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"Cutting class to play video games"

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ABSTRACT

Video games represent a class of new leisure activity that makes use of advances in information technology. These increasingly popular pastimes can crowd out time spent on other activities. I exploit week-to-week variation in video game popularity to identify variation in video game playing time likely due to changes in game quality rather than to individuals selecting into gaming. I find that when video game sales increase, students spend more time playing games, and less time attending class and doing homework. Differential effects for college students and those with lower incomes indicate large effects for these groups. Newly developing ICT based pastimes, such as use of online social media, could have similar effects.

1. Introduction

As video gaming grows in popularity it has become an increasingly time consuming leisure activity. Increased time spent playing must crowd out time in some other activity. Since video games are especially popular with adolescents and young adults, some of this time could come from educational activities. This paper examines the degree to which time spent playing video games crowds out human capital accumulation.

The specific issue addressed here is whether students substitute class time or homework time for video game playing time when more desirable video games are available. Video game players tend to devote relatively more time playing video games when current games are more popular. Using time use survey data, I show that this additional video game playing time tends to crowd out time attending classes and, to a lesser extent, time doing homework. Moreover, the crowding out effect tends to be larger for males, lower income students, and for college students versus high school students, possibly due to either less parental supervision or greater class schedule flexibility. These findings indicate when intermediation policies to mitigate this time substitution pattern might have a larger impact.

Many entertainment options can vie for student attention. For example, tardiness and absenteeism are said to result when televised sporting events of local teams go into overtime on the previous night. Much of the increase in the popularity of video gaming is due to greatly improved quality brought about by advances in information and computer technologies.[1] Video game players spend a considerable amount of time playing any one game. A typical game may take 40 hours or more of game play before it has been beaten.[2] This gaming time could crowd out time spent on educational activities.[3] An important characteristic of video games is the considerable time variation in the value of playing games. Because individual games differ in quality and tend to have short lifecycles, week-to-week variation in the popularity of current games can lead to large week-to-week variations in the time spent gaming. I exploit intertemporal variation in the value of playing video games to measure the magnitude of the crowding out effect of video games on educational inputs. This method is likely to reveal a causal effect that is not related to selection of marginal students into video game playing. The identifying assumption is that educational inputs are not affected by high video game sales directly but are only indirectly affected by sales through the substitution into video game playing time.

The data on time spent playing games, attending class, and doing homework come from the American Time Use Survey (ATUS).

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[1] Total sales of video games, computer games, and in-game purchases in the US were $15.4 billion in 2014, more than double the amount from a decade earlier (Source: Essential Facts About the Computer and Video Game Industry, the ESA). In contrast, US gross box office sales of first-run movies amounted to $10.4 billion in 2014, a ten percent increase from a decade earlier (Source: Box Office Mojo).

[2] Howlongtobeat.com

[3] For example, Cabane et al. (2016) find that adolescent participation in music and sports crowds out other leisure activities, including computer games, and is associated with higher educational attainment.

For the period from 2005–2012, the ATUS includes complete diaries of over 100,000 total respondents' time use over a given 24 hour period. The ATUS includes over 100 separate possible activities on which respondents' classify their time use over a single day. These activities include time spent playing games, attending class, and doing homework. The ATUS contains respondent diaries that cover every day over the nine year period but it does not constitute a panel because each respondent is interviewed only once. It is linked to the Current Population Survey making it possible to control for other individual or family demographic characteristics which may confound interpretation.

To address the selection issue, I adopt an identification strategy that uncovers a plausibly causal relationship operating through variation in game popularity and in other activities. This is accomplished by merging game popularity information. Specifically, I first show that time spent playing games by ATUS respondents is positively related to the volume of sales of video games in the current and previous week. This is consistent with other research finding that gaming activity spikes with game purchases and tapers off quickly (Engelstätter and Ward, 2016) When the quality of current games is rated higher and perceived to be better, more gamers purchase them and spend more time playing them. This generates variation in time spent gaming that tends to be uncorrelated with the gamer's educational characteristics. I then link this variation in gaming to variation in time spent on two educational inputs: class attendance and homework. This way, the effects of time spent gaming are less likely to result from selection of poorer students into gaming.

The results indicate decreases in school attendance and time doing homework due to video game playing. When video game sales are particularly high, ATUS respondents are more likely to engage in gaming, are less likely to attend class, and may spend less time on homework. A one standard deviation in video game sales leads to an average reduction in class time of about 16 minutes which corresponds to nearly a 10%% reduction. Video game time is consistently estimated to decrease homework time but this result is smaller and not always statistically significant. The marginal effect of gaming on homework by males is larger than for females but the effect on class attendance is not different from females. The crowding out of class time effect is larger for students from lower income households. Video game playing may crowd out class attendance more for college students than high school students, possibly due to less parental oversight. However, this also may reflect their ability to shift playing to days with fewer classes.

