

Review of *Grand Theft Childhood:*
The Surprising Truth about Violent Video Games and What Parents Can Do
by Lawrence Kutner, Ph.D. and Cheryl K. Olson, Sc.D.

Kutner and Olson, who are co-founders and directors of the Harvard Medical School Center for Mental Health and Media and Board Members of the Phillip Morris USA Parent Resource Center, make the following statements in the Preface to their book: “We did not set out to prove anything about video games. We have no vested interests for or against them” (p. 2). The authors’ professed lack of bias is called into question by their research design and how they interpret their own data. They report the findings of their \$1.5 million research grant from the Justice Department, in which they asked middle school children to report, among other things, on their own video game habits and their own problematic behaviors. They then compared kids who played M-rated (extremely violent) games a lot to those who played them less or not at all. They report that among both boys and girls, M-Gamers were significantly more likely to have been in a physical fight, to have hit or beat up someone, to have damaged property just for fun, and to have gotten into trouble with the teacher or principal. They state:

We found significant relationships between M-rated game play and a broad range of aggressive or problem behaviors among middle school students . . . What’s more, in most cases the odds of engaging in these behaviors at least once during the previous year increased with the relative “dose” of M-rated game exposure: the more M games on the children’s lists, the greater the relationship (p. 99).

They also state:

We found that boys [and girls] who regularly played at least one M-rated game had significantly lower belief in the use of nonviolent strategies and significantly more positive perceptions of aggression (p. 105).

Should parents be concerned? Not really, conclude the authors. They say these data are correlational only, and it may be that kids who already have these problems are more attracted to violent video games or that a third factor may be causing the relationship. Although this is true, the correlations are consistent with the notion that the games promote aggression and are inconsistent with the notion that the games provide benefits for kids, as the authors imply.

It is true that you can’t make a causal claim out of correlational data, so the obvious question is, why did they set up the study this way if they were going to conclude that any relationship they found was meaningless? If they wanted to argue for or against a causal claim, they needed to do an experiment, or they could have set up a longitudinal study in which they surveyed kids twice. They could have measured other background variables (such as family characteristics or early experiences) that might account for both media habits and child problems. Then, if they controlled for these background variables and child problems at Time 1, and they still found the relationship between game play at Time 1 and problems at Time 2, they could have talked about potential causal relationships. But these researchers didn’t design their study with that in mind (even though their funding was sizeable enough), so instead, they dismissed

these correlations as revealing no problems.

The authors acknowledge the help of a Congressman in connecting them with the Justice Department (p. 3). This leads me to suspect that their grant was an “earmark” and not competitively reviewed. Having served as a reviewer for proposals for the National Institute of Mental Health and the National Science Foundation, my opinion is that the design of their research would not have passed muster in competition.

Following up on the correlational data, the authors make the following “reassuring” comments:

It’s also important to note that the problems we studied are common among teens. For example, over half of boys and one third of girls in our sample had hit or beaten up someone at least once during the previous year. This doesn’t mean they are bad kids or are likely to be violent adults (p. 101).

They also argue:

Although boys and girls who play electronic games a lot (in hours per week and days per week) are significantly more likely to bully others, it’s important to know that *most children who play these games are not bullies* (p. 102, authors’ italics).

These two arguments are self-contradictory because in one, the relationships are nothing to worry about because the problematic behaviors are common, and in the other, we should not worry because the offending behaviors are rare.

While downplaying the implications of their correlational data, Kutner and Olson emphasize the data they believe makes their work a “landmark” study: They interviewed the kids themselves and asked them why they play videogames. While there’s nothing wrong with soliciting kids’ opinions, researchers must recognize the self-serving bias that is at play when someone is asked about his or her own behavior. Many of these children reported that there were no negative effects on themselves. Some even said that violent video games helped them reduce stress and made them feel less aggressive afterwards. Leaving aside the long record of peer-reviewed research that shows that this so-called “catharsis” effect from media violence does not happen, it doesn’t take a research expert to have doubts about a child’s willingness to admit that something he or she loves to do is harmful. Do these authors think that, to obtain the best scientific evidence, we should be asking kids whether junk food harms them? Should we be asking kids whether staying up late on school nights interferes with their academic performance?

Based on their own data, the authors have the gall to say that *not* playing (violent) video games may be psychologically damaging. They base this on the fact that M-Gamers are more likely than others to play video games with other kids. From this they conclude (based on no evidence) that kids who don’t have this experience are losing out on the development of social skills. They even argue that the Virginia Tech shooter’s problems may have been due to his *not* playing video games!

In fact his [Cho's] lack of involvement with video games was a marker of his poor social skills and his status as a social outcast (p. 198).

Kutner and Olson's most vociferous argument amounts to setting up and then knocking down a straw man. They argue that violent video games are not responsible for school shootings, and that the games are being used as a scapegoat. They argue that there are more important factors that lead to youth violence, and that by focusing on video games, we miss the big picture. However, most responsible researchers don't accuse video games of being *the* cause of youth violence. They say that violent video games are a contributing factor not only to youth violence but to a variety of outcomes such as desensitization to violence, increased hostility, and greater acceptance of violence, which are unhealthy outcomes whether they lead to violence or not. Being honest about these findings is not to excuse the other societal and family factors that encourage kids to become violent. The scapegoat argument is like saying that doctors who rail against feeding kids junk food are scapegoating the food industry, when we know that the lack of exercise is a more important problem (or vice versa). Both a lack of exercise and poor nutritional habits contribute to obesity, and reporting on research in one area does not imply that the other area is not important.

When the authors ask why there is no consensus among the experts (pp. 58-59), they cite four people. The two on their side are paid consultants for the toy and game industries (Jeffrey Goldstein and Mark Griffiths). The other two, who are on the opposing side, are not. Academic researchers have no incentive to fudge their data. Their grants are awarded based on the soundness of their research designs, including their ability to either verify or *falsify* their hypotheses. And their careers are rewarded based on whether their findings hold up over time when other researchers attempt to replicate their findings. On the other hand, researchers receiving industry support are rewarded for findings that are in line with the industry's interests.

I am puzzled why these researchers should have such a biased take on their findings, twisting their reasoning so hard to make violent video games seem not only harmless, but even beneficial to kids. In interviews, they have assured the public that they receive no remuneration from video game companies. It does seem more than coincidental that a book named *Grand Theft Childhood* telling parents not to worry about violent video games should be launched in the same month as a game titled *Grand Theft Auto IV*. Maybe there was no collusion; maybe it just seemed like a good marketing idea to the publisher. It's a misleading title anyway, because it seems to imply criticism of the games rather than criticism of researchers whose findings suggest harm to kids.

This book does a disservice to parents who want to know if a heavy dose of violent video game play is healthy or unhealthy for their children. Any nonbiased observer of the new data in this book and the literature as a whole would urge caution. By promoting violent game playing and downplaying the risks, the book also gives unwarranted (and unneeded) support to the producers and sellers of these games.

For a review of the findings of research on video games from the pre-eminent academic expert, I recommend “FAQs on Violent Video Games” from Professor Craig A. Anderson’s web site: http://www.psychology.iastate.edu/faculty/caa/Video_Game_FAQs.html

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