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## ADOLESCENT VIDEO GAME PLAYING AND FIGHTING OVER THE LONG-TERM

MICHAEL R. WARD 

*I present new evidence of the link between video game play and fighting. The General Learning Model predicts that increased aggression from playing violent video games. These predictions are tested using a large longitudinal data set tracking adolescents over time. Consistent with previous research, there is a positive raw correlation between video game playing as an adolescent and aggressive outcomes, in this case fights, even more than a decade later. However, multivariate and instrumental variables estimators do not find a causal relationship. Some implications are: support policy for further interventions is undermined, future research should be more careful about identification threats, and similar methodological approaches can be applied to the effects of other new communication technologies. (JEL D18, L86, O35)*

### I. INTRODUCTION

There is considerable debate over the link between playing violent video games and aggressive behaviors. A large empirical literature comes to no consensus. The American Psychological Association (APA) finds that while there is a relationship between violent video game use and increases in aggressive behavior, aggressive cognitions, and aggressive affect “insufficient evidence exists about whether the link extends to criminal violence or delinquency” (APA, 2015). In this paper, I investigate the effects from adolescent video games playing on acts of violence later in life. Moreover, I compare different empirical methodologies with weaker and stronger assumptions for identifying a causal effect. I find that estimators that place more plausible assumptions on the data tend to find smaller effects or no effects. The magnitudes of all estimated effects are small.

The findings relate to the literature on learned aggression. The “General Aggression Model” (GAM) holds that since violent video game images can be shocking they would usually illicit a strong reaction. According to GAM, through exposure to violent images, one learns the violent responses that further one’s goals within the game context. These responses could carry over to the real-world, in which players

of violent video games would react to similar real-world situations with the violent responses they learned in-game. Desensitization theory, a variation of GAM, posits that through long-term repeated exposure one becomes acclimated so that the shocking imagery no longer generates repulsion and recoiling. Being more accustomed to the images allows one to take similar, usually violent, actions in the real world more often, even long after the desensitization occurred.

The literature testing for learned aggression from violent video games comes from multiple study methodologies: experimental, correlational, observational, and longitudinal. Each has limitations in their applicability to learned aggression. For example, experimental methods have clear causal interpretations but cannot test for continued exposure or for long-term effects. Correlational study methodologies also relate contemporaneous exposure and outcomes but have often assumed away other competing causes making causal inference problematic. Longitudinal studies can both test for long-term effects and overcome many of the limitations on causal inference making by accounting for time-invariant confounding factors. While the analysis described below uses longitudinal data, identification of a causal link comes from accounting

### ABBREVIATIONS

2SLS: Two Stage Least Squares  
GAM: General Aggression Model  
IV: Instrumental Variable  
OLS: Ordinary Least Squares

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for both observable and *unobservable* competing causes of violence.

Video games are likely just the first of many newly available applications from digital media to gain public policy scrutiny. Technological developments have raised policy concerns related to Internet addiction, cyber-bullying, loss of privacy, and identity theft. For example, Facebook began as recently as 2004, YouTube began in 2005, Twitter was created in 2006, Uber and Airbnb began in 2009, and Nest's connected household thermostat ushered in the Internet of Things to end-consumers in 2014. These technologies have been broadly adopted because they offer substantial user benefits, some becoming "indispensable" in less than a decade. Before they were introduced, few consumers could foresee the benefits of these applications, let alone that they would become engrained in their daily lives. These applications have been adopted so quickly and so pervasively that there has been only limited study of these possible unintended consequences.

As experience has been gained with each application, some policy analysts have noted the potential for different societal problems. Social media may tend to addiction and alienation, the "sharing economy" may increase consumer risks and scams, and internet enabled appliances may leave users vulnerable to cyber-attacks and identity theft. We may not have had enough experience with many of these to be able to evaluate their potential for long-term adverse effects. However, the longer experience with video games has generated more investigation of their effects by social scientists. This paper uses video games to point out some methodological concerns in these investigations. The next section describes the theory of learned aggression. This is followed by a description of three of the more common methods used to investigate the effects of social phenomena. The Adolescent Health Study ("Add Health") data are described with specific attention to its variables relevant to investigate the link between video games and fighting. The results of each methodology are presented and compared. I conclude with an evaluation of the specific results for policy intervention as well as some general guidance for future related studies.

## II. THEORY

From the sensational crime stories of the nineteenth century (Comstock and Buckley 1883), to the garish comic books of the early

twentieth century (Hadju 2009), to the contemporary debate over violent games, Americans have long been concerned about the harmful effects of violent media on children. Unlike comic books and pulp "true crime" stories, violence in media, including video games, has received substantial attention by psychologists and media specialists. In particular, in their meta-studies, Greitemeyer and Mügge (2014) identify 98 and Ferguson (2015) identifies 101 empirical studies addressing the psychological effects of violence in video games.

The basis for many of many these studies is the GAM. GAM itself integrates other aggression theories, including affective aggression (Green 1990), cognitive neoassociation<sup>1</sup> (Berkowitz 1967), script theory (Huesmann 1998), and excitation transfer (Zillmann 1971). GAM integrates these with more general social learning and social cognitive theories (e.g., Bandura 1973; Mischel and Shoda 1995) and social information processing models (e.g., Crick and Dodge 1994). Identifying with a game character can improve self-esteem of low self-esteem children (Lieberman 1998) and individuals with higher trait hostility have shown a larger increase in aggressive cognitions in response to pain (Anderson et al 1998).

Critics have noted that GAM is both all-encompassing and lacking in sufficient foundational support (Ferguson and Dyck 2012). Since most manifestations of aggression fall within the GAM structure, falsification may not be possible. GAM is based on the assumptions that aggression is mostly learned, cognitive, and automatic, all of which are called into question by mounting empirical evidence. While GAM requires gamers to be unable to distinguish between fiction and reality, researchers have found that even small children use a fictional story's context to evaluate its truthfulness (Corriveau et al. 2009; Woolley and Van Reet 2006). Finally, some researchers claim that aggression is better viewed on a continuum from adaptive behaviors to maladaptive, and not necessarily something that need be decreased (Ferguson and Beaver 2009; Hawley and Vaughn 2003; Smith 2007).