School absenteeism from video games could have large educational effects. Marburger (2001) finds that students who are absent from a particular college class meeting are more likely to miss exam questions on material covered that day. In a randomized experiments, Marburger (2006) finds that a college class with a mandatory attendance policy increases attendance and course performance while Radhakrishnan et al. (2010) find that greater incentives to do homework improves course performance. Dobkin et al. (2010) use a regression discontinuity approach on required class attendance to find that attendance leads to better course performance. Studies finding a video game to learning association often suffer from selection issues (Cummings and Vandewater, 2007 and Swing et al., 2010). However, Stinebrickner and Stinebrickner (2008) find that college performance falls for previously non-gaming students whose dormitory roommates bring gaming consoles to college. Longitudinal data on younger children indicate small decreases in time spent studying due to increases video game playing and television viewing (Nakamuro et al., 2013) but that video games may improve some non-cognitive skills (Suziedelyte, 2015). Finally, there is growing evidence that educational outcomes suffer from increased computer access likely because of increased access to entertainment options (Woessmann and Fuchs, 2004, Angrist and Lavy, 2002, Belo et al., 2015, and Malamud and Pop-Eleches, 2011).

Video games are just one of the growing entertainment uses of ICTs. Time spent on social media is also rising steadily. It is possible that these other uses also crowd out educational activities. Parents who want to foster the education of their children may want to impose restrictions on their children's video game usage as well as other ICT based entertainment activities. The situation is not too different from parents restricting their children's television viewing over the past half century.

2. Empirical methodology

It is possible to link video game time to educational inputs as simple conditional correlations, as shown below. However, a potential negative association between video game play and time devoted to learning may be due to selection of individuals with different preferences for learning as well as a causal result of crowding out. It is possible that marginally performing students are less attached to school and invest less in human capital. Marginally performing students also may have a preference for video game playing. Even without a difference in preferences, they may allocate some of the time freed up from reduced participation in educational activities toward video game playing. In both cases, we would expect a negative correlation between gaming and educational inputs. However, in either case, video game playing is the result of other factors and not the cause of lower investments. I adopt an instrumental variables methodology that should be free of these confounding factors.

I construct an instrumental variable from video game popularity. When the currently available games are perceived to be higher quality, the utility from playing video games rises. This is a temporary increase because the attractiveness of video games tends to fall quickly with cumulative time played. This temporary increase in marginal utility can result in large swings in the sales of video games from week to week. Thus, week-to-week variation in video game sales will be a valid instrumental variable if it affects time spent playing video games but has no direct effect on time spent on educational activities.

Parameter identification comes from the exclusion restrictions on video game sales. The specification becomes:

$$Educ\ Time_i = \alpha \widehat{VG\ Time}_i + \beta X_i + \epsilon_i \tag{1}$$

$$VG\ Time_i = \gamma VG\,Sales_i + \delta X_i + \nu_i \tag{2}$$

The test of hypothesis is whether time devoted to educational activities is affected by the projection of video game time on video game sales or whether $\hat{\alpha} < 0$. Because the time spent in an activity on any given day is truncated at zero, both equations are estimated as Tobit models. Truncation is particularly severe because, on any given day in the final sample, about one-third of students do not attend class and about 85%% do not play games in the final sample. To avoid bias in the estimation of standard errors due to generated regressors in the second stage estimation, estimates and standard errors are bootstrapped.

The instruments should not suffer from the "weak instrument" problem. Video games have short life cycles as new games are constantly being introduced and quickly fade in popularity. Moreover, video game quality is highly heterogeneous leading to considerable week-to-week variation in the desirability of the games currently available. When the currently available games are more popular, as indicated by recent sales volume, the amount of time spent playing games is expected to increase. This week-to-week variation in game playing is likely to stimulate demand and not merely induce selection of those pre-disposed to gaming and, possibly, lower human capital investments.

However, this method is not without problems. Time variation in the sales of video games is likely to satisfy the exclusion restriction requirement. That is, there is not likely to be alternative route by which video game sales could affect human capital investments except through video game playing. However, this measure may affect different types of students differently, a potential violation of the equal treatment assumption. Currently available popular video games may induce lower performing students to spend more time gaming. Nevertheless, the instrumental variable specification should reduce the bias due to selection.[4]

There are a few other issues with this method. First, since college students' class schedules often differ from day to day, they may allocate their game playing time toward days in which they have fewer classes. This could contribute to a negative observed relationship between game playing and class attendance even if students are not cutting class. In later specifications, I examine a difference in effect size for college students. Second, gamers who extend their playing time into the evening and night hours may be prone to decrease educational inputs on the following day. If a respondent has recently obtained a new video game, he likely will have increased his video gaming time over the previous and next day as well as the observed day. Since ATUS diaries document time spent over a 24 hour period beginning when they rise in the morning, they miss the effect of yesterday's playing on today's educational inputs. This means that the time recorded playing video games likely underestimates the time spent due to new game acquisition.

