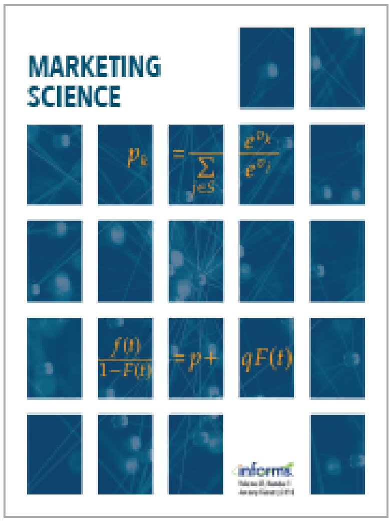


Marketing Science

Publication details, including instructions for authors and subscription information:
http://pubsonline.informs.org

Protecting Consumers from Themselves: Assessing Consequences of Usage Restriction Laws on Online Game Usage and Spending

Wooyong Jo, Sarang Sunder, Jeonghye Choi, Minakshi Trivedi

informs
http://pubsonline.informs.org/journal/mksc

MARKETING SCIENCE
Vol. 39, No. 1, January–February 2020, pp. 117–133
ISSN 0732-2399 (print), ISSN 1526-548X (online)

Protecting Consumers from Themselves: Assessing Consequences of Usage Restriction Laws on Online Game Usage and Spending

Wooyong Jo,[a] Sarang Sunder,[b] Jeonghye Choi,[c] Minakshi Trivedi[b]

[a] Goizueta Business School, Emory University, Atlanta, Georgia 30322; [b] Neeley School of Business, Texas Christian University, Fort Worth, Texas 76129; [c] School of Business, Yonsei University, Seoul 03722, South Korea

Contact: wooyong.jo@emory.edu, https://orcid.org/0000-0001-9789-3113 (WJ); s.sunder@tcu.edu, https://orcid.org/0000-0002-2875-9209 (SS); jeonghye@yonsei.ac.kr, https://orcid.org/0000-0002-5862-3599 (JC); m.trivedi@tcu.edu, https://orcid.org/0000-0003-3821-2781 (MT)

Received: June 9, 2017
Revised: April 16, 2018; March 26, 2019
Accepted: April 11, 2019
Published Online in Articles in Advance: January 24, 2020

https://doi.org/10.1287/mksc.2019.1174

Abstract. Given the rise of the online gaming industry, consumer protection laws have been implemented to restrict excessive gaming on the internet. This research evaluates one such consumer protection policy and its effectiveness from both marketing and public policy perspectives. Specifically, we investigate the impact of usage restriction in South Korea on both gamers and the industry using individual-level game usage and spending data. Combining the difference-in-differences approach with a regression discontinuity design and propensity score matching, we show that although the regulation reduces overall game usage, the effects vary depending on past behaviors; that is, counter to expectations, the regulation effect, although negative for average gamers, is less so for heavy gamers and in fact directionally flips for the heaviest gamers. Furthermore, we find that its revenue impact is negligible, suggesting that gamers do not change spending patterns because of the intervention. Thus, usage restriction laws may deter light gamers from potentially excessive gaming but do not work well to dissuade heavy gamers, suggesting that a more nuanced approach (as opposed to a blanket usage restriction law) may be called for. Finally, we discuss the implications of such usage restriction laws as a vehicle to control overconsumption and protect consumers.

History: Avi Goldfarb served as the senior editor and Hema Yoganarasimhan served as associate editor for this article. This paper has been accepted for the *Marketing Science* Special Issue on Consumer Protection.

Funding: This work was supported by the Ministry of Education of the Republic of Korea and the National Research Foundation of Korea [Grant NRF-2017S1A3A2066788].

Supplemental Material: Data files and the online appendix are available at https://doi.org/10.1287/mksc.2019.1174.

Keywords: consumer protection • online gaming • public policy • usage restriction • game spending

Introduction

The creation and cultivation of consumers' repeat behaviors are usually associated with economic benefits to firms. However, certain products may have negative connotations, especially when extensively repeatedly purchased. In such instances, overconsumption may present unique societal and health-related challenges in the marketplace. As a result, policymakers have used regulatory instruments (such as taxes, usage restrictions, bans, etc.) to promote public interests and prevent socially undesirable behaviors (e.g., drug addictions and excessive drinking and smoking). The marketplace is strewn with examples and cases of regulatory actions aimed toward protecting consumers from overconsumption behaviors. These actions range from sugar taxes to curb obesity (Khan et al. 2016) and excise taxes to prevent smoking (Wang et al. 2015) to imposing bans on plastic bags to encourage eco-friendly consumption behaviors (Sharp et al. 2010). Although extant literature is generally in agreement that the use of such regulatory instruments is important for consumer protection, the effectiveness of these policy interventions is unclear (Dharmasena and Capps 2012, Ringold 2002).[1] One such addictive behavior that is gaining a great deal of attention is online game addiction, which is the focus of this research.

