

Weak Results, Misleading Conclusions

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Review of the article "Longitudinal Effects of Violent Video Games on Aggression in Japan and the United States" [Pediatrics](#) 2008; 122: e1067-e1072

The Anderson et al., paper is on an interesting topic. Unfortunately there are numerous flaws in the literature review, methodology and conclusions that greatly reduce my enthusiasm for it, and call into question the meaningfulness of the study.

In the literature review the authors suggest that research on video game violence is consistent when this is hardly the case. The authors here simply ignore a wide body of research which conflicts with their views. A bibliography of research studies finding either null results for video game violence or results that suggest that violent game play reduces aggression is appended to this review.

The authors fail to control for relevant "third" variables that could easily explain the weak correlations that they find. Family violence exposure for instance, peer group influences, certainly genetic influences on aggressive behavior are just a few relevant variables that ought either be controlled or at minimum acknowledged as alternate causal agents for (very small) link between video games and aggression.

Overall results are very weak with effect sizes ranging from (.07 to .15). Video game exposure overlapped in this study approximately half a percent to 2% with the variance in aggression, which is as close to zero as one can get without being zero. If anything it is remarkable how little effect that violent games had on trait aggression, considering that other relevant variables were not controlled. Likely if other variables had been better controlled, such small effects may have vanished.

Lastly the authors link their results to youth violence in ways that are misleading and irresponsible. The authors do not measure youth violence in their study. The Buss Aggression measure is not a violence measure, nor does it even measure pathological aggression. Rather this measure asks for hypothetical responses to potential aggressive situations, not actual aggressive behaviors. In the paragraph beginning "youth violence is a public..." the authors appear to generalize their results to youth violence, but offer no compelling reason why this should be, particularly in light of the weak results they achieve. The authors also fail to note that during the period in which violent video games became increasingly popular, youth violence has plummeted approximately 66% to levels not seen since the 1960s (childstats.gov, 2008; FBI, 1951- 2007). Although I suspect the authors would simply try to argue that this does not matter, such arguments are disingenuous, particularly as they raise the issue of youth violence themselves.

In short, given the weak effect sizes, the lack of control of relevant variables, and the failure of the authors to acknowledge data and research which contradicts their hypothesis, I am left with little confidence that the results of the current study provides much meaningful information on the impact of violent games.

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