3. Time use and video game sales data

The primary data used is the American Time Use Survey (ATUS). A growing literature that is based on time use data tackles many disparate subjects. Connolly (2008) examines labor-leisure tradeoffs from inclement weather. Aguiar and Hurst (2007) show that a doubling of time spent shopping reduces prices paid by 7–9%%. Kalenkoski et al. (2007) examine how family structure affects the time mothers and fathers spend on primary and passive child care and on market work. Sen (2012) shows that increased costs of driving due to higher gasoline prices can increase the physical activity levels of those affected. Price (2008) finds that first-borns receive more quality time with parents than second children. Lazzaro and Frateschi (2017) and Muñiz et al. (2017) examine arts and culture participation using Italian and Spanish time use data.

The ATUS was begun by the Bureau of Labor Statistics in 2003 and has been updated continuously since then. The ATUS uses a random sample drawn from households that have recently completed their participation in the Current Population Survey (CPS). Sample households are selected based on the characteristics of the CPS reference person, and the respondent is then randomly selected from the list of adult (defined as age 15 or older) household members. All qualifying household members have the same probability of being selected. The ATUS collects over 1000 diaries per month with some coverage on every single day. The ATUS respondent describes each activity (such as sleeping or traveling to work) over a 24 hour period to an interviewer which is ultimately coded to a three-tier scheme, going from broad top-level category to finer sub-categories (e.g. travel to work belongs to a finer category than travel). For each episode, the ATUS collects the beginning time and the ending time of the activity.

This study primarily uses the data on time spent gaming, attending class, and doing homework over a period spanning 2005–

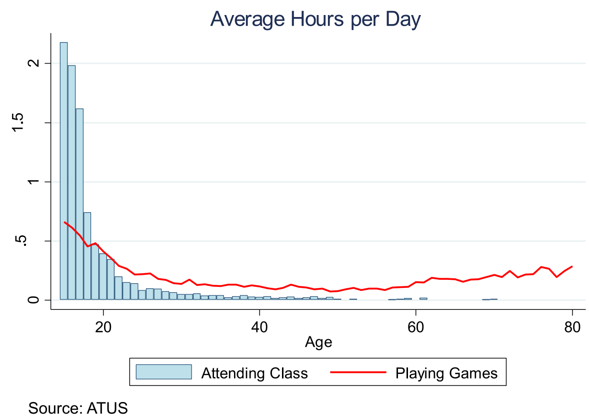

Fig. 1. Time spent playing games and attending classes by age.

2012. The time coded as playing games includes "playing computer, board, or card games" and so is broader than playing video games. In fact, Fig. 1 indicates that, while the hours spent playing games falls after the teen years, it rises again once respondents reach 60. This later increase likely does not include video game playing. In this case, not only does time spent playing games measure video game playing with error, the error is correlated with age and thus potentially with educational inputs. If unaddressed, these correlations would tend to bias estimates of α toward zero. However, the sample used in the estimations includes only those identified by the CPS as enrolled in high school or college. This subsample is almost exclusively made up of younger respondents whose gaming activity more likely involves video games. For example, while this subsample represents 10%% of all respondents, it represents nearly 99%% of class time and over 90%% of homework time.[5] Moreover, the identification strategy outlined above uses the projection of time playing games on video game sales which will tend to be dominated by time playing video games.

Since the American Time Use Survey uses the Current Population Survey (CPS) as a sampling frame, the ATUS data files contain the same demographic information as the CPS. Information on age, sex, race, household income, and household size from the CPS is used to control for other factors that might affect video game playing or school attendance. Because video game sales, and likely video game playing, follow strong seasonality patterns, week-of-year dummy variables are included in all specifications. Thus, identification is derived from deviations across years around typical sales and video game playing for a given week in a year. Finally, because there may be within week cycles in the opportunity costs of time, dummy variables for day-of-week are also included.

As mentioned above, the sample is derived so as to focus on those "at risk" of attending class or doing homework. My analysis focuses on respondents who the CPS indicates as "in high school" or "in college" which forms a sample of 9999 observations. I also exclude observations during summer when school is typically not in session and as well as observations during the weeks from Thanksgiving until the end of the calendar year, including weeks when school is typically in session. This is because video game purchases during this period are often intended as holiday gifts and so will not affect time use until well after the purchase. These broad seasonality exclusions reduce the sample size to 6779. I exclude observations that fall on holidays and those that fall on Saturdays and Sundays, which are over sampled, when school is typically not in session. These conditions further reduce the sam-

[4] For example, unequal treatment might occur if more 'hardcore' gamers have a greater response to a game's popularity. As a possible check, unreported quantile regressions detect no significant differences in the marginal effects of game popularity on gaming time at the 25, 50 and 75 percentiles of video game players.

[5] The variable for class time is defined as the time "Taking class for degree, certification, or licensure," which can include some adult education.