Desensitization, a narrower theory of learned behaviors, is based on habituation, or diminished emotional responsiveness to intense stimuli

1. When higher level cognitive processes are available to people when they become angry, they can attribute how they feel to its trigger so that they can understand the consequences of acting on the anger.

through repeated exposure. Exposure times are often measured in months, but might occur within weeks of intense exposure in some instances. It occurs when the emotional response evoked in repeated situations proves to be unnecessary within the context. Desensitization therapies, in which a patient is gradually exposed to increasingly offending stimuli, can assist individuals in unlearning anxieties and phobias, for example, visits to a lake to overcome a fear of water. In the video game context, prolonged and repeated exposure to violence in the media may habituate its normal psychological impact and eventually desensitize the observer to the violent imagery. If desensitization also generalizes to the non-virtual world, an inappropriate violent response may result in similar situations (Grizzard et al. 2017). The difference in the testable implications of desensitization relative to GAM is that it occurs over a longer time period as a result of prolonged and repeated violent video game exposure.

The empirical analyses below can be seen as tests for both short-term (GAM) and long-term (desensitization) effects of video game violence. Survey questions on fighting refer to incidents within the past 12 months and the video game playing can be interpreted as representative of the usual amount of time spent gaming. Short-term responses are identified with effects found within the same survey wave while long-term effects would be those found across waves. While both could be important, a permanent violent change in behavior later in life, as with desensitization, would likely engender more public policy concern.

In experimental studies, typically a small pool of subjects is randomly assigned to play a violent game while the control subjects play a less violent game. Randomly selecting subjects into treatment and control groups endeavors to equally assign subjects with potentially confounding effects (e.g., predisposition toward violence) to both groups. This way, selection bias does not confound the causal interpretation. For example, Carnagey, Anderson, and Bushman (2007) sorted 227 college students into eight 20 minute violent or nonviolent game sessions and then had them view videos of real-life violence. Consistent with GAM, participants who previously played a violent video game had lower aggression-related physiological responses while viewing real violence. Anderson and Carnagey (2009) conducted three experiments each with 100 to 150 college students. During and

after game play, subjects' aggressive cognition, affect, attitudes, and behavior were assessed. In all cases, the treatment group measured higher levels of these aggression measures. This particular study was designed to rule out game competitiveness rather than game violence as a determinant of aggression. The prevalence of published experimental studies support learned aggression but these appear particularly susceptible to publication bias (Hilgard, Engelhardt, and Rouder 2017).

In addition, experimental methods may lack external validity since the control state may not reflect the likely real-world counterfactual. Specifically, it is likely that video games are violent because consumers demand violent games. Gamers would be less attracted to the experimental nonviolent control state. Some of these would-be gamers in the control state may opt for entertainment activities that are known to be associated with violence (e.g., alcohol consumption, gang activity). Dahl and Dellavigna (2009) found that this "voluntary incapacitation" can explain some of the decrease in observed violence stemming from watching violent movies. Video game playing tends to be even more time intensive, alone or with friends in a familiar setting that allows little scope for engaging in violent outbursts. Had she not been gaming, the alternative activities likely provide more opportunities for violent actions. However, in experimental settings, members of both the treatment and control groups in most studies are equally incapacitated.

Correlational studies relate survey responses regarding a subject's video gaming to her level of aggression or incidents of violence. For example, Anderson and Dill (2000) surveyed 226 college students and Krahle and Möller (2004) surveyed 231 eighth-grade adolescents. Both found a positive correlation between the survey measures for exposure to video game play and aggressive behavior. A common shortcoming of correlational studies is that exposure is non-random. Since respondents self-select into their level of exposure to video games, there are often multiple pathways to aggressive outcomes. Measurements are often simple correlations from survey responses without addressing possible confounding factors that create selection bias. There is evidence of selection of more aggressive individuals into violent games (Breuer et al. 2015; von Salisch et al. 2011). Some studies attempt to control for alternative causes of the observed outcomes by using a multivariate regression

approach for observable potential confounders. These tend to control for only gender since males both tend to play more video games and fight more often. Ward (2010) analyzed the 37,000 observation Youth Risk Behavior Survey to find that almost all of the correlation disappears after controlling for gender, race, age, and location.

A few longitudinal studies have been undertaken. Data that include observations of the same individual over time have the potential to address the selection issue by decomposing the effect on the outcome into more aggressive individuals taking up the activity (selection) versus changes in aggression linked to changes in the activity (learned behavior). For example, Willoughby et al. (2012) analyzed 1,492 adolescents, Ferguson et al. (2012) analyzed 165 mainly Hispanic youths over 3 years, and Breuer et al. (2015) studied 276 teenagers and young adults. These tend to use multivariate methods and find no effects or small effects of video game play on aggression. None of the studies use modern techniques of uncovering causal effects such as matching estimators, instrumental variables, a regression discontinuity design, or difference-in-differences.

### III. METHODS

Three different methods for examining the link between video game playing and fighting are compared. First, a baseline is generated from simple correlation between variables measuring video game playing and fighting. Next, a multivariate regression model is specified which attempts to control for observable exogenous confounders. Finally, an instrumental variables approach is adopted in which peers' video game playing is used as the instrument. Each successive method makes fewer assumptions and so better reflects the causal effect.

The first specification is a simple univariate correlation between fighting outcomes and video game playing:

$$(1) \text{ Outcome}_i = \beta_0 + \beta_1^{\text{Corr}} \text{Video Game}_i + \epsilon_i.$$

The parameter  $\beta_0$  measures the baseline level of the fighting outcome. The test of this hypothesis is  $\hat{\beta}_1^{\text{Corr}} > 0$  or that the outcome is more prevalent among those who play video games. Since confounding factors are likely to be present, this is not meant to represent a causal relationship. However, it provides an upper-bound for a causal effect and provides a reference comparison for the other estimators.

