

Real Violence Versus Imaginary Guns

Why Reframing the Debate on Video Game Violence is Necessary

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The debate on whether video game violence does or does not have an influence on players remains a heated one in the general public and among scholars. Naturally, we see one influence the other. For instance, after the 2018 Parkland shooting in the United States, President Trump initially invoked video games as a cause. Some Republican politicians and Trump administration officials invoked statements by the American Psychological Association linking video games to aggression, although his administration later backed down from such claims after official hearings during which evidence was prevented.¹ Many of the misunderstandings regarding the current nature and strength of video game violence research come from the difficulty in distinguishing violence from aggression. At the same time, scholars may inadvertently miscommunicate or fail to recognize the weaknesses within aggression research, eliciting/generating more confidence about the research on violent video games in the general public than is warranted from current data. At this juncture, the weaknesses of aggression research are well known. Current controversies now focus on the use and misuse of meta-analysis, the related issue of psychology's "crud factor," and the misuse of near-zero effect sizes. In this essay, I will briefly summarize the evidence for effects of games on violence (which society cares about). I will then spend more time focusing on the effects of games on prank-level aggression

1 School Safety Commission: "Final Report of the Federal Commission on School Safety" (2018). <https://www2.ed.gov/documents/school-safety/school-safety-report.pdf>, retrieved April 16, 2019.

(which society arguably does not care about), including “crud factor” results, misuse of meta-analysis, and “death by press release.” I will conclude by observing that psychological science has gradually reduced the standard of evidence for the link between games and aggression over the course of 20 years, arguably in a defensive reaction to preserve the ostensible value of psychological science itself.

THE EVIDENCE REGARDING VIOLENT CRIME

Space constraints preclude an exhaustive summary of this data, but several pools of evidence highlight an increasingly clear lack of evidence for an impact of violent games on societal violence, ranging from mild bullying behaviors all the way to mass shootings. This evidence comes from several sources, none of them perfect, but all pointing in the same direction. These include:

Inverse Correlation Between Violent Video Game Sales and Violence.

Most of the data in this realm comes from the US, where the inverse relationship between violent video game sales and significant reductions in youth violence, homicides, and other outcomes is clear and has been known for some time.² Such correlational data must be interpreted with caution, given the potential for ecological fallacies. However, other data does suggest that the release of very popular violent games is associated with immediate declines in crime.³

Little Evidence that Mass Homicide Perpetrators Consume High Amounts of Violent Media Including Games. This particular pool of evidence dates as far back as 2002 with a US Secret Service report that noted that school shooters tend to consume less rather than more violent media than the amount expected for males of their age group.⁴

2 Ferguson, Christopher J: “Does Media Violence Predict Societal Violence? It Depends on What You Look at and When,” in: *Journal of Communication* 65 (2015), pp. E1–E22.

3 Markey, Patrick M./Markey, Charlotte/French, Juliana: “Violent Video Games and Real-World Violence: Rhetoric versus Data,” in: *Psychology of Popular Media Culture* 4 (2015), pp. 277–295. McCaffry, Kevin/Proctor, Ryan: “Cocooned from Crime: The Relationship Between Video Games and Crime,” in: *Social Science and Public Policy*, In Press.

4 United States Secret Service and Department of Education: “The Final Report and Findings of the Safe School Initiative: Implications for the Prevention of School Attacks in the United States (July 2004).

Cross-National Comparisons Find High Game-Consuming Countries are Low Violent Crime Countries. The first analyses along these lines came from the Washington Post following the 2012 Sandy Hook shooting, but a recent update with Patrick Markey confirmed these conclusions.⁵ Essentially, high game-consuming countries such as Japan, South Korea, and the Netherlands are among the least violent on the planet.

All the above data are societal in nature. Intriguingly, the psychological research field has seldom engaged with this societal-level data and, for the most part, ignores its existence. Unfortunately, this creates a situation in which the psychological science remains largely divorced from the real world. To the extent that psychological research may disagree with the real world (although that itself is a matter of interpretation, as I will show in a moment), scholars may often come across as implying that the real world is less important than what happens in psychological laboratories.