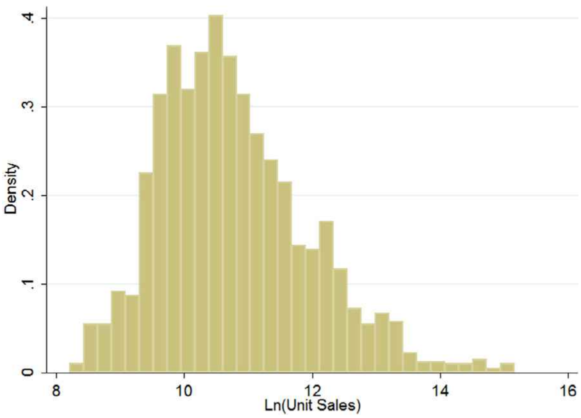

Fig. 2. Histogram of sales of video games during the initial week on the market.

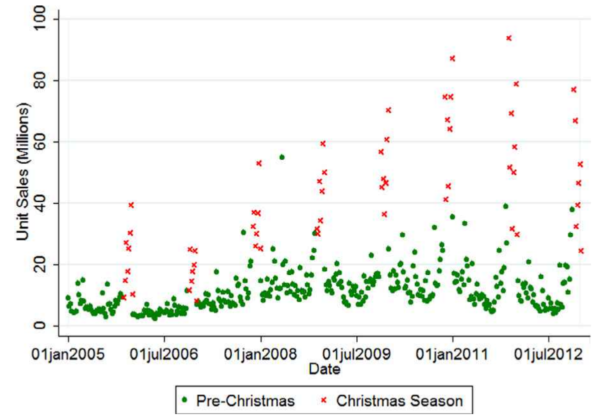

Fig. 3. US weekly video game sales.

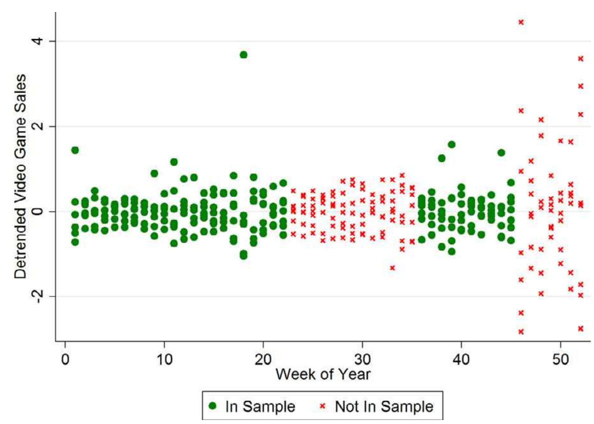

Fig. 4. US weekly video game sales de-seasoned.

ple size to 3319. Finally, since video game sales data are not available for the first weeks of 2005, omitting these brings the sample to 3016.

The video game sales instrumental variable is derived from US video game unit sales data from VGChartz (www.vgchartz.com). Beginning in 2005, this site has provided consistent unit sales volume information for each of the top 30 selling console based video games each week. The week is a natural unit of time in this market as games tend to be released late in the week for more intensive weekend play. Games ranked 31st or lower in a week are likely to represent a small portion of the level of total sales and an even smaller portion in the variation of the change in sales from week to week.[6] While console based games represented the dominant portion of video game usage, this measure excludes computer and mobile phone based video games. However, to be a valid instrument, the measure need not be comprehensive but need only be correlated with a portion of video game playing time.

Three aspects of video game demand generate week-to-week variation. First, there is a constant flow of new games to the market. During the sample period, 2005–2012, the VGChartz dataset contains nearly two thousand different game titles over the 364 weeks or about five new product launches per week. Second, video game consumption typically is highest upon game release and fades quickly. In a sample of avid gamers, Engelstätter and Ward (2016) find that game sales and duration of game play are highest the week it is released with the duration of play for a specific game falling by about 10%% each subsequent week. Third, games are quite heterogeneous in popularity. Fig. 2 depicts a histogram of the logarithm of the unit sales of games during their initial week on the market. The 75 percentile game has almost five times the sales as the 25 percentile game. These aspects of the market tend to generate large intertemporal variation in aggregate games sales that would lead to variation in the amount of time gamers spend playing games.

One confounding issue clear from Fig. 3 is the extreme seasonality of video game sales especially during the Christmas shopping season. Fig. 4 suggests that de-seasoning by regressing video game sales on week-of-year dummy variables can account for most of the anticipated seasonal variation in video game sales.[7] However, two other considerations for the estimation arise due to video games purchased during the Christmas shopping season. First, even the de-seasoned sales data appear to have a larger variance during the Christmas shopping season. Second, since these purchases tend to be for later gift giving, they typically are not played until weeks after purchase implying that the lag structure may be longer and more variable than for purchases in other times of the year. Because of this, a different pattern of coefficients for lags for the Christmas shopping season can be expected than for the rest of the year. These concerns lead to the exclusion of observations for weeks 47–52, even if school might be in session. Table 1 reports summary statistics for the student population.