Given the increasing number of cases of problematic gaming being highlighted in academia (Caplan et al. 2009, Young 2009) as well as the popular press (Lee 2011, Robinson 2015, Alter 2017), there have been growing concerns about the negative consequences of this behavior on the small segment of heavy gamers. The typical policy response in the face of overconsumption or overusage has been to restrict usage.[2]

By and large, regulatory bans on excessive gaming in most countries have adopted techniques that restrict gaming access by gamer's age and time of day. Examples include Vietnam restricting access to online games by minors from 10:00 p.m. to 8:00 a.m. and Thailand banning children under 18 years of age from entry to internet cafés from 10:00 p.m. to 10:00 a.m.[3] The key questions for policymakers (and game companies) here are, *how effective are these policy interventions in reducing gaming behavior? Furthermore, do these policies influence all gamers equally?* The answers are not straightforward, especially given that past research on regulatory bans is inconclusive regarding their effectiveness in producing the intended consequences (Miyazaki et al. 2009, Sharp et al. 2010). To the best of our knowledge, there has been no systematic study assessing the impact of policy interventions such as usage restriction laws on online usage and spending behaviors.

To empirically examine these issues, we focus on the usage restriction law implemented in 2011 in South Korea that limits access to online games for teenagers under the age of 16 (regulated gamers) between the hours of midnight and 6:00 a.m. Using log and sales data of the online game, we find that this intervention indeed lowers game usage of the regulated gamers on average. However, more detailed analyses reveal that gamers are not equally affected. Heavier gamers, who typically are at higher risk of behavioral addiction, are less impacted, with smaller decreases, and perhaps even increases, in play time compared with the more casual, lighter gamers. Furthermore, we also find that game spending behavior does not change significantly as a result of the regulation.

This paper makes the following contributions. First, we believe this to be the first effort to assess the effects of usage restriction laws on online gaming behavior using microdata. These findings should be useful to the growing stream of academic and policy work aimed at evaluating the downsides of excessive gaming behaviors (e.g., Charlton and Danforth 2007, Van Rooij et al. 2011) and the role of regulations for consumer protection. Second, we document the importance of considering heterogeneous effects when assessing the effectiveness of a policy. Although, on average, the usage restriction laws do have the intended (negative) effect on usage, the true impact of the regulation is more nuanced. We show that whereas the law is effective in reducing usage for lighter gamers, perhaps thus preventing future overconsumption behaviors, it is less effective in reducing established habits (in fact, extremely heavy gamers increase usage). This counterintuitive finding calls for a more tailored (as opposed to a one-size-fits-all) approach to policy design when considering usage restrictions in the online context. Third, we demonstrate the revenue impact of the regulation, an issue of grave importance to the online gaming industry. Counter to expectations, we find that the usage restriction laws do not affect the spending behavior of the gaming patrons.

The rest of this paper is organized as follows. We first briefly discuss the literature in the area of overconsumption and the role of regulations for consumer protection. We then introduce the market context and data and present model-free evidence. After describing the dependent variables, we develop difference-in-differences (DID) models supplemented by a regression discontinuity (RD) design and propensity score matching (PSM) to assess the policy's impact on game usage and spending. Next, we discuss the empirical findings followed by a battery of robustness checks. Finally, we conclude with a discussion of our contributions and the implications of our findings as well as limitations and avenues for future research.

Background

Related Literature

This paper is related to two growing streams of literature that explore the prevalence of overconsumption in gaming behavior and the role of policy interventions in controlling this behavior and thus protecting consumers. Since the first electronic game was introduced in 1958 at the Brookhaven National Laboratory, advances in technologies have resulted in more sophisticated games that offer a novel form of entertainment. In fact, even as far back as a decade ago, 72% of U.S. households played video or internet games, with 8.5% of adolescents and 12.5% of the population diagnosed as being clinically addicted to them (Gentile 2009). Prior research suggests that excessive gaming is associated with various problems such as clinical depression, poor academic performance, and social anxiety (Lo et al. 2005, Heo et al. 2014).

In recognizing the negative consequences of excessive gaming on society and the gamers' limited ability in dealing with them, many public policy interventions have been designed to restrict excessive gaming. Whether these interventions are successful or not, however, remains in doubt for several reasons. First, gaming is more of a personal choice in virtual space rather than physical space, as in the case of addictive substances such as cigarettes or drugs. Clearly, the former is less easily observed and monitored, making effective intervention a greater challenge. Second, although several regulatory actions do indeed function as designed, some studies indicate the possibility of associated side effects or unintended consequences (Ringold 2002, Wang et al. 2015). Research investigating the impact of policy interventions in the context of privacy (Goldfarb and Tucker 2011), piracy (Miyazaki et al. 2009), nicotine intake (Sharp et al. 2010), and fast-food consumption (Dhar and Baylis 2011) have demonstrated that such restrictions do not always achieve intended results.

Clearly, unintended consequences of regulations need to be anticipated as one of the potential costs of a nationwide market intervention and properly controlled from a public policy perspective. This, then, is broadly where our interest lies—that is, to study market reactions to governmental efforts in restricting excessive gaming behavior and to determine whether such policies indeed achieve their objectives.

