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Violent Video Games and Violent Crime

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Video games are an increasingly popular leisure activity. As many best-selling games contain hyper-realistic violence, many researchers and policymakers have hypothesized that violent games cause violent behaviors. Laboratory experiments have found evidence suggesting that violent video games increase aggression. Before drawing policy conclusions about the effect of violent games on actual behavior, these experimental studies should be subjected to tests of external validity. Our study uses a quasi-experimental methodology to identify the short-run and medium-run effects of violent game sales on violent crime using time variation in retail unit sales data of the top 30 selling video games and violent criminal offenses from both the Uniform Crime Report and the National Incident-Based Reporting System from 2005 to 2011. We find no evidence of an increase in crime associated with video games and perhaps a decrease.

JEL Classification: D08, K14, L86

1. Introduction

Violence in video games is a prominent policy concern. The issue has generated six reports to the U.S. Congress by the Federal Trade Commission (Federal Trade Commission 2009) and in 2010 led to a law in California making it a punishable offense for a distributor to sell a banned violent video to a minor. This law was later struck down by the U.S. Supreme Court in June 2011 (Supreme Court 2011). Policymaker concern has been motivated by the connection between violent video game imagery and psychological aggression in video game players, particularly adolescents. While researchers have documented an effect on aggression in the laboratory, some have suggested that violent video games are responsible for violent crime such as school shootings (Anderson 2004).

The short-run effect of violent games on aggression has been extensively documented in laboratory experiments (Anderson, Gentile, and Buckley 2007). These experiments generally conclude that media violence is self-reinforcing rather than cathartic. This link has not been found with crime data however. Ward (2011) found a negative association between county-level video game store growth and the growth in crime rates. In a relevant study, Dahl and DellaVigna (2009) find that popular violent movies caused crime to decrease in the evening and weekend hours of a movie's release lasting into the following week, with evidence that violent movies were drawing men into theaters and away from alcohol consumption. These two studies suggest the real world relationship between violent media and crime may be more complex than the results from laboratory

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studies indicate. There is also disagreement within the psychological literature itself about the interpretation of laboratory studies of video game violence. For instance, critics have pointed out methodological problems such as the use of poor aggression measures and inflated effect size (Ferguson and Kilburn 2008). Also, after correcting for publication bias, studies of media violence effects provided little support for the hypothesis that media violence is associated with higher aggression (Ferguson and Kilburn 2008).

We estimate the reduced form effect of intensely violent video games on crime using a strategy similar to Dahl and DellaVigna (2009). We proxy for video game play using video game sales information harvested from VGChartz, an industry source tracking the weekly top 30 best-selling video game titles from 2005 to 2011. The violence content for each video game was matched using information provided by the Entertainment Software Rating Board (ESRB). Our measures of crime are from the Uniform Crime Report (UCR) and the National Incident-Based Reporting System (NIBRS). We use both crime data sources separately to create a time series of violent and nonviolent crime levels for the periods in question. The UCR data measures crime at the monthly level, whereas the NIBRS data measures crime by hour of day. We construct weekly measures of crime by aggregating the NIBRS data by week.

Our main finding is that we fail to find evidence that violent video games increase crime. In contrast, we find evidence that violent games cause a modest reduction in crime. Our analysis indicates that crimes are either invariant to or are decreasing in video game popularity. This is true for both nonviolent and intensely violent video games and independent of the crime data used. These results suggest that the generalized aggression hypothesis which warns of violent behaviors emanating from violent media may be negligible or may be mitigated by other factors that decrease violence such as intensive time use and catharsis. We estimate the elasticity of crime with respect to intensely violent game sales to be small, on an order of -0.02.

Caution should be used in applying our research design outside our sample frame as it exploits only short-run variation in weekly sales which may be different from effects in the long run. The decrease in violent outcomes we observe may still be masking possible long-run harm to society if gamers develop biased beliefs about eventual danger, or if video game play draws students out of productive schooling. Our approach also may misstate the average elasticity of games if behavioral effects from higher quality games diverge from that of lower quality games. Insofar as there are negative short-run elasticities and long-run positive elasticities of crime with respect to video games, regulation aimed at reducing violent imagery and content in games could have both social benefits and social costs.

2. Background

From the sensational crime stories of the 19th century (Comstock and Buckley 1883), to the garish comic books of the early 20th century (Hadju 2009), to the contemporary debate over violent games, Americans have always 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. Anderson and Bushman (2001) and Anderson, Gentile, and Buckley (2007) discuss hundreds of controlled studies on the effects of violence in media, whereas the number of studies on violence in print media is smaller in comparison.

Three possible theoretical mechanisms have been proposed for the impact of violent media on crime, which we label “aggression,” “catharsis,” and “incapacitation.” The aggression mechanism is based on a psychological theory called the “general aggression model” (GAM). GAM posits that violent video games increase aggressive tendencies. This model generalizes from social learning theory (Bandura 1973), script theory (Huesmann 1998), and semantic priming (Berkowitz and LePage, 1967; Anderson, Benjamin, and Bartholow 1998) through a process of social learning whereby the gamer develops mental scripts to interpret social situations before they occur. This effect creates reasoning biases, a tendency to jump to conclusions and may even cause personality disorders (Bushman and Anderson 2002). While GAM suggests that aggression increases with repeated exposure to violent content, most of the evidence for it comes from short-run laboratory experiments.

The catharsis explanation is that video games act as a release for aggression and frustration so that actual expressions of aggression are reduced. While many gamers believe the catharsis hypothesis is credible (Olson, Kutner, and Warner 2008; Ferguson et al. 2014), it is not without controversy. For instance, most cross-sectional studies fail to find cathartic effects. And while Denzler, Förster, and Liberman (2008) state rather unequivocally that the “social psychological literature lends no support for the catharsis hypothesis,” they also find that aggression can reduce further aggression in those cases when it serves to fulfill a goal. A possible physiological mechanism for catharsis comes from evidence that internet video game playing is associated with dopamine release that might act to sate the gamer (Koepp et al. 1998; Han et al. 2007). Han et al. (2009) study the similarity of the effects of video game playing and methylphenidate (i.e., Ritalin) in children with Attention Deficit Hyperactivity Disorder and suggest that internet video game playing might be a means of self-medication.