Multivariate analyses allow for comparisons to be made within demographic strata (e.g., boys playing video games vs. boys not playing video games). Other observable exogenous variables available include age, race and ethnicity, household income, and school. It is likely that other confounding effects exist in the form of parental supervision, role models, etc. but are not observed. The multivariate method implemented here is either ordinary least squares (OLS) or a Probit estimator for binary outcome measures.

(2)

$$\text{Outcome}_i = \beta_0 + \beta_1^{\text{OLS}} \text{Video Game}_i + \beta_2 X_i^1 + \beta_3 X_i^2 + \dots + \beta_{N+1} X_i^N + \epsilon_i,$$

where  $X_i^1$ ,  $X_i^2$ , and  $X_i^N$  represent alternative possible determinants of the outcome. The estimated coefficient,  $\beta_1^{\text{OLS}}$ , is now the partial correlation between the outcome and video gaming conditioned upon the other co-determinants included in the specification. Since some potential determinants of fighting are unobserved, the estimate,  $\hat{\beta}_1^{\text{OLS}}$ , could still be biased.

Instrumental variables (IVs) techniques can generate unbiased estimates even when not all of the relevant confounders are observable. Two instrumental variables are constructed from each individual's peers' video game playing—the fraction of peers that play video games and the average number of hours they play. Video game playing has a social dimension to it leading some gamers to play so as to engage more fully with one's peers (Olson 2010). Peers' video game playing will satisfy the instrument's exclusion restriction if video game playing has an effect on peers' video game playing but not their fighting. This may not be true if some peers are chosen because they share the common interest in both video game playing and in fighting (Manski 1993). In this case, the IVs may remove only part of the correlation between the instrumented variable and unobserved confounders and result in some remaining bias. Even in this case, any remaining correlation with unobserved confounders is likely to be positive causing the bias in the coefficient estimates to be positive.<sup>2</sup>

2. The parameter estimate equals the true value plus the covariance due to friends' shared preference for video games times the effect of friends' video games on violence, assumed to be positive,  $\hat{\beta} = \beta + \text{cov}(\text{VG}, \text{VGpeer}) \times f(\beta)$ . Homophily implies the covariance is positive. If own video game playing causes violence,  $\beta > 0$ , then it is likely that peers' video game playing also cause violence,  $f(\beta) > 0$ . Hence, if  $\beta > 0$ ,  $\hat{\beta} \geq \beta$ .



Thus, estimates from these IVs would tend to continue to overstate the likelihood that video game playing causes fighting.

IV estimation is implemented with the standard two-staged least squares for continuous outcomes and IV Probit for binary outcomes,

$$(3) \text{ Outcome}_i = \beta_0 + \beta_1^{\text{IV}} \widehat{\text{Video Game}}_i + \beta_2 X_i^1 + \beta_3 X_i^2 + \dots + \beta_{N+1} X_i^N + \epsilon_i$$

$$\begin{aligned} \text{Video Game}_i &= \alpha_0 + \alpha_1 \text{Peer Video Game}_i \\ &+ \alpha_2 X_i^1 + \alpha_3 X_i^2 + \dots \\ &+ \alpha_{N+1} X_i^N + \nu_i. \end{aligned}$$

Ideally, this will eliminate the source of bias in  $\hat{\beta}_1^{\text{OLS}}$  in Equation (2). The variation in Video Game<sub>*i*</sub> going into the estimate of  $\hat{\beta}_1^{\text{IV}}$  is the result of Peer Video Game<sub>*i*</sub> resulting in an unbiased estimate.<sup>3</sup> In practice, no instruments are perfect and imperfect instruments will eliminate some, but not all, of the bias.

#### IV. ADD HEALTH DATA

By relating video game playing as an adolescent to fighting both as an adolescent and later as a young adult, this analysis attempts to uncover both short-term and long-term effects. Importantly, it meets many of the requirements for evaluating the public policy concern over desensitization causing a long-term aggression effect. First, the video game playing information refers to usual daily activities which could represent continued exposure and sustained use. Second, actual fighting outcomes draw attention to whether heightened aggression manifests into a behavioral change. Third, fighting is recorded in multiple waves over many years allowing for the measurement of both short-term and longer-term effects. Fourth, when not being “voluntarily incapacitated,” non-video game players choose their next-best competing risk activities similar to what is likely to result from any policy intervention.

The data are from the National Longitudinal Study of Adolescent to Adult Health (Add

Health).<sup>4</sup> It is a nationally representative sample of adolescents in Grades 7–12 in the United States between April and December 1995. This cohort has been followed into young adulthood with four in-home interviews, with Wave II in 1996 when most were still in high school, Wave III in 2001/2002 when most were in their early 20s, and Wave IV in 2008, when the sample was 24–32 years old. There are over 20,000 participants in Wave I. Some participants drop out of the sample but Wave IV still contains over 15,000 participants. In contrast, the 26 correlational studies included in the meta-analysis by Greitemeyer and Mügge (2014) had a combined total of 25,774 participants. Add Health’s 15,460 Wave IV participants represent over twice as many as the 6,283 participants in all nine longitudinal studies Greitemeyer and Mügge (2014) identify.<sup>5</sup> The Ferguson (2015) meta-analysis identifies a different set of 101 studies with a total of 106,070 observations. The single Add Health sample used here is one-sixth the size of all 101 studies identified by Ferguson (2015) combined.

The video game exposure variable comes from the question “How many hours a week do you play video or computer games?” asked in all four waves. About one-half of all respondents report that they played some video games and they played an average of 1 hour per week. Video game playing intensity was highly skewed with a median of 2 hours per week for those who played video games in Wave I and 10% playing 12 or more hours per week. The analysis focuses on the long term effects of adolescent choices by examining whether video game playing in Wave I affects fighting in each of the subsequent waves.<sup>6</sup> The measure of video game playing does not specify whether or not the games were violent meaning that it includes playing some nonviolent games. This measurement error will affect the estimates of interest.