Market Context: Korea's Shutdown Law

The institutional setting that we focus on is online gaming in South Korea. With one of the world's fastest broadband infrastructures and highest internet penetrations, Korea reports approximately 10% of the population between the ages of 10 and 18 as suffering from excessive gaming. Furthermore, according to the recent report by the Korea Information Society Development Institute (2014), the average Korean teenager spends close to 92 minutes a day on internet use and game play—roughly twice as much as the 52 minutes a day for the corresponding American teenager (Bureau of Labor Statistics 2013).

On the policy side, several regulations have been designed to restrict excessive gaming. We investigate the effectiveness of one such regulation in Korea. In 2011, the Korean government implemented the "shutdown" policy, a regulation that limits access to games for teenagers under the age of 16 between the hours of midnight and 6:00 a.m. The regulation is applicable to online (or internet-based) games as well as to online networks of popular video games such as PlayStation Network and Xbox Live. The mechanism through which the government enforces this law uses a unique social security number for each gamer as a sign-in for online games. The shutdown law was strongly opposed by the online gaming industry given that it was directly aimed at reducing game usage among the same teenagers who made up a major portion of its core consumer segment. The implicit assumption was that a reduction in game usage would lead to a decline in revenues. However, the magnitude and direction of the policy intervention effect on game usage as well as firm revenues have remained unclear. To the best of our knowledge, there have been no systematic analyses of the regulation effect on the gaming industry in Korea.

Data

Data Description

To understand the effects of usage regulation, we obtain gamer-level data from one of the largest game publishers in Korea.[4] The game, targeted to sports fans across all ages, was launched in April 2010 and has been the most popular game in the sports management category. Gamers play the role of team manager by organizing team rosters, dealing with finances, and setting up team strategies. Leagues are run every week, and gamers play against other gamers or computer-controlled teams in the same league, handling strategy, tactics, team finances, and so on. Note that the mechanics of the game work similarly to those of an online sports fantasy game (much like fantasy leagues in the United States). The focal game does not require monthly subscription fees, and the firm's revenue is only generated by selling game items; no alternative source of revenue exists.[5]

We obtain gamer-level data from July 2011 (17 weeks prior to the shutdown policy implemented on November 20, 2011) to February 2012 (14 weeks after policy implementation). The data include the individual-level history of game play since sign-ups, cash expenditures, and corresponding demographics. Because we wish to examine the impact of the shutdown policy imposed in November 2011, we focus on gamers who signed up at least eight weeks prior to the regulation, resulting in 7,218 gamers.[6] Based on our extensive conversations and qualitative interviews with the firm, we learned that a one-month time period is sufficient to measure the effects of regulation, especially in the online game category requiring no monthly subscription fees. Less than that captures too much noise, and more than that misses significant changes in behavior. We thus define a four-week data window to measure the immediate market response from the regulation (i.e., the first four-week data window for the preregulation game usage and the second four-week data window for the postregulation game usage; for similar treatment, see Zhang and Zhu 2011 and Seiler et al. 2017).

Table 1 provides summary statistics for the variables as well as their definitions. We see that gamers play the focal game, on average, 52 times during the study period and spend an average of 24 minutes per visit while making in-game purchases three times and paying $1.7 per purchase. To explore any differences across gamers, we split the users into heavy (top 20%) and light (bottom 80%) gamers based on their past game usage.[7] Unsurprisingly, heavy gamers outplay and outspend light gamers across most of the metrics except user tenure and age; that is, heavy gamers display higher visit frequency, per-visit time, purchase frequency, and per-purchase spending. Figure 1 also clearly shows that the heavy gamers' play time is much greater than that of light gamers. Taken together, Table 1 and Figure 1 indicate significant variation between heavy and light gamers when it comes to play time and spending patterns.[8] Thus, we explore how the usage restriction policy affects gaming behavior.

Table 1. Summary Statistics

Variable	Description	All gamers		Light gamers (bottom 80%)		Heavy gamers (top 20%)	
		Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Total play time	Total play time (in minutes)	1,728.610	2,765.997	899.750	1,413.996	5,042.900	4,065.003
Visit frequency	Number of game plays (visits)	52.077	53.730	38.047	37.156	108.181	70.554
Per-visit time	Average play time per visit (in minutes)	23.911	18.623	19.001	14.257	43.543	20.957
Total spending	Total spending (in dollars)	28.030	125.393	15.034	88.945	79.995	208.830
Purchase frequency	Number of purchases	2.876	11.439	1.442	6.164	8.609	21.478
Per-purchase spending	Spending amount per purchase (in dollars)	1.733	4.473	1.240	4.033	3.705	5.486
User tenure	Time since sign-up (in weeks)	21.261	6.179	21.278	6.096	21.194	6.502
Male	Gender of user (the proportion of male gamers)	0.702	0.458	0.678	0.467	0.798	0.402
Age	Age of user (in years)	23.419	7.868	23.250	7.903	24.095	7.690

Note. Std. dev., standard deviation.

Model-Free Evidence

To explore the effect of the policy intervention on gaming behavior, we present model-free evidence highlighting some patterns in the game usage data.

Overall Patterns in Play Time. Recall that the shutdown law affects gamers below the threshold of 16 years. As such, we define gamers under the age of 16 as the *treatment group* and gamers above or equal to the age of 16 as the *control group*. We examine any differences in play time between the two groups by plotting play time against age in Figure 2. Both groups of gamers increase their play time from the pre- to postregulation period, with the treatment group showing a marginal increment.[9] Thus, the regulation seems to achieve its designed objective because it limits the increase in game usage for the treatment group, supporting the regulation effect.

