Closing the Door on the Media Violence Hypothesis?

A review of

*Grand Theft Childhood: The Surprising Truth About Violent Video Games and What Parents Can Do*

by Lawrence Kutner and Cheryl Olson


*Media Violence and Aggression: Science and Ideology*

by Tom Grimes, James Anderson, and Lori Bergen


Reviewed by

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Back in the 19th century, dime novels and penny dreadfuls were considered a media scourge that would lead youths to violence and rebelliousness and would lead women, who at the time it was thought could not distinguish fact from fiction, to idle fantasy. Throughout the 20th century, panics have arisen over the supposed deleterious effects of everything from jazz music to comic books to Dungeons and Dragons to Harry Potter. In retrospect we can look back and see that none of these media forms sparked off waves of violence, insanity, or Satanism. Indeed, we may chuckle at the quaint ideas of those long-dead people who thought that books now regularly assigned in high school literature classes may have sent youth spiraling off into madness or psychopathy.

In reality, of course, we have merely replaced the folk-devils of the past with new folk-devils—television and particularly video games in recent decades. As happened generations ago, obliging social scientists who are part of the “elder” establishment produce “research” in support of these panics. Yet does this research really hold up to the exaggerated claims of certainty that have stretched the limits of conclusiveness normally available to social scientists?

Two new books say “No,” that in fact the media violence hypothesis, which currently enjoys darling status in psychology particularly, is just another turn of the moral panic wheel. *Grand Theft Childhood: The Surprising Truth About Violent Video Games and What Parents Can Do* focuses on video games specifically, whereas *Media Violence and Aggression: Science and Ideology* focuses more broadly on research on all media. Each of these books should be considered critical reading for social scientists. Neither book is without its flaws, but they do point to an ideology within the social sciences that has subsisted on poor data and misleading statements, and is in need of a course correction if psychology is to retain any future credibility. I'll consider the books individually in the following paragraphs.

Of the two books *Grand Theft Childhood* has certainly generated the most attention, coming as it did at about the same time as the release of the video game *Grand Theft Auto IV* and precisely because it flies in the face of the public hysteria (or at least the hysteria common among society's elders) over video games. *Grand Theft Childhood* is written with nonacademic audiences in mind, and so as a typical trade book, it makes for pleasant reading on a controversial topic. The book makes several valuable contributions to the video-game literature.

First, the book points out how the current furor over video-game violence mirrors past moral panic cycles over various media forms. This is particularly enlightening in view of the fact that violent crimes among juveniles have plummeted in the United States, even as violent video games have skyrocketed in popularity. Some antigame scholars claim that this doesn't matter, but this claim is an exercise in ideologically driven scientific laziness. It would be akin to worrying about global warming while we enter a new ice age. The history of media-focused moral panics, as well as social science's misguided role in them, forms an important discussion that so far has simply not taken place.
Second, *Grand Theft Childhood* does a nice job of elucidating the vast methodological and theoretical weaknesses of the majority of video-game research. These weaknesses include, but are not limited to, the use of invalid measures of aggression, inconsistent or even null results being reported as positive, cherry-picking of hypothesis-supporting data, cynical attempts to compare video-game research effect sizes with those found in medical science (using flawed statistics), and failure to control for relevant “third” variables. These problems certainly have been identified elsewhere for studies of both video games and television violence (Ferguson, 2007; Jensen, 2001; Savage & Yancey, 2008).

Yet essentially the same flaws have persisted in the media violence field, with little effort at correction, for decades. As I would argue that most social scientists, and certainly almost all nonscientists, are not aware of how media violence studies are conducted, a discussion of methodology and its limitation is long overdue. For example, media violence studies, when reported in the news media, have simply stated something along the lines of “Playing video games was found to cause aggression,” neglecting to add that aggression was measured, perhaps, by filling in the missing letters of words (filling in kn**e** as *knife*, say, rather than *knave* as an indicator of aggression), completing story stems, or delivering nonpainful noise bursts to a consensual reaction-time game opponent.

Unfortunately, psychologists, rather than educating the public on the limitations of research or taking a limited and objective stance, have glossed over (I’d argue purposefully) the inconvenient details in order to sell an ideological message (although this gets into the second book’s arguments). I suspect that most people in the general public consider *aggression* to mean, at a minimum, yelling, kicking, hitting, insulting, and so forth, and would be shocked to learn that these behaviors are seldom examined (Savage & Yancey, 2008). The authors of *Grand Theft Childhood* do a deft, dispassionate job of covering this issue. Although the antimedia scholars are likely to howl at this unburying of the skeletons, Kutner and Olson are quite astute in their observations.

Why hasn’t psychology as a discipline policed research about media violence effects more diligently? I suspect that the observations that Kutner and Olson make about media violence research and methodology could easily be transferred to many other areas of psychological research that rely on ad hoc measures of behavior (that is, measurements not of the behavior itself but of something else entirely, which the authors cleverly argue is close enough). I suspect that this is one critical reason that psychology continues to embrace the highly flawed media violence research as a core belief—to acknowledge the flaws in this area, in which psychology has placed so much status, would be to acknowledge the vast flaws that are endemic to psychological research on a broad array of topics. The “hard” scientists would be right after all—that psychology is little more than “opinions with numbers.” Perhaps it may be that professional psychological organizations are not able to objectively review the research on media violence (in the recent American Psychological Association review on video games, the committee was made up solely of antigame scholars reviewing their own work and ignoring that of skeptics; naturally they declared it
conclusive), as the flaws in this one area could be considered pervasive throughout the entire discipline.