Consider that the true relationship is  $y = \beta v + \epsilon$  where  $y$  is a fighting outcome,  $v$  is violent gaming, and  $\epsilon$  is supposed to be the

4. Detailed description of the data, as well as how to obtain them, can be found at <http://www.cpc.unc.edu/projects/addhealth>.

5. Because of missing values for some variables, only 15,351 of the Add Health observations are actually used here.

6. Continued video game playing in multiple waves might also be an appropriate measure for long term exposure to violent imagery. The results reported below are qualitatively unchanged for measures that incorporate video game playing in multiple waves. This is due, in part, to video game playing being correlated across waves.

3. Two separate instruments are generated from peers: the fraction of friends who play video games and the average amount they play. It is possible that some peers are chosen so as to play video games together. When this occurs, the amount of video game playing by peers is no longer completely independent of the focal observation’s video game playing. It is still the case that the IV estimates are less likely to be affected by bias than non-IV estimates.

i.i.d. error term. However, we only observe  $g = v + n$ , gaming as the sum of violent gaming (signal) and nonviolent gaming (noise). This yields the OLS estimate of  $\beta$  as

$$\hat{\beta} = \frac{\text{cov}(g, y)}{\text{var}(g)} = \frac{\text{cov}(v + n, \beta v + \epsilon)}{\text{var}(v + n)}.$$

So that we have

$$\text{plim } \hat{\beta} = \beta \left( \frac{\sigma_v^2 + \sigma_{vn} + \sigma_{n\epsilon}/\beta}{\sigma_v^2 + \sigma_n^2 + 2\sigma_{vn}} \right).$$

Note that with perfect measurement  $n = 0$  making  $\sigma_n^2$ ,  $\sigma_{vn}$ , and  $\sigma_{n\epsilon}$  all zero and  $\text{plim } \hat{\beta} = \beta$  implying that the parameter estimate is consistent. However, all data are measured with some error. In this case,  $n$  is nontrivial portion of  $g$  causing  $\sigma_n^2 > 0$ . If  $\sigma_{vn} = \sigma_{n\epsilon} = 0$  but  $\sigma_n^2 > 0$ , then  $\text{plim } \hat{\beta} = \beta \left( \frac{\sigma_v^2}{\sigma_v^2 + \sigma_n^2} \right) < \beta$  or the OLS estimate is the same sign as the true parameter but is biased toward zero. This is sometimes called the “attenuation” bias. The case where  $\sigma_{vn} \neq 0$  is a bit more complicated since the sign of the bias cannot be determined if  $\sigma_{vn} < 0$ .

Data from Raptr.com on the time spent playing video games for over 150,000 individuals do not identify which games are violent but do identify the rating of the games played. All violent games are rated “Mature” and almost all “Mature” games are described as “intensely violent” by the ESRB (Cunningham, Engelstätter, and Ward 2016). About 70% of Raptr gamers played a mix of both mature and not-mature games. I calculated the covariance of time playing mature games with non-mature games as a proxy for as  $\hat{\sigma}_{vn} \approx 0.05$ . Likewise, these data suggest that  $\hat{\sigma}_v^2 \approx \hat{\sigma}_n^2$ . In this case, the OLS estimate is biased toward zero with  $\hat{\beta} \approx \beta/2$ .

Using an IV estimator can provide consistent estimates even in the face of measurement error. This would be the case if the instrument,  $z$ , is uncorrelated with the measurement error,  $n$ . This is because the instrumented regressor is  $\tilde{v} = \text{cov}(v + n, z) = \text{cov}(v, z) + \text{cov}(n, z) = \text{cov}(v, z) + 0$ . Since instrumenting removes the correlation of  $\tilde{v}$  with  $n$ , the denominator in the plim no longer contains  $\sigma_n^2 + 2\sigma_{vn}$ . However, in the present case, the instruments are video game playing by peers,  $g\_peer = v\_peer + n\_peer$ . It is likely that  $\text{cov}(n, n\_peer) \geq 0$  so that some of the attenuation bias remains even in the IV estimates.

Table 1 provides some descriptive information from the survey questions related to fighting that were used. The first question for Wave I asks if,

in the past 12 months, the respondent had been in a fight ever, once, or more than once. From this, I construct two dummy variables for any fighting (Once or More than Once) and a more restrictive fighting twice or more. I dropped from the analysis the 1% of respondents were reported as refused to answer, did not know, or not applicable. The second question records the number of fights that resulted in medical care in the past 12 months. Legitimate skips were recorded as zero if the respondent had elsewhere indicated they had not been in fights. In Wave II, one question asks for the number of serious fights in the past 12 months which was converted to a binary outcome since few selected more than two. Another question again asked for the number of fights in the past 12 months that required medical attention. In Wave III, when respondents are in their early 20s, they were asked separately for the number of fights in the past 12 months in which they or another required medical attention. In Wave IV, when the bulk of respondents were aged 24–32, two questions asked for the number of fights between groups of belligerents and the number of serious fights. Again, these were converted to a binary outcome due to too few instance greater than two.

The Add Health data also contain a wealth of information about the characteristics of each participant. Nearly 1,000 variables contain demographic information, family information, school performance and activities, daily activities, social interactions, physical development, romantic and sexual encounters, and risky behaviors. I include a small set of predetermined variables as potential confounders that could not be caused by the subject’s video game playing. These include the subject’s age, sex, racial group, Hispanic origin, school, and family income.

A unique feature of the Add Health data is that each individual surveyed is asked to identify up to 10, but usually 4–6, individuals as their peers or friends. For a subsample of the data, the friends identified by the focal subject are also in the original survey. From their responses, instrumental variables are constructed from the video gaming activity of the focal subject’s friends as the fraction of friends who play video games and the average hours they spend playing video games. Except for possible video game homophily in friendship formation discussed above, these should be a valid IVs. Under these assumptions, I use peer video game playing to estimate,  $\hat{\beta}_1^{IV}$ , the instrumental variables estimate from Equations (3).