The incapacitation explanation is based on the economic theory of time use (Becker 1965). Many modern video games are time-intensive forms of entertainment involving intricate narratives with complex plots and characterizations that take dozens, and sometimes several hundreds, of hours to complete.[1] Insofar as video game play draws adolescents from other activities, the time use explanation implies a possible short-run decrease in violence if individuals substitute away from riskier outdoor leisure to indoor leisure, but allows for a possible long-run increase in violence as predicted by GAM. The American Time Use Survey (ATUS) indicates that individuals aged 15–19 spent an average 0.85 hours per weekday playing games and using computers, but only 0.12 hours reading, 0.11 thinking, and 0.67 in outdoor recreation, such as sports or exercising. Ward (2015) uses ATUS data to show that, when the currently available video games’ sales are higher, individuals spend more time gaming and less time in class. Stinebrickner and Stinebrickner (2008) find that students randomly assigned a roommate in college with a video game console studied less often, and performed worse in school.

3. Methodologies

These three explanations have separate theoretical predictions relating violent video games’ effects on violence and crime. The tests we develop have the potential of discriminating between

[1] The Web site, How Long to Beat, http://www.howlongtobeat.com, provides user-submitted statistics on completion times. The 2011 blockbuster, The Elder Scrolls V: Skyrim, lists completion times between 100 and 330 hours. The 2008 hit, Grand Theft Auto IV, lists 12 to 162 hours, with the lower bound 12 hours recorded for a “speed trial” effort to complete the game as fast as possible.

explanations. We use two separate methodologies to determine a causal link between playing violent video games and engaging in criminal activities.

Variation Over Time

We begin by estimating a standard multivariate regression model of the incidence of various crimes as a function of sales of nonviolent and violent video games. Our outcome variables of interest, C_t, are the total number of reported criminal incidents in week t across the United States that are classified as violent or nonviolent.

Video game sales are available on a weekly basis, whereas crime statistics are available at the monthly (UCR) or daily (NIBRS) level. We, therefore, aggregate crimes into monthly measures for the UCR sample and weekly measures for the NIBRS sample. A game purchased in one week is often played in subsequent weeks until the user loses interest and moves on to another game. Our main explanatory variables are aggregated current and lagged values of weekly sales volumes for all games and for violent video games in particular. The actual number of lags, T, is based on analysis of actual gaming behavior, which we discuss later.

We model the supply of criminal offenses, C_t, as:

$$\begin{aligned}\ln(C_t) = \beta^{\text{all}} \ln\left(\Sigma_{\tau=0}^{T} G_{t-\tau}^{\text{all}}\right) + \beta^{v} \ln\left(\Sigma_{\tau=0}^{T} G_{t-\tau}^{v}\right) \\ + \Sigma_w \beta^w \text{week}_t + \Sigma_y \beta^y \text{year}_t + \varepsilon_t\end{aligned} \tag{1}$$

All specifications include month-of-year fixed effects for models using UCR-based crime variables or week-of-year fixed effects for models using NIBRS-based crime variables to control for seasonality. We also include year fixed effects to account for changes in crime and game sales that vary at the year level. Thus, our identification comes from variation about the month or week's "typical" sales. We decompose video games into those with violent content, G_t^v, in addition to all games, G_t^{all}. In this manner, we hope to identify a marginal effect of GAM from violent games versus an incapacitation effect from all games. We can address the issue of game play after the date of purchase by including more lags. As we adopt a double logarithm specification, the coefficients of interest can be interpreted as elasticities. Equation 1 is estimated using the Newey–West (1987) correction for standard errors under the assumption of possible heteroskedasticity and autocorrelation.

Correlations between video game play and crime may be due to unobserved determinants of crime that are also correlated with the determinants of video game play as well as reverse causality. A low opportunity cost of time would affect both video game sales and the relative return to criminal activity (Jacob and Lefgren 2003). We account for most of the variation in the opportunity cost of time over the year with monthly or weekly dummy variables. Reverse causality may occur if higher crime rates cause people to stay indoors and play more video games. Finally, it is also possible that game publishers base their release dates on nonseasonal factors that are unobserved by the econometrician and that affect both video game demand and criminal activity.

To purge our results of endogeneity bias, we need an instrument that is highly correlated with video game sales but uncorrelated with the unobserved determinants of crime. Weekly sales of individual games are highly sensitive to both game quality (Zhu and Zhang 2010) and time on the market (Nair 2007). The first instrumental variable is the average age in weeks of the games in the current top 30 calculated from the VGChartz data. The second instrument is a measure of their average quality taken from expert review data collected from the GameSpot Web site.[2] GameSpot provides news, reviews, previews, downloads and other information for video games. The GameSpot staff reviewed all but a handful of the games in our sample and rated the quality of the titles on a scale from 1 to 10 with 10 being the best possible rank. These so-called GameSpot-scores assigned to each game are based on evaluations of graphics, sound, gameplay, replay value and reviewer's tilt, or idiosyncratic biases, and are intended to provide an at-a-glance sense of the overall quality of the game.

Insofar as expert review and game age have no independent association with the unobserved determinants of crime, the exclusion restriction should hold. While we can think of no obvious reason why either instrumental variable should be correlated with the unobserved determinants of criminal behavior, it is possible that such correlation exists. For instance, higher rated violent games may increase violence independent of the effect on sales through effects on the amount of time the games are played. We interpret our IV strategy primarily as a robustness measure.