Next, we consider whether the regulation effect may be moderated by gamers' past usage behavior. As before, heavy gamers are defined as the top 20% using their past play time. Table 2 shows that the control group increases game usage in the postregulation period. Perhaps more interestingly, although the regulation has the expected negative, albeit marginal, impact on light gamers, the directionality of the effect changes for heavy gamers; that is, light gamers in the treatment group reduce their usage, whereas gamers in the control group increase their usage, thus indicating that the policy has the intended negative effect on light gamers in the treatment group. However, the difference in the usage change between heavy gamers in the treatment group and those in the control group is not significant, suggesting that the same effect of regulation is not indicated for heavy gamers.

Figure 1. (Color online) Play Time Distributions for Heavy and Light Gamers

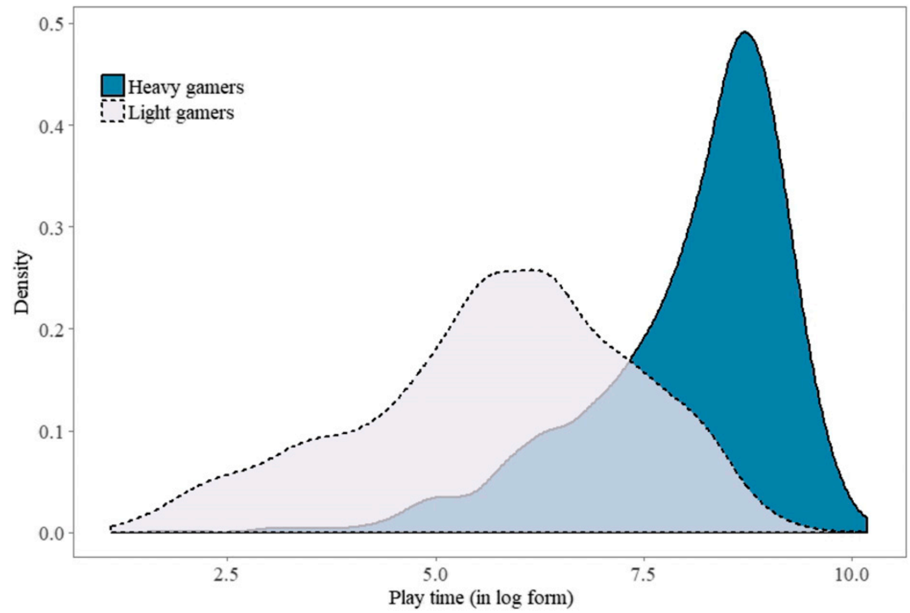
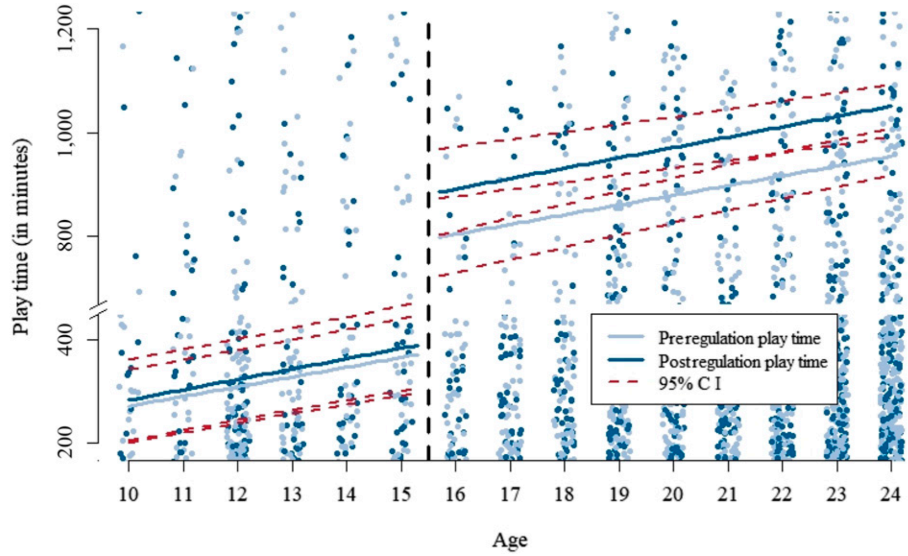


Figure 2. (Color online) Model-Free Evidence: Play Time by Age



Notes. Dots in light and dark blue indicate average play times in minutes for the pre- and postregulation periods, respectively. Dashed lines in red represent 95% confidence intervals. The *x*-axis shows gamers' ages, dichotomizing those into the treatment and control groups using the threshold of 16 years.