Kutner and Olson also include data from one of their own studies in *Grand Theft Childhood*. This study, funded by the Department of Justice, examined video-game playing habits (particularly playing M-rated games specifically) in relation to a variety of aggression-related outcomes. Results were inconclusive overall, with effects in both directions. Playing M-rated games weakly correlated with increased bullying behaviors but also decreased bullying victimization. On the other hand, kids who played no video games at all were the most aggressive. Many of the comparisons were, in fact, null results or very small in size, but at least the authors are savvy enough to acknowledge that this mixed bag (common in media violence research) summed up to very little. Indeed, the authors concluded that video-game exposure produced little if any meaningful effects on aggressive behavior. As the authors did not control for family violence exposure, even these effects may be spurious.

This study also doesn't advance a central issue left unresolved in media effects literature—namely, even if a media form does increase aggression, is this necessarily a bad thing? Arguably, aggression in moderate amounts is highly adaptive. Unfortunately, most media effects studies, this one included, fail to find any “clinical” cutoffs for aggression that indicate pathological aggression. It is left implied that all aggression is bad, which is probably a naïve view.

A more controversial finding from the Kutner and Olson study is that many boys report using violent games to reduce aggression. This necessarily raises the question of which outcome can be believed. Naturally, antimedia scholars will question whether boys are insightful enough to report on their own aggression. Despite the fact that this view is both self-serving and patronizing, it nonetheless is not without merit. However, the only alternative is the kind of behavioral outcome measures that Kutner and Olson already demonstrated are not valid. So who is to be believed—the self-report of boys or adamantly antimedia scholars using invalid measures of aggression? I think more discussion of this issue is warranted and has not yet appeared in the media violence literature. Those quibbles aside, *Grand Theft Childhood* is certainly one of the best and most insightful books on the market today about violent video-game effects.

Whereas *Grand Theft Childhood* may be viewed as a nuts-and-bolts guide for parents, *Media Violence and Aggression* is clearly meant for academic audiences with a deeper look at the ideological and theoretical dogmas upon which the media violence hypothesis subsists. If *Grand Theft Childhood* could possibly be dismissed as a nonacademic book for parents (although I'd not be quick to do so), *Media Violence and Aggression* is a thoughtful and sophisticated work that dismantles the core assumptions of the media violence hypothesis piece by piece. There is some overlap with *Grand Theft Childhood*, such as a summary of the historical patterns of antimedia moral panics and a discussion of the
methodological problems of media violence research, although greater attention is focused on theory and politics.

This book makes several core contributions to the discussion on media violence effects above those seen in other critical works. First, *Media Violence and Aggression* treats the media violence hypothesis itself as a sociological phenomenon and is right to do so. Understanding the sociological and political structures that support the framework of the media violence hypothesis dismantles its self-made story as an objective scientific “fact.” Such a certain claim toward fact or “law” status would have been gutsy for physicists to make; as such, the ability of the media violence hypothesis to subsist despite weak supporting data has been remarkable. Understanding it from a sociological perspective helps us understand how this belief has persisted, not just recently, but intergenerationally, despite little real-world data supporting that media have any effect on violent crime whatsoever.

The second unique contribution of this book is the effective dismantling of the convergence argument. Briefly, this argument has suggested that, despite significant flaws in individual media violence research studies, *given that* the convergence of data supports the media violence hypothesis, then the hypothesis must be true. Of course, one might note that the “given that” in this statement is under debate. In fact, previous scholars have identified that media violence is far less consistent than antimedia scholars tend to suggest (Freedman, 2002; Savage, 2004). Perhaps most famously, the American Academy of Pediatrics's claim of 3,000 overall studies with only 18 null findings was found to be wildly off the mark, with, in fact, only about 200 studies about evenly mixed (Ferguson, in press).

Yet, even if we ignore that inconvenience, does a convergence of poorly designed, invalidly measured, ideologically biased research demonstrating weak effects truly add up to fact? Put another way, if you pile numerous pieces of junk on top of one another, when does it add up to anything other than a pile of junk? This, as pointed out in *Media Violence and Aggression*, is a serious error in logic of the convergence argument. It is, in the end, an argument in support of a particular ideology, not one in support of fact.

*Media Violence and Aggression* is certainly the denser book, which is, of course, both good and bad, making it simultaneously more thoughtful and more of an academic (and for lack of a better way of putting it, less fun) read. *Media Violence and Aggression* is certainly as pleasant a read as any academic book, better probably than most; it's just that as a trade book for general audiences *Grand Theft Childhood* has the clear advantage in this realm. *Media Violence and Aggression*’s heavy emphasis on theory and philosophy may be difficult to wade through for some, particularly those like me who remain skeptical of most psychological theorizing. The authors are quite clear in pointing out that media violence theories are largely outdated, particularly in their avoidance of non–learning-based contributors to aggression such as genetics or even agency. (Yes, I know some media violence theories pay lip-service to “innate” tendencies at least, but it's usually just that, lip-service.)
Nonetheless, the pages and pages of theory, both in discussing antimedia theory and alternative philosophical views, can leave one shaking one's head, rueing psychology's love of theory over substance. Indeed, antimedia theories become simultaneously so complex and illusory that they become impossible to falsify, which is why statements suggesting that the precipitous decline in violence in the United States and other nations is unimportant are treated seriously rather than as the pseudoscience that they are.

At the same time Grimes, Anderson, and Bergen suggest that perhaps a corner has been turned; more scholars are speaking out against the media violence hypothesis, and much of the public remains healthily skeptical. As the current generation of media-savvy youth grows up to become researchers themselves, it is likely that much of the hysteria about television and video games will fade, the science rightfully identified as ideology rather than fact. Of course, by then, we'll have some new fing-fangled media to get our knickers all in a bunch.

References