Variation in the Marginal Effect in the Cross-Section

A second test attempts to measure a difference in marginal effects across geographic areas representing high video game demand areas. However, only the NIBRS crime data can be disaggregated geographically to the county by week level. The video game sales data are only available as a time series. Since video game launches occur simultaneously throughout the United States, disaggregated data are not available. Thus, the panel does not introduce any independent variation in the video game treatment across geographic areas. However, we expect any effect of video games to be amplified in areas of high video game demand. If violent games have a positive (negative) effect on crime, the effect should be larger (smaller) in areas where video games are consumed more. We proxy video game demand with the fraction of population aged 10–29, who we label as the youth. This is the age demographic most associated with video game playing. Our estimating equation is given by:

$$\begin{aligned} \ln C_{it} = \beta^{v} \ln\left(G_{\tau}^{v}\right) + \beta^{vn} \ln\left(G_{\tau}^{nv}\right) + \beta_{Y\text{th}}\, Y\text{th}_i + \\ \beta_{y\text{th}}^{v} \ln\left(G_{\tau}^{v}\right) \times Y\text{th}_i + \beta_{y\text{th}}^{vn} \ln\left(G_{\tau}^{nv}\right) \times Y\text{th}_i + \beta_X X + \varepsilon_{it} \end{aligned} \qquad (2)$$

We test whether the main effect is stronger for high video game demand areas by testing whether our estimates of β_{yth}^{v} and β_{yth}^{nv} differ from zero. As the tests of hypotheses exploit differences in marginal effects in the cross-section, they are relatively immune to release date endogeneity concerns. The control variables, X, include year and week-of-year fixed effects. A double log specification is retained for consistency with the preceding analysis. To avoid logarithms of zero values of crimes in small levels of geography, we aggregate counties into deciles based on their youth fraction. That is, the 10% of the sample population with the smallest youth fraction are aggregated together in the first decile, the 10% with the highest youth fraction are in the tenth decile, and the rest are aggregated into the other eight deciles accordingly. Because video game sales data do not vary across deciles, standard errors are clustered on the sample week.

[2] http://www.gamespot.com/

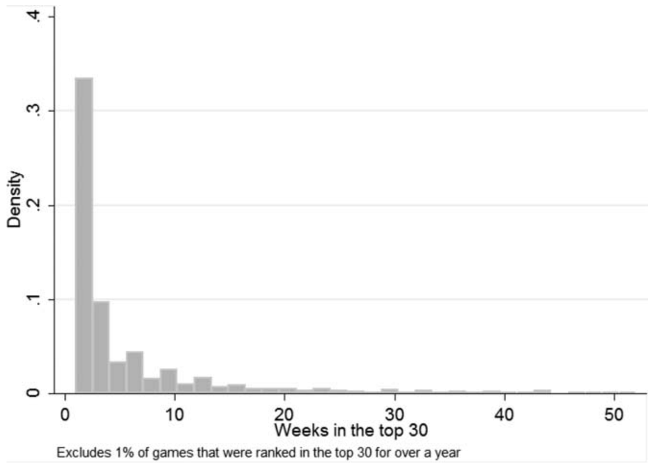

Figure 1. Number of Weeks a Game is in the Top 30 Sellers.

4. Data

A fundamental problem we face is obtaining observational data on exposure to violent video games. We base our analysis on four different video game data sources to proxy violent video game exposure, that is, Gamespot, ESRB, VGChartz, and Raptr. We explain each source in detail below. Having introduced our video game data, we follow up with a detailed explanation of our two different crime data sets.

Video Game Data

Our data on video game sales were obtained from a popular online industry outlet called VGChartz.[3] VGChartz reports U.S. retail video game unit sales for each week's top 30 selling console-based video games. VGChartz uses a variety of sources to collect data. These include manufacturer shipments, data from tracking firms, retailer and end user polls, and "statistical trend fitting." VGChartz reports by global region, for example, United States, Japan, Europe, Middle East, Africa, and Asia, but disaggregated sales within a region are not available.

We wrote a PERL script to harvest each week's top 30 titles in the United States from 2005 to 2011. The data were then cleaned and restructured to create a panel data set of weekly sales by title for the first week of January 2005 to the last week of December 2011. Our raw data set consists of 2050 separate titles spanning 364 weeks and across multiple gaming consoles and includes weekly unit sales, violent content information, and quality rating information.

Despite the richness of such high frequency sales data, VGChartz is not exhaustive. It omits a portion of all sales in the U.S. video game market as it is a truncated sample of the top 30 titles in a week. We believe that this weakness may be mitigated to some degree by the skewed distribution of sales. A game's week of release is almost always its top-selling week. Figures 1 and 2 indicate that most games stay in the top 30 for only a few weeks, and game sales decay rapidly with each week on the market. Such large turnover in game titles and variation in sales by week provide substantial variation for identification.

[3] http://www.vgchartz.com

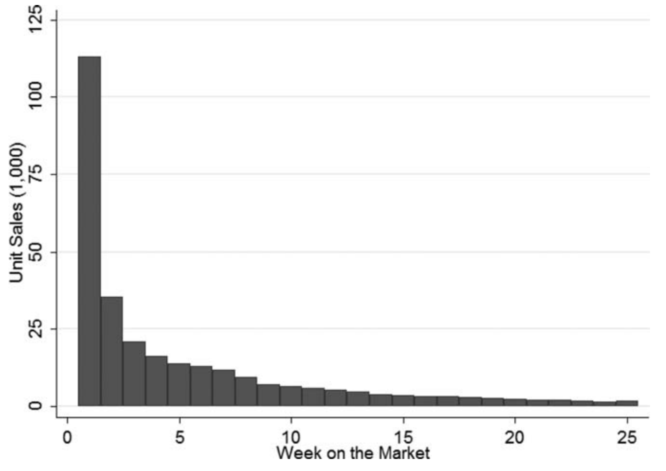

Figure 2. Average U.S. Video Game Unit Sales by Weeks After Release.

Table 1 compares VGChartz data to the Entertainment Software Association (ESA) data. While it would appear that VGChartz titles are only one-quarter of all units in 2006, it is worth noting that this is in part due to the ESA including sales of non-console-based games such as computer and smartphone games (ESA Annual Report 2010).[4] This fraction rises to about 80% in 2011, and while this raises some concerns about comparability over time, we expect some of this effect to be subsumed into the year dummies.