Within-Day Gaming Patterns. The above model-free evidence shows that whereas play time reduces on average among light gamers in the treatment group, the same effect is not observed for heavy gamers. This raises the following questions: *Are gamers under the age of 16 reacting to the shutdown regulation by reallocating their gaming hours within the day? If so, how are heavy and light gamers reallocating their play time?* Because we observe the time stamps of game play (through logs), we split the 24-hour day into 3-hour time slots and then compute the average play time in each slot for the pre- and postregulation periods. Figure 3 shows that, as expected, whereas play time during the curfew period (12:00 a.m. to 6:00 a.m.) is positive in the preregulation period, the corresponding play time drops to zero in the postregulation period. Moreover, the treatment group indeed reallocates play time to other parts of the day, because play time significantly increases between 6:00 a.m. and 6:00 p.m. (a paired *t*-test shows significance for each time slot). Interestingly, play time remains the same during the evening hours (6:00 p.m. to 12:00 a.m.), when the treatment group is more likely to be under the purview of their parents.

Table 3 presents further evidence for play time reallocation by comparing heavy and light gamers in the treatment group. Again as expected, the treatment group does not play the game during the curfew period. During the noncurfew hours, however, heavy gamers significantly increase their play time across all time slots (except the slot right before curfew, when a sudden disconnection due to the regulation may disrupt game play), whereas light gamers do not (except for the slot including lunch hours). This finding suggests that the regulation affects heavy and light gamers differently.

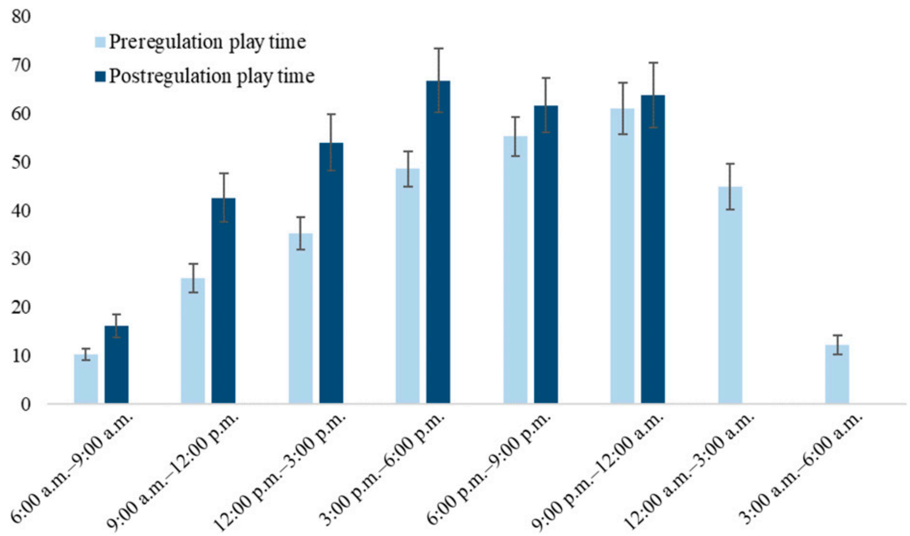
Summary. The visual representations in Figures 2 and 3 combined with the statistics in Table 2 and 3 indicate that the average regulation effect is significant and negative, as expected, but the negative effect

Table 2. Model-Free Evidence: Pre- and Postregulation Play Time

	Treatment group (gamers affected by the regulation)			Control group (gamers unaffected by the regulation)			
Sample	Preregulation period	Postregulation period	Difference (A)	Preregulation period	Postregulation period	Difference (B)	Difference-in-differences, (A) − (B)
Light gamers (bottom 80%)	105.744 (262.202)	97.000 (432.606)	−8.744	950.443 (1,450.803)	1,050.451 (1,591.358)	+100.008**	−108.752**
Heavy gamers (top 20%)	1,046.409 (1,424.721)	1,146.590 (1,872.196)	+100.181				+0.173

Notes. Time is measured in minutes. Standard deviations are in parentheses.
$^{**}p < 0.01$.

Figure 3. (Color online) Model-Free Evidence: Play Time by Time Slot for the Treatment Group



Notes. Play time is measured in minutes. Error bars represent standard errors.

might be due to light gamers and not heavy gamers (who are the main target of the regulation). This model-free evidence indicates a surprising and perhaps counterintuitive result regarding the impact of the shutdown law. Because this in itself does not imply a causal effect, we use multiple econometric specifications to show that these patterns hold, thus providing empirical evidence of the regulation effect on online gaming.

Methodology

Our goals are threefold. First, we attempt to assess and quantify the impact of the regulatory ban on game usage behavior. Second, we aim to highlight the moderating role of past usage behavior on the policy's impact. Specifically, we show that the regulatory ban has a heterogeneous effect on usage patterns. Third, we study the impact of the regulatory ban on gamers' spending behavior. Contrary to conventional wisdom, we show that though the regulatory ban seems to heterogeneously influence game usage, it does not influence gamers' spending behavior; that is, revenue impact is negligible. To capture the above-mentioned effects and make a causal inference on the policy's impact, we use the DID methodology and combine it with the RD design supplemented by the PSM technique.