4. Results

The estimation procedure requires identifying the appropriate lag structure for recent video game sales affecting the probability of an ATUS respondent reporting that she spent time playing games. This is done by including leads and lags of up to four weeks. It is expected that only past sales affect current gaming, implying that coefficients on leads should be zero. At the same time, even though gamers can play out of their inventory of previously purchased games, they tend to play the most recent games. Thus, we expect that only a few lags of weekly sales would determine variation in time spent playing video games. Fig. 5 depicts the coefficients and 95%% confidence intervals from a Tobit specification of all ATUS respondents' game playing time that includes four leads and lags as well as the current week's value of video game sales. Detailed results underlying this figure are reported in Table 2. As expected, leads are never statistically significantly different from zero. The coefficient for the current week is the largest with values declining for longer lags, although only the coefficients for the current and preceding week are statistically sig-

[6] Because video game sales are concentrated among the newest games, sales volumes are highly skewed. For example, weekly sales of the top ranked game averaged 330,000 units while sales of the 30th ranked game averaged 18,300 units.

[7] The large positive residual in week 18 corresponds to the release of Grand Theft Auto IV, the most successful game released during this period. The estimates reported below are not qualitatively affected by this outlier.

Table 1
Summary statistics of students within ATUS.

Variable	Mean	Std. Dev.	Min	Max
Any game playing	0.127	0.333	0	1
Any class attendance	0.573	0.495	0	1
Any homework	0.470	0.4999	0	1
Hours of game playing	0.256	0.914	0.000	11.483
Hours of class attendance	3.087	3.088	0.000	12.917
Hours of homework	1.099	1.757	0	13.167
Video game sales (10 millions)	0.121	0.073	0.025	0.548
Video game sales over two weeks (10 millions)	0.246	0.139	0.053	1.227
High school student	0.459	0.498	0	1
Male	0.434	0.496	0	1
Black	0.148	0.355	0	1
Asian	0.048	0.214	0	1
Hispanic	0.168	0.374	0	1
Usual work hours	12.456	17.701	0.000	95.000
Married	0.184	0.388	0	1
Unmarried household	0.285	0.452	0	1
Household size 2–3	0.377	0.485	0	1
Household Size 4++	0.530	0.499	0	1
Age 15–18	0.481	0.500	0	1
Age 19–22	0.139	0.346	0	1
Age 23–26	0.082	0.275	0	1
Age 27–30	0.084	0.277	0	1
Age 31–34	0.064	0.244	0	1
Age 35–38	0.050	0.218	0	1
Age 39–46	0.080	0.271	0	1
Age 47–54	0.020	0.141	0	1
Age 55++	0.000	0.018	0	1
Family income under $15K	0.138	0.345	0	1
Family income $15K–25K	0.150	0.357	0	1
Family income $25K–50K	0.190	0.392	0	1
Family income $50K–75K	0.197	0.398	0	1
Family income over $75K	0.326	0.469	0	1

Sample based on 3016 ATUS respondents enrolled in school who's Interview did not fall during Summer or Winter holidays or on weekends.

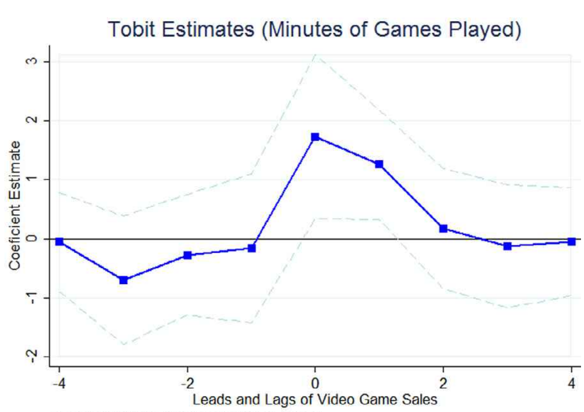

Fig. 5. Coefficients of leads and lags of video game sales on time spent playing games.

nificantly different from zero. These results suggest that lags of 0 and 1 weeks of game sales will not suffer from a "weak instrument" problem. In the tests of hypotheses that follow, the sum of sales for these two time periods are used the game sales instrumental variable.

It may be possible to address instrument validity by attempting to relate video game sales to non-game playing activities. Video game sales could cause changes time spent in other leisure activities as well as more time playing video games. If so, any effect on educational inputs could stem from these other leisure activities and not video game playing. To test for this, I repeat the same exercise of relating leads and lags of video game sales to time spent in each of computer use, playing sports, reading, and TV watching. The sample and specification are identical to Table 2 with only the dependent variable changed. The coefficient estimates and confidence intervals are displayed in Fig. 6. With the exception of a possible reduction in playing sports in the concurrent week and the four week lead and lag for reading, no coefficient values are statically different from zero at typical confidence levels. These results bolster the claim that the exclusion restriction is valid.