THE EVIDENCE FROM PSYCHOLOGICAL STUDIES

Psychological studies can be either experimental or correlational/longitudinal. We'll consider each in turn.

Experimental Studies. Experimental studies of violent video game effects typically take individuals (often, though not always college students) and randomize them to play violent or non-violent games in an artificial, laboratory setting. So long as the games are equal on all levels other than violence, this provides an argument for causal effects. Because it would be unethical or even illegal to cause individuals to behave violently, the aggression measures are, by nature, prank-level aggression—such as giving someone hot sauce when they do not like spicy food or putting someone's hand in a bucket of ice water. Such measures can certainly be interesting, though likely tell us little about violent crime. Nevertheless, this pool of studies has been known to suffer from a number of flaws.

Publication Bias. First, it is now well-understood that experimental studies of video games and aggression suffer from publication bias and, when such bias

<https://www2.ed.gov/admins/lead/safety/preventingattacksreport.pdf>, retrieved April 16, 2019.

5 Markey, Patrick M/Ferguson, Christopher J: *Moral Combat: Why the War on Violent Video Games is Wrong*, Dallas, TX: BenBella Books 2017.

is controlled for, effects drop pretty close to zero.⁶ Further, more recent preregistered studies⁷ of violent game effects have returned non-significant findings.⁸ Thus, despite some claims to the contrary, it is not clear that experimental studies of violent game effects have provided evidence for causal effects.

Poor Matching of Video Game Conditions. For about a decade, it has been understood that a common confound of video game experiments has been a failure to match video games carefully on factors other than violent content.⁹ Other factors such as game difficulty, frustration, and competition may differ systematically between mainly violent and non-violent games, introducing critical confounds.

Use of Unstandardized Aggression Measures. Unstandardized aggression measures allow for researchers to pick and choose outcomes that fit their hypotheses while ignoring those that do not. It has been demonstrated that such unstandardized aggression measures result in upwardly biased effect size estimates.¹⁰

Demand Characteristics. In many designs, the close pairing of the game condition with measures of aggressiveness makes the study hypotheses obvious. Under such conditions, participants may be able to guess the study hypotheses and change their behavior accordingly.

6 Hilgard, J/Engelhardt, CR/Rouder, JN: "Overstated evidence for short-term effects of violent games on affect and behavior: A reanalysis of Anderson et al (2010)," in: *Psychological Bulletin* 143 (2017), pp. 757-774.

7 i.e. Those in which the analysis plan and hypotheses are published in advance of data collection to reduce questionable researcher practices.

8 Ferguson, CJ/Trigani, B/Pilato, S.et al.: "Violent video games don't increase hostility in teens, but they do stress girls out," in: *Psychiatric Quarterly* 87 (2016), pp. 49-56.
McCarthy, RJ/Coley, SL/Wagner, MF/Zengel, B/ Basham, A.: "Does playing video games with violent content temporarily increase aggressive inclinations? A pre-registered experimental study," in: *Journal of Experimental Social Psychology* 67 (2016), pp. 13-19.

9 Adachi, PJC/Willoughby, T.: "The effect of violent video games on aggression: Is it more than just the violence?" in: *Aggression and Violent Behavior* 16 (2011), pp. 55-62.

10 Elson, M/Mohseni, MR/Breuer, J.et al.: "Press CRTT to measure aggressive behavior: The unstandardized use of the competitive reaction time task in aggression research," in: *Psychological Assessment* 26 (2014), pp. 419-432.

CORRELATIONAL/LONGITUDINAL DESIGNS

Correlational and Longitudinal designs do not control video game exposure, thus limiting causal attributions, but do allow for the assessment of more serious aggression or violent behavior. However, they too are known to experience a number of critical issues.

Failure to Adequately Control for Relevant Variables in Longitudinal and Correlational Studies. Many studies fail to control for important variables that may explain links between violent games and aggressiveness, ranging from gender to trait aggression to genetics. Studies that control for such variables suggest that actual socialization effects for violent games (or other media) are minimal.¹¹

Unstandardized Self-Report Measures. As with experimental studies, many correlational and longitudinal studies use poorly-designed self-report measures. This problem is compounded by their self-report nature. Most studies do not include checks for unreliable or mischievous responding, both of which can cause spurious correlations.