Dependent Variables

The dependent variables of interest in this research fall into two categories, usage (measured as *total play time*) and spending (measured as *total spending*). We build DID models for each of the dependent variables and estimate the regulation effect accordingly.[10] Usage (*total play time* in minutes) and spending (*total spending* in dollars) are modeled in log-linear form to account for nonnegativity and skewness. Because of the potential selection issues that arise with spending, we model spending using a Tobit I model. For

Table 3. Model-Free Evidence: Play Time by Time Slot for the Treatment Group

	Light gamers (bottom 80%)			Heavy gamers (top 20%)		
Time slot	Preregulation	Postregulation	Difference	Preregulation	Postregulation	Difference
6:00 a.m.–9:00 a.m.	4.959	7.009	+2.049	31.922	52.815	+20.893**
9:00 a.m.–12:00 p.m.	7.915	11.093	+3.178	98.312	168.134	+69.822**
12:00 p.m.–3:00 p.m.	11.721	17.605	+5.884*	128.998	199.138	+70.140**
3:00 p.m.–6:00 p.m.	16.749	20.802	+4.054	175.601	249.915	+74.314**
6:00 p.m.–9:00 p.m.	20.187	18.769	−1.418	195.061	232.677	+37.615[a]
9:00 p.m.–12:00 a.m.	21.907	21.219	−0.688	216.935	233.594	+16.659
12:00 a.m.–3:00 a.m. (curfew)	17.270	0.000	−17.270**	154.937	0.000	−154.937**
3:00 a.m.–6:00 a.m. (curfew)	5.037	0.000	−5.037**	41.139	0.000	−41.139**

Note. Time is measured in minutes.
[a]$p = 0.062$.
*$p < 0.05$; **$p < 0.01$.

ease of exposition, we refer to each dependent variable in the following empirical model section as an *outcome*.

Empirical Model

Recall that the treatment group includes users who are under the purview of the policy (gamer age < 16 years), and the control group contains those who are unaffected (gamer age ≥ 16 years). In order to estimate the causal effect of the shutdown regulation, we need to show a significant DID in gaming behavior between the treatment and control groups *after* controlling for various confounding and selection issues that may arise from cross-sectional and temporal factors.

DID Model. We begin with the baseline DID model specification

$$\begin{aligned} Outcome_{it} = {} & \beta_1 TR_{it} + \beta_2 LagOutcome_{it} \\ & + \beta_3 TR_{it} \times LagOutcome_{it} \\ & + \boldsymbol{\theta Z_{it}} + \mu_i + \tau_t + \varepsilon_{it}. \end{aligned} \tag{1}$$

The outcome variables include usage (total play time) and spending (total spending amount), as described earlier. The term TR_{it} is an indicator variable that takes the value 1 if gamer i is subject to the regulation at time t (treatment group) and 0 otherwise (control group). The term β_1 captures the average effect of the regulation on the treatment group. $LagOutcome_{it}$ is either lagged total usage (in 1,000 minutes) or spending (in dollars), and β_2 measures the effect of prior behavior.[11] We include the interaction term $TR_{it} \times LagOutcome_{it}$ to investigate the effectiveness of the regulation at different levels of prior game usage or spending. The term β_3 captures the moderating effect of the regulation on altering game usage or spending of regulated gamers. Term $\boldsymbol{Z_{it}}$ denotes a vector of observable characteristics at the user level, and $\boldsymbol{\theta}$ is the corresponding vector of parameter estimates. The terms μ_i and τ_t denote the user and time (week-level) fixed effects. Total play time is modeled as a log-linear model and total spending as a Tobit I model. We also estimate nested versions of the model and show that the average regulation and interaction effects are consistently significant.

Regression Discontinuity Design. The DID model controls for some of the selection problems through rich fixed-effects specifications; however, some selection issue may still remain because the group assignment using the cutoff of age is nonrandom. To mitigate this issue, we supplement the DID analyses with RD designs. Causal inferences in RD designs are more credible in quasi-experimental settings than in conventional DID approaches because they require milder identifying assumptions (Imbens and Lemieux 2008, Lee 2008).[12]

In our setting, the minimal assumption to identify the regulation effect is that the gaming behavior for those just above the threshold of 16 years should represent a valid counterfactual for the treatment group just below the threshold.[13] To this end, we find a three-year bandwidth both below and above 16 years of age to be appropriate in that it affords a large enough sample size to make statistical inferences, resulting in the RD sample including 299 and 387 gamers in the treatment and control groups, respectively. We then run the DID models using the RD sample.[14] Note that Lee (2008, p. 676) shows that the treatment effect in the RD design is "as good as random" as long as individuals are unable to control an assignment variable near a cutoff. We thus postulate that the RD design can be analyzed and interpreted similarly to a randomized experiment.

Propensity Score Matching. To further mitigate the selection issue, we even the playing field by making sure that both groups' gaming patterns are comparable prior to the policy intervention; that is, we supplement the RD design with PSM and estimate the regulation effect for matched pairs of treatment and control groups (Imbens 2000, Angrist and Pischke 2009).[15] PSM allows us to select the sample such that treatment and control groups (just above and below the discontinuity) are similar to each other on observables. To be specific, we obtain propensity scores through a logit model using the optimal pair-matching technique outlined in Rosenbaum (1989). User's probabilities of being exposed to the regulation (i.e., treatment) are estimated using observable characteristics in the preregulation period (i.e., visit frequency, per-visit time, gender, user tenure, etc.). Thus, pairs of treatment and control groups are formed such that the overall within-pair distance across all pairs is minimized, thus guaranteeing that for every user in the treatment group, there is at least one match in the control group. We then check the validity of the PSM procedure by comparing the covariates across treatment and control groups in the pre- and postregulation periods. Table 4 provides summary statistics of the RD sample before and after applying PSM. (See Section G of the online appendix for the summary statistics of matched pairs and the distribution of propensity scores.)