**TABLE 1**  
Fighting Questions from Add Health

Wave	Question	Possible Responses	Count
I	During the past 12 months, how often did each of the following things happen? You got into a physical fight.	Never Once More than Once Refused/Do not Know/NA	13,821 3,920 2,832 173
I	During the past 12 months, how many times were you in a physical fight in which you were injured and had to be treated by a doctor or nurse?	Range 0–365 times Legitimate Skip Refused/Do not Know/NA/Miss	12,084 8,528 124
II	In the past 12 months, how often did you get into a serious physical fight?	Never 1–2 times 3–4 times 5 or more times Refused/Do not Know	11,773 2,427 302 170 66
II	During the past 12 months, how many times were you in a physical fight in which you were injured and had to be treated by a doctor or nurse?	Range 0–333 times Legitimate Skip Refused/Do not Know	2,868 11,839 31
III	During the past 12 months, how many times were you in a physical fight in which you were injured and had to be treated by a doctor or nurse?	0 times 1–56 times Refused/Do not Know/NA/Miss	14,375 579 243
III	In the past 12 months, how often did you hurt someone badly enough in a physical fight that he or she needed care from a doctor or nurse?	0 times 1–67 times Refused/Do not Know/NA/Miss	14,101 847 249
IV	In the past 12 months, how often did you take part in a physical fight where a group of your friends was against another group?	Never 1–2 times 3–4 times 5 or more times Refused/Do not Know	15,163 431 46 18 43
IV	In the past 12 months, how often did you get into a serious physical fight?	Never 1–2 times 3–4 times 5 or more times Refused/Do not Know	14,851 713 59 34 44

Summary statistics for the data used below are reported in Table 2. This table reports means and standard deviations for the sub-sample with and without friend information as well as the probability that the means of these variables come from a common distribution. Note that there are significant differences between the means of almost all variables across sub-samples. This indicates non-random selection into the sample with peer information. If this form of self-selection is related to video game usage, it could bias the estimates of interest. This will be discussed further when the instrumental variables results are presented.

One issue with any longitudinal data set is that some cohort members drop out of the sample in later survey waves while others “survive” into later waves, often for predictable, or non-random, reasons. For example, if the more violent subjects tend to drop out but video game players do not, then the measured fighting activity in later waves would be biased downward and the procedures

outlined above could find a smaller association when one does exist. One way to examine for selection into later waves is to compare mean values of Wave I responses between those who will drop out and those who will not. Table 3 reports that most of these differences, although small, are statistically significant. For what follows, I assume that the mechanisms generating this selection are uncorrelated with video game playing so as to eliminate “survival” bias.

## V. RESULTS

The general pattern of results does not support a learned violence. The simple correlations between playing more video games early in life to all fighting outcomes are small, but positive and significant in all cases. However, these results cannot be considered causal. With the multivariate regressions, the magnitude of the parameter estimates are reduced and estimates for only



**TABLE 2**  
Summary Statistics—Means and Standard Deviations

	With Friend Information	No Friend Information	<i>p</i> Value on Difference
Video game hours per week	2.696 (6.109)	2.956 (6.819)	.013
Peers' video game hours per week	2.447 (4.378)		
Fraction of peers playing any video games	0.215 (0.233)		
Male	0.483 (0.500)	0.522 (0.500)	.000
Hispanic	0.132 (0.339)	0.189 (0.392)	.000
Race: White	0.677 (0.468)	0.624 (0.485)	.000
Race: Black	0.203 (0.402)	0.243 (0.429)	.000
Race: American Indian	0.036 (0.186)	0.037 (0.189)	.710
Race: Asian	0.067 (0.249)	0.050 (0.217)	.000
Race: Other	0.073 (0.260)	0.104 (0.305)	.000
Age	47.750 (56.355)	43.046 (44.424)	.000
Income	16.086 (1.667)	16.130 (1.851)	.120
Observations	8,752	6,599	

**TABLE 3**  
Differences in Variable Means across Survey Waves

	Wave I	Wave II	Wave III	Wave IV
Video game hours per week	2.807 (6.424)	2.870*** (6.434)	2.707*** (6.229)	2.718** (6.284)
Peers' video game hours per week	2.447 (4.378)	2.501* (4.442)	2.457 (4.460)	2.466 (4.445)
Fraction of peers playing any video games	0.215 (0.233)	0.220*** (0.233)	0.215 (0.232)	0.215 (0.230)
Male	0.500 (0.500)	0.492 (0.500)	0.476*** (0.499)	0.476*** (0.499)
Hispanic	0.157 (0.363)	0.157 (0.364)	0.148** (0.355)	0.149** (0.356)
Race: White	0.654 (0.476)	0.665*** (0.472)	0.663 (0.473)	0.670*** (0.470)
Race: Black	0.220 (0.414)	0.214** (0.410)	0.213** (0.409)	0.215* (0.411)
Race: American Indian	0.036 (0.187)	0.036 (0.188)	0.036 (0.186)	0.035** (0.183)
Race: Asian	0.059 (0.236)	0.058 (0.233)	0.063*** (0.243)	0.055*** (0.228)
Race: Other	0.086 (0.281)	0.084 (0.278)	0.080*** (0.271)	0.082** (0.274)
Income (\$1,000)	45.728 (51.617)	46.116 (52.265)	46.621** (50.399)	46.434* (50.524)
Age (in Wave I)	16.105 (1.749)	15.802*** (1.632)	16.039 (1.744)	16.054 (1.746)
Observations	15,351	11,236	11,308	11,882

*Note:* Standard errors in parentheses test for difference in mean from Wave I values.

\* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

**TABLE 4**  
Estimated Coefficients with no Controls for Possible Confounders

Period	Wave I	Wave I	Wave I	Wave II	Wave II	Wave III	Wave III	Wave IV	Wave IV
Method Outcome	Probit Any Fight	Probit 2+ Fights	OLS Fights w/Injury	Probit Serious Fight	OLS Fights w/Injury	OLS Fights w/Injury Self	OLS Fights w/Injury Others	Probit Gang Fight	Probit Serious Fight
Ln video game hours per week	0.144* (0.011)	0.124* (0.013)	0.019* (0.004)	0.114* (0.014)	0.010* (0.003)	0.011* (0.002)	0.020* (0.003)	0.194* (0.022)	0.152* (0.020)

Note: Robust standard errors in parentheses.