Demand Characteristics. As with experimental studies, the close pairing of questions about video games with measures of aggression or violence (or worse still, asking participants to rate the violent content of the games they play) create significant demand characteristics and potential spurious positive results.

Researcher Expectancy Effects. One curious effect that has been observed is the presence of researcher expectancy effects. In particular, it has been observed that studies that employ citation bias (citing only studies favorable to the authors' personal views) tend to have higher effect sizes than those with more balanced literature reviews.¹² As with experimental studies, preregistration can help remove some researcher expectancy effects. Thus far, preregistered correlational studies, as with experimental studies, have not been encouraging of violent game effects¹³—aside from one study with college students.¹⁴

11 Schwartz, JA/Beaver KM: "Revisiting the association between television viewing in adolescence and contact with the criminal justice system in adulthood," in: *Journal of Interpersonal Violence* 31 (2016), pp. 2387-2411.

12 Ferguson, CJ: "Do angry birds make for angry children? A meta-analysis of video game influences on children's and adolescents' aggression, mental health, prosocial behavior, and academic performance." *Perspectives on Psychological Science* 10 (2015), pp. 646-666.

13 Przybylski, A./Weinstein, N.: "Violent video game engagement is not associated with adolescents' aggressive behaviour: evidence from a registered report," in: *Royal*

To conclude this section on research from psychological studies, the data from nearly four decades worth of research is, on balance, not impressive for violent game effects. Nonetheless, it remains common to find a few scholars defending the potential for effects. These defenses are undoubtedly in good faith, but include critical errors in thought. Namely, these include the misuse of meta-analysis, as well as declining standards of evidence wherein ever smaller, close-to-zero “crud factor” effect sizes are considered “evidence” for effects, despite many reasons to suspect that such tiny effect sizes do not represent population level effects. It is to these issues I now turn.

ON THE MISUSE OF META-ANALYSES

It has become something of an unfortunate tradition in the social sciences that, when individual research studies disagree regarding support for a hypothesis, meta-analyses are summoned as a djinni to fix the problem via a magical wish. Unfortunately, meta-analyses only function well in this regard when considering a homogeneous pool of randomized controlled trials. For messy social science studies with unstandardized measures, poor control condition contrasts, researcher expectancy effects, and the like, we can be certain that the pooled average effect size is *not* a remotely precise measure of a population effect size. Put simply, meta-analyses can tell us which foibles of research methodology are associated with higher or lower effects, but they cannot tell us what the *true* effects are. Nonetheless, many scholars persist in such a belief.

As an example, the American Psychological Association relied on meta-analysis in its technical report on video game violence.¹⁵ From a field that, during the time frame considered, likely included 60-70 empirical studies, the APA included only 18. Puzzlingly, 5 of these contained no data relevant to the

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<https://royalsocietypublishing.org/doi/10.1098/rsos.171474>

14 Ivory, AH/Ivory JD/Lanier M: “Video Game Use as Risk Exposure, Protective Incapacitation, or Inconsequential Activity among University Students: Comparing Approaches in a Unique Risk Environment,” in: *Journal of Media Psychology* 29 (2018), pp. 42–53.

15 American Psychological Association: “APA review confirms link between playing violent video games and aggression” (2015). Apa.org

<https://www.apa.org/news/press/releases/2015/08/violent-video-games.aspx>, retrieved April 16, 2019.

question of whether violent games cause aggression, lacking either aggression measures or contrasts between violent and non-violent games. Thus, it is unclear how the APA task force extracted effect sizes from these studies. But the task force's failure to consider the impact of the methodological issues discussed earlier, as well as their overreliance on spuriously high bivariate effects from correlational and longitudinal studies, result in pooled effect size estimates that assuredly bear little resemblance to population level effects.