Regulation Effects

The results are organized as follows. First, we describe the estimation results of the DID models using the full sample for both outcomes of interest (total play time and total spending). Next, we repeat the analyses using the RD sample and replicate the findings to ensure that the results are robust. Finally, we discuss a battery of robustness checks and explore additional effects of the regulation.

Table 4. Summary Statistics of RD Sample Before and After PSM

Variable	Treatment group Mean	Control group: Before matching Mean	Before matching Difference	After matching Mean	After matching Difference
Visit frequency	20.355	23.651	3.297*	21.505	1.150
Per-visit time	18.933	19.242	0.310	19.317	0.384
Purchase frequency	0.455	0.486	0.031	0.304	–0.151
Per-purchase spending	0.276	0.479	0.204	0.220	–0.056
User tenure	19.267	19.176	–0.091	19.208	–0.059
Male	0.722	0.819	0.097**	0.773	0.051
Age[a]	13.736	17.171	3.435**	17.141	3.405**
Propensity score	0.455	0.421	–0.034**	0.443	–0.012

[a]*Age* is not used when obtaining propensity scores.
*$p < 0.05$; **$p < 0.01$.

Main Results

DID Results from the Full Sample. First, we discuss the estimation results for game usage using the full sample (without applying the RD design). As can be seen from Table 5, total play time decreases for the treatment group after the regulation; that is, gamers under the age of 16 reduce their play time more than the control group because of the regulation. Holding all control variables at their respective means, we compute the average regulation effect from the DID model. We find that gamers under the age of 16 reduce their play time on average by 8.3% compared with other gamers.[16] This suggests that the regulation seems to have the intended effect on the average gamer. However, when we interact past game usage behavior ($LagOutcome_{it}$) with the treatment variable, we see that the regulation effect diminishes as the gamer's past usage increases; that is, for heavier users with higher past game usage, the negative regulation effect is weakened, indicating that the shutdown law might not be as effective for reducing game usage among the regulated users. The magnitude of this moderating effect can also be easily computed given that $LagOutcome_{it}$ is a continuous variable. We find that for a 100-minute increase in past play time, the negative regulation effect further reduces by 6.2%, from 8.3% to 2.1%. Next, we turn to the effects of the regulation on spending behavior.

Recall that the full sample consists of users ranging from teen age to middle age, suggesting differences in purchasing power across the age groups. To control for the income effects (in the absence of income data) that may confound the DID estimates, we limit our control group to gamers who are in high school (between 16 and 19 years of age). Thus, the treatment group remains the same (n = 1,404), whereas the control group includes the 644 unregulated teenage gamers in high school. Interestingly, the main treatment effect and the interaction effect are insignificant in both models. This suggests that though the regulation, on average, reduces game usage, it does not have an effect on spending behavior; that is, the regulation

Table 5. DID Model Results from the Full Sample

Variable	Total play time: Estimate	Total play time: Std. error	Total spending: Estimate	Total spending: Std. error
TR_{it}	–0.214**	0.035	–0.707	0.541
$LagOutcome_{it}$	1.348**	0.040	–0.063**	0.019
$TR_{it} \times LagOutcome_{it}$	0.621**	0.110	–0.003	0.049
$LagSpending_{it}$	0.000	0.000		
$LagTime_{it}$			–0.008	0.458
$UserTenure_{it}$	–0.049**	0.006	0.117	0.995
Intercept	4.331**	0.147	–25.463	22.010
User fixed effects	Yes		Yes	
Time fixed effects	Yes		Yes	
Adjusted R^2 (log-likelihood)	0.715		(–1,009)	
Observations	57,744		16,384	

Notes. Standard (Std.) errors in the total play time model are clustered at the individual level and robust to heteroscedasticity. Total spending is modeled using a Tobit I model.
**$p < 0.01$.

does not appear to differentially influence in-game spending patterns of either group, and firm revenue remains unaffected by this regulation. These results provide some initial insight into the effects of the regulation ban. In the following sections, we apply more rigorous econometric techniques to causally study the regulation effect.

DID Results from the RD Sample. As described in the "Methodology" section, we apply the RD design to alleviate selection issues that may be affecting the baseline DID results. Table 6 describes the estimation results of the RD sample for game usage and spending. We begin with basic analyses with no controls in columns (1) and (2), add observed controls in column (3), further address selection issues in column (4), and finally control for unobserved heterogeneity in column (5).

Table 6 shows that the shutdown law has a negative and significant effect on the average gaming time across all the models. The effect size ranges from −0.467 to −0.686 in terms of average reduction in total play time. Across all the models, we see that treated gamers do reduce their usage patterns more than the control group, indicating that the regulation is successful on average. As before, the average regulation effect (holding all other variables at their respective averages) decreases total play time by 32.2% among the treated gamers compared with control group gamers. However, when studying the interaction effect of past usage behavior on the regulation effect, interesting patterns emerge. In all the models, the regulation effect is positively moderated by past game usage behavior, thus leading to the diminishing regulation effect for heavy users.[17] That is, the regulated heavy gamers do not reduce their game usage as much as their lighter counterparts when the policy is implemented. The magnitude of the moderation effect is also nontrivial. For every 100-minute increase in past game usage, the (negative) regulation effect further decreases by 12.3%, from 32.2% to 19.9%.