\* $p < .01$ .

three of nine outcomes are significantly different from zero. Finally, with the instrumental variables regressions, all estimated effects are negative but most are not statistically different from zero. If any of these estimates are biased, the bias is likely positive making the true value even smaller. In sum, as we employ estimators that require more plausible assumptions to infer causality, the support for learned aggression and desensitization from video games disappears.

The effects of video game playing are estimated to affect nine different outcomes relating to fighting occurring over four waves of interviews. For binary outcomes, a Probit specification is used,

$$\begin{aligned} \text{Prob}(\text{Outcome}_i = 1) \\ = \Phi(\beta_0 + \beta_1 \text{ Video Gaming}_i \\ + \beta_2 \text{ Confounders}_i, \sigma^2) \end{aligned}$$

where  $\Phi(\mu, \sigma^2)$  is the normal distribution with mean  $\mu$  and variance  $\sigma^2$ . Because the Probit is a non-linear function, the marginal effect of video gaming on a binary outcome is not the coefficient estimate of  $\beta_1$ . Instead, by the chain rule, it becomes  $\beta_1 \phi$ . Below, both the coefficients and the marginal effects evaluated at the sample mean are reported. The continuous outcomes all have highly skewed distributions and the outcome measure was transformed by taking the logarithm,

$$\begin{aligned} \text{Ln}(\text{Outcome}_i + 1) \\ = \beta_0 + \beta_1 \text{ Video Gaming}_i \\ + \beta_2 \text{ Confounders}_i + \epsilon_i. \end{aligned}$$

The resulting distributions more closely resemble a normal distribution and the coefficient estimates can be interpreted directly.

As a baseline, Table 4 reports the relevant coefficient estimates for Equation (1) for all nine

outcomes. For each outcome, the wave of the survey is reported as well as the estimator, Probit or OLS. Since no controls are included in this specification, this represents the raw associations between video game playing and fighting. Across all waves and all fighting outcome measures, there is a highly significant positive correlation between more video game playing and fighting. This finding is comparable to most of the correlational studies discussed above. The magnitudes of the effects, reported in column 1 of Table 8, allow for a more direct evaluation of the magnitude of these effects across outcome measures.<sup>7</sup> The estimated marginal values, between 0.010 and 0.051, represent a 100% increase in video game playing being associated with a 1%–5% increase in fighting.

Tables 5 reports the relevant coefficient estimates for Equation (2) controlling for a set of potential confounders for all nine outcomes. For example, males are estimated to be considerably more prone to fighting across all outcome variables and they also play more video games. There are some significant differences across racial categories. Older individuals as well as individuals from higher income households tend to fight less. The data identify the school the student attended which will tend to absorb most of the fighting resulting from differences across neighborhoods. Over 400 school dummy variables are included and are jointly significant. As can be seen by comparing Table 4 with Table 5, omitting these contributed to substantial bias. Even accounting for 50% attenuation bias, few of these coefficients would be significantly different from

7. The coefficient estimates from OLS represent the change in the outcome due to each regressor. However, since Probit is a nonlinear estimator, the coefficients need to be adjusted to represent the change in the outcome due a regressor. Table 8 calculates the marginal effect at sample means so that the reported values are comparable across all outcome measures.

**TABLE 5**  
Estimated Coefficients Controlling for Potential Confounders

Period Method	Wave I Probit	Wave I Probit	Wave I OLS	Wave II Probit	Wave II OLS	Wave III OLS Fights w/Injury Self	Wave III OLS Fights w/Injury Others	Wave IV Probit	Wave IV Probit
Outcome	Any Fight	2+ Fights	Fights w/Injury	Serious Fight	Fights w/Injury			Gang Fight	Serious Fight
Ln video game hours per week	0.028** (0.013)	0.029* (0.015)	0.004 (0.004)	0.017 (0.016)	0.003 (0.003)	0.001 (0.003)	0.003 (0.003)	0.073*** (0.025)	0.033 (0.022)
Male	0.562*** (0.023)	0.537*** (0.029)	0.076*** (0.006)	0.487*** (0.030)	0.034*** (0.005)	0.040*** (0.004)	0.078*** (0.006)	0.713*** (0.057)	0.615*** (0.044)
Hispanic	0.091** (0.045)	0.089* (0.054)	0.003 (0.012)	0.112** (0.056)	0.010 (0.010)	-0.001 (0.007)	0.007 (0.013)	0.082 (0.094)	0.031 (0.079)
White	0.042 (0.056)	0.048 (0.067)	0.009 (0.019)	-0.034 (0.068)	-0.006 (0.015)	-0.012 (0.009)	-0.003 (0.016)	-0.205* (0.115)	-0.200* (0.112)
Black	0.294*** (0.058)	0.214*** (0.070)	0.035* (0.020)	0.157** (0.072)	0.015 (0.015)	0.022** (0.010)	0.022 (0.018)	0.053 (0.120)	-0.025 (0.118)
Amer. Ind.	0.224*** (0.059)	0.202*** (0.068)	-0.002 (0.018)	0.291*** (0.073)	0.030** (0.014)	0.012 (0.011)	0.008 (0.014)	0.076 (0.131)	0.177 (0.107)
Asian	-0.039 (0.071)	-0.054 (0.087)	-0.030* (0.018)	-0.167* (0.091)	-0.005 (0.013)	-0.032*** (0.010)	-0.033** (0.016)	-0.174 (0.149)	-0.393*** (0.146)
Other	0.094 (0.066)	0.162** (0.079)	0.039** (0.019)	0.072 (0.080)	0.016 (0.020)	0.004 (0.011)	0.010 (0.020)	-0.106 (0.138)	-0.036 (0.125)
Income	-0.001*** (0.000)	-0.001** (0.000)	-0.000*** (0.000)	-0.001** (0.000)	-0.000 (0.000)	-0.000** (0.000)	-0.000** (0.000)	-0.001 (0.001)	-0.001 (0.001)
Age	-0.030*** (0.009)	-0.007 (0.010)	0.005** (0.002)	-0.035*** (0.011)	0.004** (0.002)	-0.001 (0.001)	-0.007*** (0.002)	-0.067*** (0.017)	-0.039*** (0.015)

School fixed effects are not reported. Robust standard errors in parentheses.

\* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

zero at standard confidence levels and the magnitudes would still be small. The estimated coefficients for video game playing are a fraction of their counterparts in Table 4 with only three being significantly different from zero. In this specification the elasticity estimate is in the range of 0.000 to 0.009 (see Table 8). That is, these imply a 100% increase in video game playing is associated with a 0.0%–0.9% increase in fighting.

Further insights can be found by comparing the effects on fighting across waves.<sup>8</sup> The largest effects are for Wave I that records outcomes contemporaneous with the video game playing. Analyses of contemporaneous outcomes are particularly susceptible to omitted variable bias in that those prone to violence may engage in solitary activities like video game playing so as to remove themselves from potentially violent situations. The stronger test of a behavioral change associated with desensitization relates video game playing in Wave I with fighting in

later waves.<sup>9</sup> For later waves, the largest estimated elasticity is 0.005, or a 100% increase in video game playing causes an increase in fighting of, at most, 0.5%.

Implementing the instrumental variables regression requires the estimation of two equations. First stage regressions reported in Table 6 show that peer video game playing variables are statistically significantly different from zero and so indicate that they satisfy the relevancy criterion. Consistent with peer influences, the focal respondent's video game playing increases with friends' video game playing. Excludability is not generally testable but with two instruments, the model is over-identified and Sargan tests can be conducted. In all cases, these tests fail to reject the exogeneity of the instruments. These provide suggestive evidence of instruments validity; however, they may be underpowered since both are derived from the same peer behavior.

8. Recall that Wave I occurred during the 1994–1995 when interviewees were in Grades 7–12 when over 95% were aged 13–18. Wave II occurred in 1996, Wave III in 2001–2002, and Wave IV in 2008.

9. Since both Waves I and II were conducted during adolescence, an alternative model would examine the effects on fighting in Waves III and IV from video game playing during both of these earlier waves. Results from this specification are not qualitatively different from those reported here.

**TABLE 6**  
First Stage Regressions

	(1)	(2)	(3)
Ln Peers' video game hours per week	0.025* (0.014)	0.043*** (0.012)	
Fraction of peers playing any video games	0.133** (0.054)		0.179*** (0.046)
Male	0.608*** (0.018)	0.612*** (0.018)	0.609*** (0.018)
Hispanic	-0.036 (0.043)	-0.036 (0.043)	-0.035 (0.043)
Race: White	-0.082* (0.048)	-0.082* (0.048)	-0.081* (0.048)
Race: Black	0.041 (0.052)	0.043 (0.052)	0.042 (0.052)
Race: American Indian	0.017 (0.050)	0.016 (0.050)	0.018 (0.050)
Race: Asian	0.059 (0.058)	0.060 (0.058)	0.059 (0.058)
Race: Other	-0.111* (0.062)	-0.109* (0.062)	-0.111* (0.062)
Income	-0.000** (0.000)	-0.000** (0.000)	-0.000** (0.000)
Age	-0.090*** (0.008)	-0.090*** (0.008)	-0.091*** (0.008)
School fixed effects	X	X	X
$R^2$	0.168	0.167	0.167
F test on IVs	9.16***	12.16***	15.01***

Sample includes 11,759 observations with peer information. Robust standard errors in parentheses.

\* $p < .1$  F tests on; \*\* $p < .05$ ; \*\*\* $p < .01$ .

Finally, Table 7 reports coefficient estimates for the second stage of the instrumental variables specification of Equation (3). The estimated coefficients for the control variables are qualitatively unchanged from Table 5. However, the estimates for video game playing generate different conclusions. These coefficient estimates are uniformly negative with two being significantly different from zero. While the expected value of the coefficient is always negative, the estimated standard errors in the instrumental variables estimations are much larger implying that the probability of a positive value is often higher than found in the multivariate results. With that caveat, these coefficient estimates provide no support for the hypothesized learned aggression or desensitization. If anything, video game playing leads to decreased fighting later in life.

Certain regularities can be gleaned from Table 8 comparing results for all outcomes across all estimators. Estimators that better address the causal inference problem find weaker or no support for video games causing violence. The claim that the first column of estimates represents a causal effect is implausible since it

assumes that all other possible causes of fighting are unrelated to video game playing. The conditional independence assumption underlying the second column is that the effects of omitted variables beyond those included are not related to video game playing. This specification may omit a relevant confounding effect resulting in some omitted variable bias. This column provides only marginal support for learned aggression. The assumption underlying the third column is that peers' video game playing is independent of other possible confounding causes of the focal subject's fighting (or leads to upward bias) a much weaker assumption.

All estimates of increased fighting are small. Even the implausibly large simple correlations in represent 1%–5% increase in fighting from a doubling in video game playing. The largest in the multivariate regressions imply a 0.5% increase. The instrumented specifications imply no increase. The amount of time spent playing games in the American Time Use Survey has been increasing about 3% per year from 2005 to