Before presenting instrumental variable results, reduced form Tobit results are presented in Table 3. Game playing time, class attendance time, and homework time are all regressed against the sum of video game sales over the current and previous week. The control variables have the expected signs. More time is spent playing video games by high school versus college students, males versus females, whites versus minorities, those who work fewer hours, the unmarried, and those in larger households. More time is spent attending classes by high school versus college students, possibly by males versus females, those who work fewer hours, the unmarried, and those from households with married householders. More time is spent doing homework by whites and Asians, those who work less, and those from households with married householders. Though not reported, each of the sets of dummy variables for age, income, day-of-week, and week-of year are statistically significant in all equations. The variable of interest has the expected sign in all equations but is not significant for homework. The coefficient for game time is nearly offset by sum of the magnitudes of the class time and homework time coefficients. The video game sales measure has a mean of 3.5 million units with a standard deviation of 1.4 million units. These estimates imply that a one standard deviation change in video game sales leads to a 18–19 minute diversion from class attendance and maybe a 3 minute diversion from doing homework to video game playing.

Baseline structural results indicate that video game time crowds out class time nearly one-for-one also but barely affects home-

Table 2
Lags and leads of video game sales on time spent playing games.

	Probit any game playing		Tobit hours playing games	
Video game sales				
4 week lead	−0.031	(0.098)	−0.074	(0.425)
3 week lead	−0.161	(0.130)	−0.705	(0.555)
2 week lead	−0.044	(0.130)	−0.270	(0.522)
1 week lead	−0.032	(0.155)	−0.179	(0.644)
Current week	0.444***	(0.171)	1.756***	(0.708)
1 week lag	0.242**	(0.120)	1.294***	(0.483)
2 week lag	0.072	(0.130)	0.151	(0.530)
3 week lag	−0.034	(0.126)	−0.073	(0.514)
4 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 ***$p < .01$, **$p < .05$, *$p < .10$

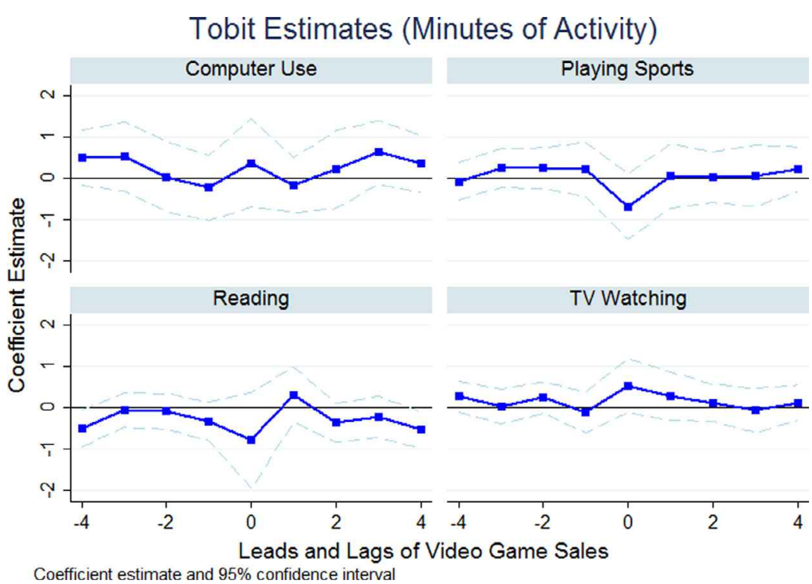

Fig. 6. Coefficients of leads and lags of video game sales on time spent in other leisure activities.

Table 3
Reduced form Tobit results.

	Video game time		Class time		Homework time	
Video game sales over two weeks	2.343***	(0.197)	−2.145***	(0.648)	−0.326	(0.529)
High school student	0.516***	(0.076)	3.076***	(0.291)	−1.280***	(0.235)
Male	2.319***	(0.060)	0.240*	(0.134)	−0.155	(0.123)
Black	−1.020***	(0.060)	0.061	(0.215)	−0.503***	(0.195)
Asian	−0.887***	(0.052)	0.510	(0.321)	0.941***	(0.310)
Hispanic	−0.985***	(0.055)	−0.183	(0.192)	−0.218	(0.175)
Usual work hours	−0.038***	(0.002)	−0.052***	(0.006)	−0.034***	(0.005)
Married	−1.041***	(0.095)	−0.603*	(0.313)	−0.373	(0.268)
Unmarried household	0.016	(0.064)	−0.474**	(0.186)	−0.406**	(0.181)
Household size 2–3	1.121***	(0.066)	−0.337	(0.347)	−0.351	(0.315)
Household size 4++	1.241***	(0.068)	−0.212	(0.342)	-0.442	(0.310)

Standard errors clustered on the observation week. The sample includes 3016 observations.
***$p < .01$, **$p < .05$, *$p < .10$

Table 4
Baseline structural Tobit results for the effect of game time on educational inputs.