We record the violence content of each game using the ESRB's rating and descriptions of the game's content.[5] ESRB is a nonprofit body whose function is to assign each title a technical rating. Games considered to be appropriate for all ages are given an *E;* games appropriate for 10 and older are given *E10*; games appropriate for teens are given a *T;* games appropriate for a mature audience are given an *M;* and games appropriate for adults are given an *A*. ESRB also provides a detailed description of the content for each title including the style of violence contained in the game. Each title was successfully matched with its ESRB rating and content information. Out of 2050 titles in our sample, 512 are rated Mature and 352 were intensely violent. All intensely violent games are rated Mature.

Figure 3 depicts the logarithm of sales for all games and intensely violent games over time. Most of the variation is due to the release of new games each week that are popular initially but experience a rapid decay in sales. Although the two lines follow a similar pattern, including a large peak around the Christmas gift-purchasing period, sometimes strong sales of a single title will cause them to diverge. For instance, the spike in intensely violent game sales in the middle of 2008 is not mirrored in the more general series. This represents the release of Grand Theft Auto IV, an intensely violent and immensely popular game.

While Figure 1 indicates that consumers tend to buy a game soon after it is released, they may play purchased games over longer horizons. We have obtained data from Raptr, a video gaming social network, on over 100,000 users' gaming sessions.[6] These data cover over 10 million

[4] http://www.theesa.com—The reported numbers from ESA also include games for personal computers which amount to about 10% of the market each year and are intentionally not included in VGChartz.

[5] http://www.esrb.org

[6] http://raptr.com/

Table 1. Comparison of Unit Sales of Video Games (millions) from VGChartz and the ESA

Year	VGChartz	Entertainment Software Association	Percent (%)
2005	90.7	227.4	39.9
2006	63.8	240.1	26.6
2007	131.6	268.1	49.1
2008	187.2	298.6	62.7
2009	181.9	290.1	62.7
2010	218.4	267.4	81.7
2011	206.5	245.9	84.0

Sources: VGChartz from authors' calculations and ESA from http://www.theesa.com/facts/pdfs/esa_ef_2014.pdf.

sessions for particularly avid gaming enthusiasts. We have aggregated game play sessions by day and recorded when a user obtains a new game. Figure 4 records the number of hours played across all games around the time a new game is acquired. It is clear that, for this sample, gaming activity is highest on the day that a new game is purchased and falls for about three weeks afterward. The "scalloped" shape emerges because video games are typically released for more intensive weekend play. It also appears that individual video game play time increases just before a new video game purchase. This suggests that new game purchases are driven, in part, by a pent up demand for new video gaming experiences.

To explore the link between game sales and game playing, we related our weekly measure of video game sales to time use data on playing games. The Bureau of Labor Statistics has administered the ATUS since 2003 by taking a random sample from households that have recently completed their participation in the Current Population Survey (CPS). The ATUS records activities of a participant for a single day and collects over 1000 diaries per month with some coverage of every single day. Each activity is ultimately coded to a three-tier scheme, going from broad top-level category to finer subcategories. For our purposes, the subcategory "playing games" includes the activities "playing computer, board, or card games" and so includes activities other than playing video games. Basic demographic information on age, sex, race, household income, and household size from the CPS is available for every survey respondent.

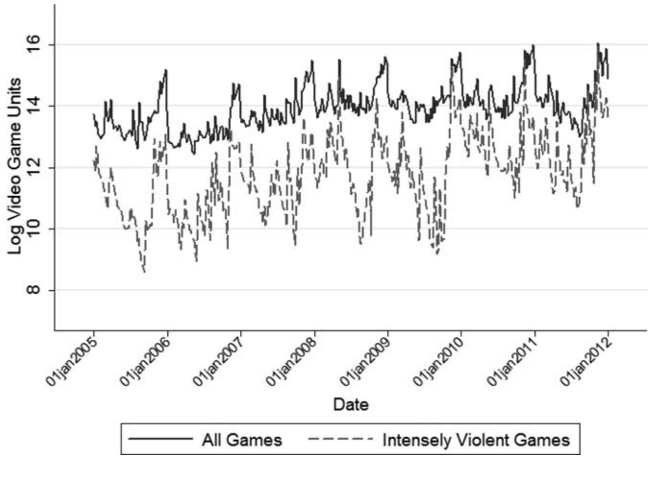

Figure 3. Ln Weekly Sales of Video Games.

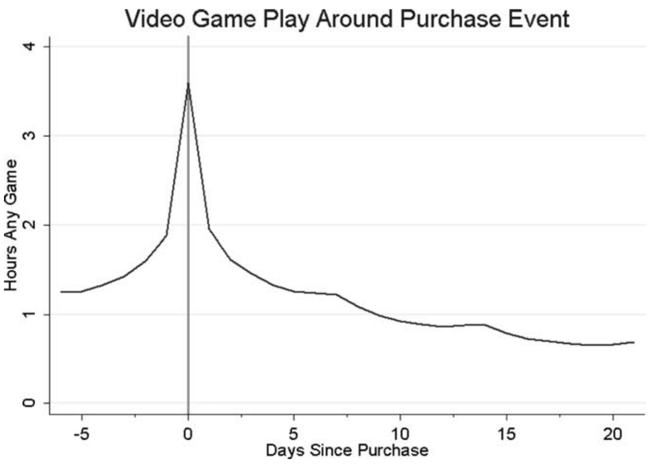

Figure 4. Time spent Playing Video Games Before and After a New Video Game Purchase.

To confirm that video game sales positively affect the probability of playing video games, we estimate Probit and Tobit regressions relating a dummy variable for playing games and the amount of time playing games to video game sales over the current and preceding weeks as well as controls for basic demographics and sets of dummy variables for year, week-of-year, and day-of-week. The details are reported in Table A1 and summarized in Figure 5. This figure reports coefficient values and confidence intervals for various leads and lags of video game sales on the probability of playing a game. While leads of video game sales appear to have no effect on game playing, game playing is affected by current sales, sales lagged one week, and, perhaps, lagged two weeks. We take this as evidence that aggregate video game playing time is correlated with video game sales for up to three weeks.