**TABLE 7**  
Estimated Coefficients Controlling for Confounders and Using Instrumental Variables

Period Method	Wave I Probit	Wave I Probit	Wave I OLS	Wave II Probit	Wave II OLS	Wave III OLS Fights w/Injury Self	Wave III OLS Fights w/Injury Others	Wave IV Probit	Wave IV Probit
Outcome	Any Fight	2+ Fights	Fights w/Injury	Serious Fight	Fights w/Injury			Gang Fight	Serious Fight
Ln video game hours per week	-0.067 (0.357)	-0.072 (0.465)	-0.038 (0.100)	-0.639** (0.297)	-0.069 (0.057)	-0.076 (0.061)	-0.116 (0.083)	-0.357 (0.816)	-0.995*** (0.189)
Male	0.634*** (0.206)	0.610** (0.269)	0.097 (0.062)	0.846*** (0.110)	0.071** (0.036)	0.081** (0.038)	0.145*** (0.051)	1.014*** (0.309)	1.001*** (0.052)
Hispanic	0.071 (0.067)	0.061 (0.084)	-0.002 (0.019)	0.097 (0.082)	0.010 (0.013)	-0.006 (0.010)	0.010 (0.020)	0.183 (0.142)	-0.068 (0.078)
White	0.036 (0.082)	0.102 (0.101)	0.009 (0.032)	-0.161* (0.093)	-0.009 (0.016)	-0.018 (0.013)	0.003 (0.024)	-0.365** (0.145)	-0.194* (0.112)
Black	0.371*** (0.081)	0.275*** (0.097)	0.043 (0.029)	0.173* (0.101)	0.020 (0.015)	0.022 (0.015)	0.059** (0.026)	0.005 (0.151)	0.016 (0.113)
Amer. Ind.	0.224*** (0.080)	0.189** (0.093)	0.011 (0.027)	0.243** (0.100)	0.016 (0.014)	0.015 (0.014)	0.014 (0.020)	0.055 (0.184)	0.167 (0.106)
Asian	0.002 (0.098)	0.021 (0.119)	-0.027 (0.028)	-0.075 (0.111)	0.013 (0.016)	-0.030** (0.015)	-0.010 (0.025)	-0.153 (0.168)	-0.264 (0.168)
Other	0.203* (0.105)	0.372*** (0.132)	0.023 (0.032)	0.011 (0.131)	-0.003 (0.018)	0.007 (0.018)	0.014 (0.033)	-0.217 (0.171)	-0.074 (0.123)
Income	-0.001** (0.000)	-0.001 (0.001)	-0.000** (0.000)	-0.001** (0.000)	-0.000*** (0.000)	-0.000** (0.000)	-0.000** (0.000)	-0.002** (0.001)	-0.001 (0.001)
Age	-0.047 (0.034)	-0.030 (0.044)	0.001 (0.010)	-0.100*** (0.026)	-0.005 (0.006)	-0.008 (0.006)	-0.018** (0.008)	-0.115** (0.055)	-0.104*** (0.018)

School fixed effects are not reported. Robust standard errors in parentheses.

\* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$ .

**TABLE 8**  
Summary of Results

Period	Outcome	Univariate		Multivariate		IV	
		Estimate	s.e.	Estimate	s.e.	Estimate	s.e.
Wave I	Any Fights	0.052**	(0.003)	0.009*	(0.004)	-0.067	(0.357)
Wave I	2+ Fights	0.027**	(0.003)	0.006*	(0.003)	-0.072	(0.465)
Wave I	Fights w/Injury	0.019**	(0.003)	0.004	(0.004)	-0.038	(0.100)
Wave II	Serious Fight	0.032**	(0.004)	0.005	(0.004)	-0.639*	(0.297)
Wave II	Fights w/Injury	0.010**	(0.002)	0.003	(0.003)	-0.069	(0.057)
Wave III	Fights w/Injury to Self	0.011**	(0.002)	0.001	(0.002)	-0.076	(0.061)
Wave III	Fights w/Injury to Others	0.020**	(0.002)	0.003	(0.003)	-0.116	(0.083)
Wave IV	Gang Fight	0.014**	(0.002)	0.005**	(0.002)	-0.357	(0.816)
Wave IV	Serious Fight	0.016**	(0.002)	0.003	(0.002)	-0.995**	(0.189)

Marginal effects video gaming on different measures of violence measured in elasticities (% $\Delta$ Fighting/% $\Delta$ Video Gaming).

\* $p < .05$ ; \*\* $p < .01$ .

2013.<sup>10,11</sup> Extrapolating from this and using the multivariate estimates, this suggests video games caused, at most, 0.16% more fights per year. Of course, the IV estimates suggest no increase in fighting and, perhaps, a decrease.

10. <https://www.bls.gov/tus/>

11. The number of video gamers worldwide is increasing at about 6% per year. <https://www.statista.com/statistics/748044/number-video-gamers-world/>

## VI. CONCLUSION

The evidence presented here indicates that the positive association between video game play and violence is not causal. Prior research has established that most of the simple correlation is due to confounding factors, most notably that boys both play video games more and fight more often. Estimators that better address the confounder issue fail to find a positive association. The multivariate estimates have small enough



standard errors so as to often represent a “precisely estimated zero.” The IV estimates, which should be even freer of bias, represent an imprecisely estimated negative effect. In sum, the most comprehensive study to date using the estimators that make the weakest prior assumptions about the data find no the causal link between video game playing and fighting.

Even the largest estimates represent effects that may be too small to warrant clear policy implications. Policy interventions to reduce video game induced fighting, such as violent content restrictions, would likely be imperfectly implemented and so would not fully eliminate whatever induced fighting exists. Any improvement in fighting outcomes would have to be weighed against societal losses the intervention would entail. For example, a hypothetical mandate that game designers reduce violent imagery would likely make games less desirable to a segment of the consumer base that prefers them to be violent. The intervention would lead some of these consumers to choose less preferred substitute past-times and so incur a utility loss. There are additional dynamic considerations beyond these static comparisons. Video game development is among the fastest evolving forms of human expression ever devised. If the trend in video game development over the past few decades were to continue, it would be difficult for us to imagine the experiences that games developed over the next few decades will provide. A content-based policy intervention could hamper the further evolution of this medium.

This experience may provide insights for other forms of entertainment and communication emerging from the digitization revolution. New and valuable consumer media applications continue to emerge. Many of these, such as video sharing and social media, have gained wide acceptance because they provide functionality not previously available. Undoubtedly, some applications will appear to have some problematic effects for some users, for example, stalking and cyberbullying. These adverse outcomes will be studied with an eye toward possible policy interventions to moderate undesired outcomes. It is important that these are studied with care so that the inference can be more plausibly construed as causal. The methods presented above outline some of the issues in developing plausibly causal estimates of the effects of media.

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