	Class time		Homework time	
	Not instrumented	Instrumented	Not instrumented	Instrumented
Gaming time	−0.646***	−0.978***	−0.328***	−0.145
	(0.091)	(0.288)	(0.085)	(0.270)
High School Student	3.187***	3.548***	−1.238***	−1.211***
	(0.288)	(0.322)	(0.235)	(0.285)
Male	0.467***	2.508***	−0.075	0.179
	(0.135)	(0.686)	(0.123)	(0.654)
Black	0.005	−0.971***	−0.506***	−0.652*
	(0.211)	(0.362)	(0.194)	(0.334)
Asian	0.437	−0.348	0.925***	0.815**
	(0.321)	(0.407)	(0.309)	(0.382)
Hispanic	−0.245	−1.133***	−0.226	−0.360
	(0.190)	(0.337)	(0.175)	(0.322)
Usual work hours	-0.053***	-0.088***	−0.035***	−0.039***
	(0.006)	(0.012)	(0.005)	(0.012)
Married	−0.646**	−1.614***	−0.388	−0.521
	(0.309)	(0.433)	(0.267)	(0.377)
Unmarried household	−0.487***	−0.429**	−0.389**	−0.403**
	(0.181)	(0.186)	(0.181)	(0.187)
Household size 2–3	−0.284	0.745	−0.330	−0.188
	(0.344)	(0.500)	(0.316)	(0.432)
Household size 4++	−0.146	0.995*	−0.432	−0.262
	(0.340)	(0.524)	(0.310)	(0.446)

Specifications include sets of dummy variables for age, income, day of week, and week of year. Standard errors clustered on the observation week. Total video game sales over the current and lagged week are used as an instrumental variable. To account for possible generated regressor bias, standard errors are bootstrapped from 1000 repetitions in column 2. The sample includes 3016 observations.
$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$

work time. Table 4 report both the un-instrumented and instrumented specifications of Eq. (1) for class time and homework time. Table 4 differs from Table 3 columns 2 and 3 only in that the video game sales variable is replaced by time spent playing games. Hence, the control variable estimates are largely unchanged.

The estimate of the effect of the hours playing games on class time differs significantly depending on whether it is assumed to be exogenous and uninstrumented versus assumed to be endogenous and instrumented. The endogeneity bias appears to be toward zero which would typically occur if hours playing games measures video gaming hours with significant error. The estimate from the second column indicates that video game playing time crowds out class attendance time almost one-for-one. This seems larger than anticipated since the increased gaming time could crowd out other, non-class time such as other entertainment activities. A possible explanation could be that time spent playing video games is under-reported, and class attendance is over-reported due to students' desire to conceal unflattering behaviors. If so, this would tend to generate inflated estimated magnitudes and reduce the statistical power of the estimator.

The estimated effect of gaming on homework time in Table 4 is not as strong. The estimate is negative for both specifications but is smaller and not statistically significantly different from zero when game time is instrumented. The difference in magnitudes is consistent with students less likely to do homework selecting into gaming. The estimate from the instrumented specification more likely reflects only a causal effect. The negative coefficient, though not significant, is consistent with the crowding out hypothesis.

The baseline structural estimates are further investigated to see if the crowding out effect is moderated by certain characteristics of the individual. First, since average time spent playing video games is more than three times as large for males (26 minutes versus 7 minutes), the marginal effects also could be larger. Second, since college students tend to be more independent from parental supervision, they may be more prone to skip class or forgo homework. Also, as mentioned above, college students may be budgeting their gaming times on days without scheduled classes or assignments. Third, students from lower income families may not perceive the return to education as highly or, their parents may not be able to provide as much supervision. Finally, students from families in which the responsible adult is unmarried may not have the same levels of parental support or supervision.

Differences in the marginal effect of gaming time are tested one at a time be altering the specification slightly. For each characteristic, I create a dummy variable, $Char_i$, interact it with time playing games, and estimate the following:

$$Educ\ Time_i = \alpha_1 \widehat{VG\ Time}_i + \alpha_2 \widehat{VG\ Time_i \times} Char_i + \beta X_i + \epsilon_i \quad (1')$$

$$VG\ Time_i = \gamma_1 VG\,Sales_i + \delta_1 X_i + \nu_{1i} \quad (2')$$

$$VG\ Time_i \times Char_i = \gamma_2 VG\,Sales_i \times Char_i + \delta_2 X_i \times Char_i + \nu_{2i} \quad (3')$$

The test of hypothesis is whether the estimate of the difference in marginal effects is different from zero, or if $\hat{\alpha}_2 \neq 0$. Now there is a second endogenous variable which is estimated only on the sub-sample with $Char_i = 1$. Again, all specifications are estimated as Tobits with the standard errors in the second stage bootstrapped to address the generated regressor issue.

Table 5 reports the second-stage results of the tests of the crowding out effect on class time being moderated by student characteristics. Column 1 indicates that, although males play more video games and attend classes more, the marginal effect of video game playing on class attendance is not significantly different. The tests of student independence from parental or adult supervision yield mixed results. Columns 2 and 3 indicate that the crowding out effect is relatively larger for college students and for students from lower income families.[8] However, column 4 does not indicate

[8] It is possible that the differential effect for low income households represents a subtle form of selection. Because they have less income, they buy fewer games making the threshold utility in order to purchase from a game higher. This implies

Table 5
Moderators for the effect of game time on class time.