At least for video game violence, and likely for many other research fields as well, it is likely time to abandon the belief that meta-analyses are debate enders, or that the pooled mean effect size is meaningful. Such pooled mean effect sizes, capitalizing on elevated power, are almost always "statistically significant," (which is to say they cross an arbitrary line that suggests results aren't due only to random chance in the selection of samples from a population) causing scholars to have overconfidence in the strength of evidence for effects, despite weak effect sizes (more on this in a moment). This is not to say meta-analyses are without value: as indicated above, they can actually be quite informative in understanding *why* effect sizes are elevated in some studies and lowered in others. But they seldom tell us what the *true* population effect size is.

PSYCHOLOGY'S CRUD FACTOR

The concept of "crud" factor was described by psychologist Paul Meehl to refer to the observation that almost everything correlates just a little bit with almost everything else, but that these tiny correlations should not be interpreted as meaningful.¹⁶ Unfortunately, as sample sizes increase (normally a good thing), these tiny effect sizes can pop out as "statistically significant" even though they are crud. This is an easy issue for scholars to lose sight of, considering that many are inherently excited (or biased) to find "statistically significant" results and loathe to embrace the null. This crud factor can cause scholars to make bad decisions regarding the interpretation of crud-level findings as meaningful.

Orben and Przybylski recently demonstrated this with statistically significant (but trivial) relationships between screen use and mental health. The authors compared these to statistically significant effects of similar magnitude for

16 Meehl Paul: "Why summaries of research on psychological theories are often uninterpretable," in: R. E. Snow/D. E. Wiley (Eds.), *Improving inquiry in social science: A volume in honor of Lee J. Cronbach Hillsdale, NJ: Erlbaum 1991*, pp. 13–59.

obviously irrelevant factors such as eating potatoes or wearing eyeglasses on mental health. If the magnitude of screen use is similar to potatoes on mental health, such correlations should clearly be dismissed as nonsense even if “statistically significant.”¹⁷

Most meta-analyses of video game effects find effect sizes in the range of $r = .04$ to $.08$, particularly for longitudinal studies.¹⁸ But what are we to make of effect sizes in such a range even when “statistically significant”? Such effect sizes are no different in magnitude than the effect of potatoes on suicide. Thus, is the overinterpretation of such effect sizes and indication of the crud factor or what we might also call the “suicide potato effect”?

The naïve interpretation of such effects is demonstrated by one recent meta-analysis by Prescott and colleagues.¹⁹ The meta-analysis found a best-controlled effect size estimate of $r = .078$ for longitudinal studies of video game violence. But is such an effect a reasonable indication of population effect sizes or consistency between studies as the authors claimed? It seems doubtful this is the case. First, given that such an effect size is near zero, it would best be interpreted that most studies find an effect size that is little different from zero. Second, this effect size is based on self-report surveys, many of which suffered from the methodological limitations indicated above. As such, there are good reasons to conclude that even this effect size is upwardly biased. Third, taken at face value, this effect size indicates that the ability of knowing a person’s video game habits when predicting their aggression is 0.61% shared variance, essentially only 0.61% better than a coin toss. Fourth, at least two of the effect sizes calculated from my own studies in the Prescott meta-analysis appear to be upwardly biased miscalculations, thus raising the possibility that even this effect size estimate is too high. On balance, the Prescott meta-analysis is better

17 Orben, A/Przybylski, A: “The association between adolescent well-being and digital technology use,” in: *Nature: Human Behavior* 3 (2019), pp. 173-182.

18 Ferguson, CJ.: “Do angry birds make for angry children? A meta-analysis of video game influences on children’s and adolescents’ aggression, mental health, prosocial behavior, and academic performance,” in: *Perspectives on Psychological Science* 10 (2015), pp. 646-666. Furuya-Kanamori, L/Doi, Sohail: “Angry birds, angry children, and angry meta-analysts: A reanalysis,” in: *Perspectives on Psychological Science* 11(2016), pp. 408-414. Prescott, AT/Sargent, JD/Hull, JG.: “Metaanalysis of the relationship between violent video game play and physical aggression over time,” in: *PNAS Proceedings of the National Academy of Sciences of the United States of America* 115 (2018), pp. 9882-9888.