In a second step, we examine the difference in video game playing across gender and age groups. Therefore, we not only split the ATUS sample into male/female and age above or below 30 but only focus on gameplay duration. We again use the Tobit estimator in a specification that now includes the first three weeks of video game sales (Lag 0, 1, and 2) aggregated as these three weeks are the most relevant for gameplay according to the first analysis. We use the same controls as in the first estimation. The results are reported in Table A2. Overall, the marginal effects for females

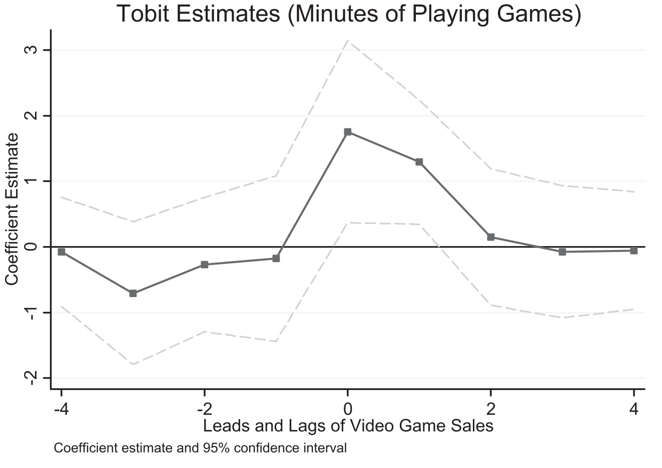

Figure 5. The Effect of Video Game Sales on the ATUS Respondents Game Playing.

are no different from that from males but the earlier table indicates that males play 40% more. The marginal effect for the younger players is larger than older players but the difference is not statistically significant. Furthermore, younger respondents play more than older ones. We interpret this as, given that a girl or a non-youth plays video games, their gaming behavior is not very different from a younger boy.

Crime Data

For our measure of crime, we use both the UCR and NIBRS. The UCR program was conceived in 1929 by the International Association of Chiefs of Police to meet the need for reliable uniform crime statistics for the nation. In 1930, the FBI was tasked with collecting, publishing, and archiving those statistics. NIBRS on the other hand is a federal data collection program begun by the Bureau of Justice Statistics in 1991 for gathering and distributing detailed information on criminal incidents from participating jurisdictions and agencies. Participating agencies and states submit detailed information about criminal incidents to the NIBRS which are not contained in other data sets, such as the UCR. For instance, whereas the UCR contains information on all arrests and cleared offenses for the eight Index crimes, NIBRS consists of individual incident records for all eight index crimes and the 38 other offenses (Part II offenses) at the calendar date and hourly level (Rantala and Edwards 2000).

Because of the detailed information about the incident, including the precise time and date of the incident, economists such as Dahl and DellaVigna (2009), Card and Dahl (2011), Jacob and Lefgren (2003), and Jacob, Lefgren, and Moretti (2007) have used NIBRS for event studies. In our case, we exploit detailed information about the date of a crime to create weekly counts. The UCR are also regularly used in economic analysis of crime by, for example, Evans and Owens (2006), Stevenson and Wolfers (2006), or Phillips and Land (2012).

One potential drawback of NIBRS compared to the UCR, however, is its limited coverage as, unlike in the UCR, only a subset of localities participates. Overall, 32 states currently participate, and many states with large markets—California, New York, DC—do not participate at all. Moreover, not all jurisdictions participate within states over time. In a first step to address selection problems, we run our estimations based on both the UCR and the NIBRS sample and compare the results. Our results are similar regardless of which data source we use. We also address possible selection problems by limiting our NIBRS sample to a balanced panel of agencies that participated with NIBRS at the start of our sample and continued each year. These agencies are located in 1082 separate counties.

Crime and game sales both follow profound seasonal patterns that could lead to spurious correlations. Seasonality in crime is largely due to weather conditions with increases in crimes during warmer months (Jacobs, Lefgren, and Moretti 2007). Game sales are greatly affected by holiday gift-giving at the end of the year. As indicated above, we accommodate these patterns with weekly dummy variables, which should capture correlation due to seasonality, and annual dummies that should capture correlation due to secular trends.

Final Samples

We investigate the relationships between violent video game sales and crime. We must drop some observations due to holiday seasonality and to construct lags. Our UCR-based data span 84

Table 2. Summary Statistics

Variable	Mean	Std. Dev.	Minimum	Maximum
Monthly UCR sample				
All video game sales (1000s)	5542	3842	1308	24,425
Intensely violent video game sales (1000s)	1203	2058	52	13,889
All crimes (1000s)	1567	187	1134	2103
Violent crimes (1000s)	634	73	492	850
The sample includes 77 monthly observations from 2005 to 2011 that exclude December.				
Weekly NIBRS sample				
All video game sales (1000s)	1192	936	252	9376
Intensely violent video game sales (1000s)	260.4	696.5	5.4	8264.80
All crimes (1000s)	47.2	4.7	34.1	55.7
Violent crimes (1000s)	18.9	1.7	14.6	22.7
The sample includes 300 weekly observations from 2005 to 2011 that exclude the eight Christmas shopping season weeks.				
Weekly by decile NIBRS panel				
All video game sales (1000s)	1138	815	252	7467
Intensely violent video game sales (1000s)	213.22	522.11	5.41	5715.99
All crimes (1000s)	4.86	3.96	0.276	15.023
Violent crimes (1000s)	1.95	1.587	0.116	6.207
Youth fraction	0.253	0.035	0.197	0.328
The sample includes 2575 week by decile observations from 2005 to 2010 that exclude the eight Christmas shopping season weeks.				

months in total from 2005 through 2011 but dropping December each year yields a final sample of 77 observations.

Our NIBRS-based data span the 364 weeks over the same period but due to dropping nine Christmas shopping season weeks and some initial weeks to construct lags, our final sample includes only 300 observations. We aggregated the VGChartz weekly observations to the month level in the UCR sample. Table 2 reports basic descriptive statistics for all three samples. There are two reasons for the large difference in magnitudes across the NIBRS and UCR data sources. The first is due to differences in the unit of observation. The UCR data measures crimes at the monthly level, whereas NIBRS measures crime at the weekly level. This also explains the differences in video game sales across the two samples. The second is due to the many more jurisdictions within the United States reporting to UCR than to NIBRS.