	Hours attending class			
Hours playing video games	−0.905*** (0.319)	−0.787** (0.315)	−1.041*** (0.316)	−0.914*** (0.321)
Hours playing video games × Male	−0.038 (0.066)			
Hours playing video games × College student		−0.228** (0.089)		
Hours playing video games × Family income over $25K			0.145* (0.083)	
Hours playing video games × Unmarried household				0.031 (0.032)
High school student	3.555*** (0.326)	4.577*** (0.512)	3.561*** (0.332)	3.537*** (0.347)
Male	2.214*** (0.835)	2.225*** (0.751)	2.427*** (0.742)	2.342*** (0.765)
Unmarried household	−0.469** (0.190)	−0.458** (0.186)	−0.451** (0.184)	−0.334 (0.229)

Specifications include sets of dummy variables for age, income, race, household size, married, day of week, and week of year. Standard errors clustered on the observation week. Total video game sales over the current and lagged week and total sales times the moderator are used as instrumental variables. To account for possible generated regressor bias, standard errors are bootstrapped from 1000 repetitions. The sample includes 3016 observations.
***$p < .01$, **$p < .05$, *$p < .10$

Table 6
Moderators for the effect of game time on homework time.

	Hours doing homework			
Hours playing video games	−0.100 (0.262)	−0.184 (0.269)	−0.137 (0.261)	−0.145 (0.258)
Hours playing video games × Male	−0.151** (0.062)			
Hours playing video games × College student		0.055 (0.067)		
Hours playing video games × Family income over $25K			−0.009 (0.072)	
Hours playing video games × Unmarried household				−0.008 (0.032)
High school student	−1.190*** (0.279)	−1.454*** (0.406)	−1.211*** (0.267)	−1.207*** (0.283)
Male	−0.453 (0.692)	0.223 (0.635)	0.176 (0.612)	0.184 (0.608)
Unmarried Household	−0.442** (0.175)	−0.403** (0.179)	−0.404** (0.178)	−0.439* (0.230)

Specifications include sets of dummy variables for age, income, race, household size, married, day of week, and week of year. Standard errors clustered on the observation week. Total video game sales over the current and lagged week and total sales times the moderator are used as instrumental variables. To account for possible generated regressor bias, standard errors are bootstrapped from 1000 repetitions. The sample includes 3016 observations.
***$p < .01$, **$p < .05$, *$p < .10$

any difference in the magnitude whether the householder is married or not. Table 6 repeats this exercise for homework time as the dependent variable. In this case, the only significant moderator is that gaming appears to induce males to spend less time on homework than females.

5. Conclusion

These results can be summarized as popular video games increases students' time spent playing video games which induces some gamers to stay away from school and maybe to be less diligent with their homework. While students from households without a married householder do not noticeably differ, males, college students, and students from lower income families have larger crowding out effects from video game playing. It is worth noting that these estimates are available only because of particular characteristics of the video gaming industry. The effect of game playing is identifiable from selection into gaming because of both the substantial week-to-week variation in the desirability of currently available games and the quick depreciation of the value of playing a particular game. It is not likely that other leisure activities yield the same degree of exogenous time variation.

The continuous development of new information technologies will tend to lead to an increasing assortment of new leisure activities. To the extent that these technologies engender engaging and entertaining activities, they will likely displace time spent in alternative activities. Some of these displaced activities may include other entertainment activities, e.g. television viewing, which revealed preference theory would suggest is utility improving. However, some effects could be related to educational inputs and attainment and, ultimately on the labor market outcomes. The findings of this analysis suggest that short-run intertemporal variation in video game demand can affect class attendance. While it may be a short logical hop to therefore conclude that the general increase in the popularity of video gaming has led to a general decrease in

that games selected by low income households will have higher average quality which could induce longer gaming sessions.

educational inputs, the analysis provides no direct evidence of this longer-term trend. However, if this is true, parents may prefer to devise new methods to ameliorate these effects. To some extent, this is similar to the evolution of parental control over children's television viewing times.

The public policy concerns over this are less clear. Psychological and educational benefits from playing video games have been documented. Granic et al. (2014) surveys the literature on the positive psychological effects of playing video games. They focus on four main areas: cognitive, motivational, emotional, and social areas and even identify a possible neuroscientific mechanism for enhanced cognitive growth. Suziedelyte (2015) identifies a causal effect of children's video game playing on problem solving tasks.

Moreover, public policy interventions are usually justified based on a market imperfection such as a foregone positive externality from education. While there appear to be substantial private returns to education (Belzil, 2007), Rodríguez-Pose and Tselios (2012) and Robertson et al. (2010) also find evidence of substantial positive externalities while Acemoglu and Angrist (2000) suggest that they are smaller. Still, while a policy restricting video game time might increase positive externalities and, thus, increase future productivity; it comes at a cost of lower current utility. Existing estimates do not allow for a clear policy prescription.

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