19 Ibid.

evidence against violent video game effects than for it. Only a decision to ignore the crud factor leads one to suggest otherwise.

Relying on such miniscule effect sizes to support a hypothesis is a statistical grasping at straws. Over time, the standards of evidence for this field considered sufficient for scholars to claim that evidence supports effects has gradually diminished. Just over a decade ago, scholars assured us that the effects were similar in magnitude to smoking and lung cancer with perhaps 10-30% of the variance on aggression and violence attributable to video game and other media violence.²⁰ Now, without the slightest hint of embarrassment, our field is reduced to arguing whether 0.61% shared variance is enough to ring the clarion bells of alarm in the general public. If this is all our field has to show for itself, it is time to pack it in or settle for being “that nasty little subject”²¹ William James once repudiated psychology for being.

CONCLUDING THOUGHTS

Considering all of the above, I argue that it is time to reframe the debate away from the notion of the effects games have on people—a line of research that has seldom borne fruit. Rather, it may be helpful to understand the interactions between games and players, their motivations for playing action-oriented games, and how such game play can be understood in the context of a greater milieu of a given individual’s life. In essence, I argue for an abandonment of the entire moral enterprise of blaming games, violent or otherwise, for negative outcomes and, instead, treating them more or less like any other hobby or, alternatively, cultural experience. I note this also means that we ought to be cautious in exaggerating positive as much as the negative impacts. But I think that removing games research from negative effects and, quite frankly, cultural criticism, would be beneficial to the objectivity of games research.

To this end, I found reasons for optimism among the other sessions at the Young Academics Workshop. Many of these sessions demonstrated the potential for a sophisticated inquiry into games and player experiences that eschewed the easy moralization of the “blame games” movement. I think a fundamental aspect of this optimism came from a degree of respect shown to gamers themselves and

20 Strasburger, Victor: “Go ahead punk, make my day: It’s time for pediatricians to take action against media violence,” in: *Pediatrics* 119 (2007), pp. e1398-e1399.

21 James H. (Ed.). *The letters of William James*. Boston, MA: Atlantic Monthly Press 1920.

gamer culture. Too often, gamer culture appears to be an easy target for stigmatization, whether through the earlier paradigm of social psychologists or more recently through cultural criticism. Divorcing the science of games from moral posturing is essential to an objective science of game effects or game culture.

As some excellent examples of the research being done, here were some of the things discussed as the Young Academics Workshop. Derek Price discussed how violence is represented in games outside the United States. Less focus on the violent game debate has allowed for a greater interest in other issues such as economic deprivation or social strife. Frank Fetzler discussed how avatars act as moral shields between the player and their behavior in games. This line of research may help us to understand the gulf between what people do in games and what they don't do in real life. Along this thread Christian Roth examined how moral disengagement allows players to take on roles in games they would not take on in real life. Natali Panic-Cidic examined how violence in games can take on meaning that allows players to explore cognitive and emotional boundaries. Exploring violence in games can actually help us to understand empathy and compassion in real life. Cornelia Janina Schnaars explored the aesthetics of violence in games and how violence itself can be rendered unto art as is often done in other media. Rüdiger Brandis and Alexander Boccia examined how ceremony and ritual in violent games are used to give meaning to the player experience. Taken together, all of these papers take seriously the perspective of game play from the player's experience, something that has been fundamentally lacking in most of the social science research.

After four decades of research, it is likely time to admit that we have not amassed an evidence base that justifies warning the public about harmful effects of violent video games. I suspect that the reluctance among some to let it go stems from dedicating a life's work to a topic that, in the end, may have been a false path. Or perhaps a defensiveness of psychology itself and a hope to see magic in the wonder of statistics however small and subjective they may be. Worse, we seem to have learned very little about the lack of value in "statistical significance" and are repudiating any worth in the concept of effect size by defending any effect size that is not zero and manages to achieve "statistical significance" in large samples, including meta-analyses. There are, to be sure, some positive movements such as preregistration and an increasing awareness that tiny effect sizes may not matter after all. But until a greater intellectually honest culture takes root in our science, it will continue chasing its tail as a *nasty little subject*.

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