For our panel sample, the same weekly crime information from the NIBRS data was aggregated for each of 1082 counties along with data beginning in 2005 on the fraction of the population aged 10–29, the primary video game playing age group. From this, counties were aggregated into deciles based on this youth fraction to construct a balanced panel spanning 2005 to 2010.

Our method is most like Dahl and DellaVigna (2009), and therefore, we contrast our study with it to illustrate its strengths and weaknesses. Like Dahl and DellaVigna (2009), we do not have geographic variation in sales data. Whereas first run movies can be described as nondurables lasting two hours on average, video games have more complex consumption patterns. Unlike feature films, video games are more like durable goods played repeatedly after purchase with highly variable time use by title and individual player. Some families' budget time allowances for video game play while others allow unlimited play time. The decision to do so is likely related to the family characteristics that are correlated with the determinants of crime, such as family structure and income. Furthermore, box office movie sales are available by day whereas video game sales are only available at the weekly level.

Table 3. The Effects of Violent Video Game Sales on Crime—UCR Data

	OLS	2SLS		
	Crimes	Sales	Violent Sales	Crimes
Variables		First stage	First stage	Second Stage
Ln video game sales	−0.027**			0.036
	(0.011)			(0.034)
Ln intensely violent video game sales	−0.000			−0.021*
	(0.004)			(0.011)
Avg. video game quality		0.386	−1.772	
		(0.357)	(1.070)	
Avg. weeks on market		−0.058*	−0.114	
		(0.029)	(0.087)	
Avg. violent video game quality		0.068	0.931**	
		(0.124)	(0.372)	
Avg. violent video game weeks on market		−0.089**	−0.299**	
		(0.029)	(0.088)	
Christmas season	No	No	No	No
Year dummies	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes
Observations	77	77	77	77
R-squared	0.981	0.902	0.791	0.967

Regressions also include year and week-of-year dummy variables. Standard errors in parentheses. First-stage *F*-statistics (4, 56) for the excluded variables are 5.10 and 5.27 which both have *p*-values less than 0.01. The $\chi^2(4)$ value for the AR weak instrument test is 7.78 with a *p*-value less than 10%.The Sargan statistic for the IVs is 1.612 with a *p*-Value (χ^2) of 0.45.
**$p < 0.01$.
*$p < 0.05$.
+$p < 0.1$.

5. Results

Variation Over Time

Our basic OLS regression results of the estimation of Equation 1 are presented in Table 3 for the UCR data and Table 4 for the NIBRS data. Each table reports both OLS and 2SLS estimates of specifications for three weeks of accumulated sales of video games sales on all crimes.[7] These specifications also exclude observations that would be affected by the Christmas gift shopping season, specifically month 12 in the UCR sample and weeks 47–52 and 1–3 in the NIBRS sample, because the time lag between purchase and playing likely differs.

The first column in each table, reporting the OLS estimates, indicates a small but statistically significant decrease in crime when video game sales are higher. The estimates are very similar for both crime data samples and indicate that the elasticity is on the order of −0.027 (UCR sample) or −0.022 (NIBRS sample). In other words, a doubling of video game sales is associated with a 2–3% decrease in crime. The estimated effect of violent video game sales is not significantly different from zero. While the negative effect from all games is consistent with incapacitation, the lack of a measurable effect from violent games does not provide evidence for either catharsis or GAM.

[7] Estimates were also generated for each of one to six weeks of accumulated video game sales "exposure." All results are qualitatively similar with estimates from more than three weeks becoming less precisely estimated.

Table 4. The Effects of Violent Video Game Sales on Crime—NIBRS Data

	OLS	2SLS		
	Crimes	Sales	Violent Sales	Crimes
Variables		First stage	First stage	Second Stage
Ln video game sales	−0.022**			0.007
	(0.007)			(0.017)
Ln intensely violent video game sales	0.000			−0.018**
	(0.002)			(0.006)
Avg. video game quality		0.006	−0.259**	
		(0.024)	(0.078)	
Avg. weeks on market		−0.013**	−0.011**	
		(0.002)	(0.006)	
Avg. violent video game quality		0.012	0.151**	
		(0.009)	(0.029)	
Avg. violent video game weeks on market		−0.008**	−0.040**	
		(0.002)	(0.006)	
Christmas season	No	No	No	No
Year dummies	Yes	Yes	Yes	Yes
Week dummies	Yes	Yes	Yes	Yes
Observations	300	300	300	300
R-squared	0.943	0.846	0.682	0.927

Regressions also include year and week-of-year dummy variables. First-stage F-statistics for the excluded variables are 21.8 and 20.2 which both have p-values less than 0.01. The $\chi^2(4)$ value for the AR weak instrument test is 31.4 with a p-value less than 1%. The Sargan statistic for the IVs is 10.19 with a p-Value (χ^2) of 0.01.
Standard errors in parentheses.
$^{**}p < 0.01$.
$^{*}p < 0.05$.
$^{+}p < 0.1$.

The next three columns in Tables 3 and 4 report the first and second stage results for the 2SLS estimator and once again feature very similar results for each crime data source. There are two instrumented variables and four instrumental variables. In the first stage, video game sales fall with the average age of all games in the top 30 as well as with the average age of all violent games. Sales of violent games are higher with higher quality violent games and lower with the average age of violent games. This relationship is slightly different for the NIBRS sample, in which both average video game quality and age have negative marginal effects on violent game sales. We present standard F-statistics on the excludability of the instruments from the first stage regression. The correlations between the instruments and our endogenous variables are strong in the NIBRS data, although (F-statistic around 20). In the second stage, the estimated effect of video game sales is positive but not significantly different from zero. For violent video games, the estimated effect is around −0.02 and significantly different from zero regardless of which crime data source is used. In Table 3, we pass the over-identification test based on the Sargan test statistic, but we fail to pass the over-identification test in the NIBRS regression. We also test for whether we have a weak instrument problem using both the F-test on the excludability of the instruments from the first stage, as well as the Anderson–Rubin test. The p-values on the Anderson–Rubin test are both statistically significant. Thus, while our IV estimation largely supports the main results, it should be interpreted with some caution. With that caveat, the negative effect from violent games is consistent with either incapacitation or catharsis, but does not support GAM.

