented as a potential contributing factor by news media outlets. Often, these descriptions

are followed with evidence that media did not play a role in the tragedy. We agree that such finger-pointing is often premature and demonstrates a tendency for news media to seek a single cause for behavior. This does not mean that violent media have no influence on aggression. Instead, it is likely the case that such extreme acts of aggression are *multicausal* (Gentile & Bushman, 2012). Further, prominent media researchers have, on several occasions, stated that such cases cannot be considered as strong evidence for violent media effects (e.g., Anderson & Bushman, 2001).

Throughout the book there is skepticism that media effects are relevant to the aggressive behaviors of bona fide criminals. Early studies in the media violence domain have linked media violence consumption to seriously violent behavior, some going back decades (see Anderson et al., 2003). Anderson and Dill's (2000) Study 1 research provides supportive evidence of a strong association between violent video game play and violent behavior, even after controlling for trait aggression. More recently (and after Ferguson's book was published), violent video game exposure was shown to yield significant effects even among clinically violent samples such as youths detained in juvenile justice facilities (DeLisi, Vaughn, Gentile, Anderson, & Shook, 2013). These effects withstood statistical testing of multiple robust confounds that have also been linked to delinquency and violence, including psychopathic personality.

Between these two dates are several studies linking media to violent behavior. Moreover, recent research indicates that media effects "matter" even among highly antisocial individuals and even when controlling for alternative explanations. And these effects matter in a multivariate context, not just at the correlation level. To be sure, more very large-scale longitudinal studies are needed to establish the relative impact of various environmental, biological, and sociological factors on real-world violent behavior, but the claim that there is no valid scientific evidence of a media violence link is simply wrong.

Other Errors, Omissions, and Misrepresentations

In reference to Table 6.1 (p. 96) again, the effect size listed between smoking and lung cancer is 0.90. This figure was likely derived from Block and Crain (2007), in which they stated that Bushman and Anderson (2001) had incorrectly converted the odds ratio presented by Wynder and Graham (1950). Ferguson fails to note that this statement was shown to be incorrect by Bushman and Anderson (2007), who noted that several methods exist for converting odds ratios into Pearson r correlations (e.g., Bonett, 2007), that each method produces values near their original reported effect size (0.40), and that it is unclear how Block and Crain were able to produce an effect size of 0.90.

On pages 116–117, Ferguson states that in studies conducted by Green and Bavelier (2003, 2006, 2007), they randomly assigned participants to play a violent or nonviolent game and observed improvements in attention and visuospatial cognition indices. However, the operational construct in question was the level of action in the video games, not the violent content. That is, there was no test of whether violent content was the active ingredient, rather than speed of action. In contrast, numerous violent video game experiments have shown that violent content is the active ingredient in producing increased physical aggression, but this is not mentioned in the book.

On several occasions throughout the book, Ferguson refers to social-cognitive theories used to understand aggression and media effects as “injection theories” in which “media is injected into passive viewers more or less like a shot” (p. 119). This is simply wrong. Social learning theory, social-cognitive theory, script theory, and the general aggression model all emphasize the active role of the person. Media effects are extremely complex, and no major social-cognitive theoretical model includes this type of “injection” process. There are other examples of factual errors throughout the book, but it would take a much longer review to cover them all.

Conclusions

In past book reviews, Craig A. Anderson has always managed to find something positive to say, even when he found serious problems. The present book presents a real challenge. We can say that the writing style is generally good: engaging, interesting, even convincing if you don't know the relevant empirical and theoretical literatures. This book also can be seen as a primer on how the media industries and their supporters attack legitimate research and researchers, sowing the seeds of doubt about the truths that mainstream researchers around the world have discovered.

Several recent books provide more comprehensive and accurate accounts of media effects research (Dill, 2013; Singer & Singer, 2012; Strasburger, Wilson, & Jordan, 2013). In our view, Ferguson's book manages to subtract from the store of scientific knowledge.

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