Table 5. Panel Regression of the Differential Effect of Video Game Sales in High Youth Counties—NIBRS Data

	Crimes	Crimes
Ln video game sales	-0.011^{+}	-0.005
	(0.007)	(0.008)
Ln video game sales × youth fraction	-4.302**	-3.821**
	(0.233)	(0.298)
Ln intensely violent video game sales		-0.004^{+}
		(0.002)
Ln intensely violent video game sales × youth fraction		-0.342*
		(0.160)
Youth fraction	85.199**	80.138**
	(3.176)	(3.686)
Christmas season	No	No
Year dummies	Yes	Yes
Week dummies	Yes	Yes
Observations	2570	2570

The panel was created by aggregating counties into deciles based on the fraction of the population aged 10–29 years. Regressions also include year and week-of-year dummy variables. Standard errors clustered at the week level.
Robust standard errors in parentheses
$^{**}p < 0.01$.
$^{*}p < 0.05$.
$^{+}p < 0.1$.

Differential Marginal Effects

It is likely that any effect of video games will be localized where gaming demand is higher. Ward (2011) exploited this idea by relating changes in various crimes in a county from year to year to changes in the number of video game stores. As mentioned before, we do not have cross-sectional variation in video game sales. Instead, our strategy focuses on differential gaming patterns by age. Video game playing is more popular among adolescents and young adults. We use this empirical regularity to test whether there is a larger marginal effect of video game sales in areas with greater "youth" prevalence as explained in the second part of the methodology section. The youth fraction which we measure as the fraction of the population aged 10–29 years has a sample mean of 0.261 with a standard deviation of 0.042. We focus our attention on the estimates of the interaction terms, β^{v}_{yth} and β^{nv}_{yth} from Equation 2, to measure a differential marginal effect.

We estimate Equation 2 using simple OLS including fixed effects for year and week-of-year. Results are reported in Table 5. Again, we omit the Christmas gift shopping season, report only those variables of interest and for sales aggregated to three weeks. The positive coefficient of the uninteracted fraction youth population variable indicates that there is more crime in areas with a higher proportion of youth. For ease of interpretation, the youth fraction enters as a deviation from the sample mean. This allows us to interpret the uninteracted video game coefficients as applying to the average county. Thus, in column 1, for the average county, the video game sales variable has no discernable effect on crime. The coefficient for the interaction term with the youth population is negative which suggests that, in areas with more video gamers, more popular games lead to marginally fewer crimes. This is consistent with a greater incapacitation effect where there are more video game players.

Again, the GAM and catharsis hypotheses relate specifically to violent video games. To test this hypothesis, we include violent game sales and its interaction with the youth fraction in column 2. The coefficient for the interaction term for all video games is not only negative but only marginally statistically significant. The coefficient for the interaction term for violent video games is also negative and statistically different from zero. These results are consistent with either incapacitation or catharsis. They are not consistent with GAM.

6. Conclusion

Regulation of the content of video games is usually predicated on the notion that the industry generates large and negative social costs through games' effect on aggression. Many researchers have argued that these games may also have caused extreme violence, such as school shootings, because of the abundance of laboratory evidence linking violent media to measured psychological aggression. Yet to date, because the field has not moved beyond suggestive laboratory studies, we argue their external validity to understanding the impact on crime is limited. With the exception of Ward (2011), social scientists have yet to move beyond the laboratory to understand whether concerns about game violence's causal effect on crime are warranted.

Similar to Dahl and DellaVigna (2009) our study suggests the evidence that violent video games have substantial social costs is weak. In fact, our study finds that in the weeks following popular video game releases, crime rates decrease. More research is needed to determine if violence in media may have social benefits by reducing crime. As with the above two studies, we find that the short- and medium-run social costs of violent video games may be considerably lower, or even nonexistent. The measured effect stemming from intensely violent video games, and not from non-violent games, is consistent with catharsis and not with incapacitation. However, this effect is not found in all specifications.

Our results do not completely rule out GAM. Most theories in GAM suggest that long-term exposure to violent media increases aggression, whereas our tests measure relatively short-term responses to video game violence. It is possible, that a positive GAM effect is large enough in the long-run to dominate the negative effect we estimate for violent video games in the shorter-run. The case for regulatory intervention depends on whether any or all of these effects apply. Insofar as positive and negative effects of violent media are at work simultaneously, the optimal design of an intervention that reduces harm from one without raising harm from the other may prove a challenge. Weighing possible competing effects on crime is a problem that has been raised by other papers in other contexts such as violent movies (Dahl and DellaVigna 2009) and youth gangs (Sobel and Osoba 2009). While some early work has been done on the long-term effects of video game play, nearly all the laboratory evidence that currently exists has only uncovered very short-term effects.[8]

Our findings also suggest unique challenges to game regulations. GAM proposes that the individuals playing violent video games are developing, accidentally, a biased belief toward people wherein they believe they are in danger. It is possible that the decrease in violent outcomes that we observe in our study, possibly due to short-run catharsis or incapacitation, is masking the long-run harm to society if these violent behaviors are developing within gamers. This suggests that

[8] Anderson (2004) notes the lack of longitudinal studies of effects of violent video games on aggression and calls for more studies aimed at investigating the long-term effects. The best evidence we have at present from laboratory studies is primarily short-run, making our study more suitable for comparison.

regulation aimed at reducing violent imagery and content in games could in the long-run reduce the aggression capital stock among gamers, but potentially also cause crime to increase in the short-run if the marginal player is currently being drawn out of violent activities. This tradeoff may not pass a cost-benefit test.

A related policy question centers on whether reducing violent content of video games so as to diminish GAM related aggression effects also would diminish any time use and cathartic effects. Presumably, publishers include content that is violent because there is a market niche that demands it. They believe that removing the violence would lower profits because it would reduce these gamers' willingness-to-pay. It is not clear how much time use might fall, but lower utility from such games would reduce game demand and game play time by some amount. The ability to craft a regulation restricting violent content that does not also lower consumer utility seems remote.

Using our approach, in some specifications, we find a negative relationship between video game sales and crime that generates an elasticity of about -0.02. As our research design exploits short-run variation in weekly sales, caution should be used in applying it outside our sample frame. For instance, if behavioral effects from popular, higher quality games diverge from that of popular, lower quality games, then our approach may misstate the average elasticity of games independent of quality. Furthermore, our estimates are exclusively based on short-run variation in sales, which may be different from effects in the long-run. For instance, the substitution out of schooling to video gameplay as Stinebrickner and Stinebrickner (2008) and Ward (2015) show might imply that long-run effects of violent games on crime are positive by reducing human capital and wages (Grogger 1998). With this caveat, we use this elasticity to construct a simple counterfactual for U.S. crimes from 2005 to 2011.

To provide context for the magnitude of our estimated effects, we consider a simple back-of-the-envelope calculation using the numerical growth in video game sales over our sample period. From Table 1, video game unit sales as reported by the ESA increased by 8.1%. Assuming this applies to both violent and nonviolent games, an estimated violent video game-to-crime elasticity of approximately -0.02 would predict almost 0.16% fewer violent crimes due to violent video game sales. Nationwide, this would translate to almost 36 fewer violent crimes committed per week. By comparison, the estimated incapacitation effect from Jacob and Lefgren (2003) of 13.3% more property crimes due teacher in-service days, would translate into about 2300 property crimes for a hypothetical national in-service day.[9] Since the video game effect occurs year round, this suggests that video games are disrupting, at most, 2000 crimes per year.

This approach can help guide investigators to develop more holistic research designs, such as field experimentation and other quasi-experimental methodologies, to determine the net social costs of violent games. The main shortcoming of our approach is due to the limitations of our data on game sales. Unfortunately, the industry does not report cross-sectional variation in game sales—only the national weekly sales of the top 30 highest grossing games are available.

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[9] Our calculation is based on a total of over 1.2 million violent crimes where the calculation of Jacob and Lefgren (2003) is based on 6.2 million annual property crimes. Both type of crimes are reported in the FBI's "Crime in the United States," http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/10tbl01.xls.

Appendix

Table A1. Estimates of Factors Affecting Playing Games from ATUS

	Probit Any Game Playing		Tobit Hours Playing Games	
Video game sales				
Four-week lead	−0.031	(0.098)	−0.074	(0.425)
Three-week lead	−0.161	(0.130)	−0.705	(0.555)
Two-week lead	−0.044	(0.130)	−0.270	(0.522)
One-week lead	−0.032	(0.155)	−0.179	(0.644)
Current week	0.444***	(0.171)	1.756***	(0.708)
One-week lag	0.242**	(0.120)	1.294***	(0.483)
Two-week lag	0.072	(0.130)	0.151	(0.530)
Three-week lag	−0.034	(0.126)	−0.073	(0.514)
Four-week lag	−0.073	(0.112)	−0.056	(0.457)
High school student	0.007	(0.042)	−0.068	(0.179)
College student	−0.078***	(0.030)	−0.428***	(0.131)
Male	0.065***	(0.013)	0.430***	(0.055)
Black	−0.235***	(0.020)	−0.994***	(0.089)
Asian	−0.238***	(0.037)	−0.977***	(0.162)
Hispanic	−0.409***	(0.021)	−1.771***	(0.093)
Usual work hours	−0.004***	(0.000)	−0.019***	(0.002)
Married	−0.110***	(0.026)	−0.567***	(0.113)
Unmarried household	−0.043*	(0.025)	−0.188*	(0.108)
Household size 2–3	0.058**	(0.027)	0.286**	(0.118)
Household size 4+	0.046	(0.030)	0.181	(0.128)
Age dummies	*X*		*X*	
Income dummies	*X*		*X*	
Day of week dummies	*X*		*X*	
Week of year dummies	*X*		*X*	

Standard errors clustered on the observation week. The sample includes all 87,831 non-Christmas season observations.
***$p < 0.01$.
**$p < 0.05$.
*$p < 0.10$.

Table A2. Differential Estimates in Minutes of Gaming by Gender and Age from ATUS

	Male	Female	Age <=30	Age >30
Video game sales sum of Lags 0,1,2	0.670***	0.752***	0.993***	0.645***
	(0.226)	(0.177)	(0.319)	(0.191)
High school student	0.085	−0.267	0.019	−0.529
	(0.237)	(0.291)	(0.194)	(1.200)
College student	−0.376*	−0.121	−0.474***	−0.004
	(0.200)	(0.152)	(0.172)	(0.213)
Male			2.749***	−0.464***
			(0.122)	(0.062)
Married	−0.835***	−0.059	−0.430**	−0.269***
	(0.138)	(0.091)	(0.190)	(0.094)
Working	0.036	−0.076	0.252	−0.354
	(0.229)	(0.171)	(0.187)	(0.234)
Black	−0.602***	−1.128***	−0.372**	−1.150***
	(0.139)	(0.109)	(0.165)	(0.099)
Asian	−0.760***	−1.303***	0.199	−1.792***

Table A2. (*Continued*)

	Male	Female	Age <=30	Age >30
	(0.242)	(0.216)	(0.266)	(0.214)
Hispanic	−1.747***	−1.778***	−1.453***	−1.968***
	(0.143)	(0.122)	(0.161)	(0.121)
Income dummies	X	X	X	X
Age dummies	X	X	X	X
HH size dummies	X	X	X	X
Day-of-week dummies	X	X	X	X
Week of year dummies	X	X	X	X
Observations	38,365	49,808	17,899	70,274

Tobit regressions from the American Time Use Survey (ATUS). Observations during the Christmas shopping season are excluded. Standard errors are clustered at the week level. Robust standard errors in parentheses.
***$p < 0.01$.
**$p < 0.05$.
*$p < 0.1$.

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