To understand the magnitude of the above moderation effects better, we compute the regulation effects at varying percentiles of past game usage and plot the regulation effects in Figure 4. Whereas light gamers significantly reduce their play time in response to the shutdown law, the directionality of the effect changes from negative to positive with heavy gamers. If the main objective of the regulation is to limit play of heavy gamers, it becomes challenging to argue the effectiveness of this regulation in preventing excessive gaming among heavy gamers.

We now turn to the estimation results for spending behavior (Table 6, columns (6)–(10)). Without PSM and heterogeneity controls, we can see from Table 6, column (6)–(8), that the main regulation effect is negative and significant in all the models, indicating the regulation impact on game spending is significant and negative. Although this seems consistent with what the gaming industry argues, once we control for selection through PSM and for unobserved heterogeneity, the main effect of the shutdown law no longer remains significant. Moreover, the moderating effect of past spending is also not significant. These results further underscore the need to include rich controls for selection and unobserved heterogeneity when assessing the causal impact of the policy on gamer behavior. Not accounting for such biases could result in an erroneous evaluation of the policy's impact.

Our results consistently show that whereas game usage actually does change, expenditures do not. Note that spending cash while playing (free) games is associated with heavy gamers; that is, paying gamers account for about 30% of total gamers, although their collective usage time in the preceding month accounts for 60% of total usage time. We argue that while play time can increase using nonregulated hours for heavy gamers, the same cannot be said for out-of-pocket expenses made within the game, especially for the young, school-going gamers who make up our treatment group. In response to the concerns of the gaming industry, then, we show that this regulation does not harm the revenue generation of companies operating free-to-play games.

To ensure that the above results are consistent, we verify the robustness of the empirical results to the potential violation of the stable unit treatment value assumption (SUTVA), alternative definitions of dependent and independent variables, pseudo–treatment effects, and robustness to different RD age bounds. Because of space constraints, we report the full results for all robustness checks in the online appendix and discuss the SUTVA issue in more detail below.

Potential Violation of the SUTVA

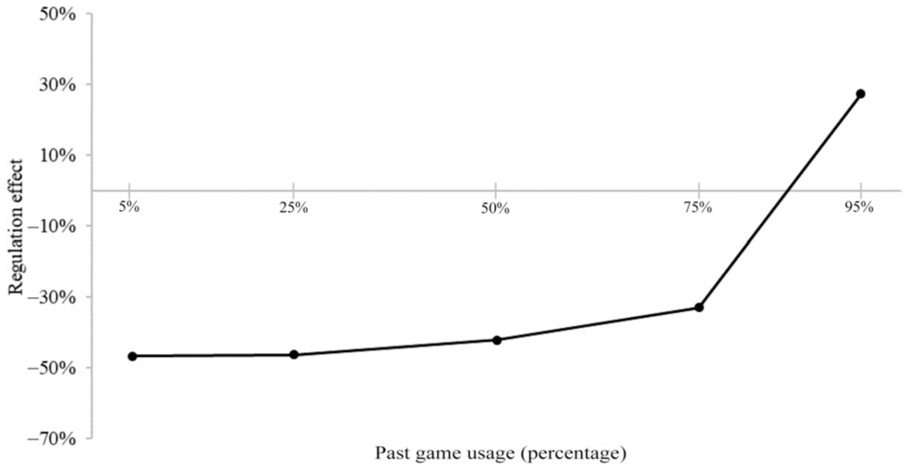
A key assumption in the RD analysis is that there is no significant interference among gamers, commonly referred to as the *stable unit treatment value assumption* (Rosenbaum 2007, Aronow and Samii 2017, Eckles et al. 2017, Athey et al. 2018); that is, given that gamers may play against one another in a peer-versus-peer match, it is possible that the regulation restricting one treated gamer from playing during the curfew hours may also affect the remaining gamers, suggesting potential interference among gamers. Estimating the treatment effect becomes complicated when this interference is significant. In this section, we adopt a three-pronged approach to address these SUTVA concerns. Specifically, we show that (1) the results remain robust to the subset of players who play only with the computer/server (for whom the SUTVA is therefore not violated), (2) network effects do not have a

Table 6. DID Model Results from RD Sample

	Total play time					Total spending				
	Treatment only	Adding interaction	Controlling observed heterogeneity	Matching propensity scores	Controlling unobserved heterogeneity	Treatment only	Adding interaction	Controlling observed heterogeneity	Matching propensity scores	Controlling unobserved heterogeneity
Model→	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TR_{it}	–0.470**	–0.643**	–0.625**	–0.686**	–0.467**	–1.297*	–1.521**	–1.254*	–0.898	0.299
	(0.090)	(0.110)	(0.114)	(0.107)	(0.135)	(0.559)	(0.586)	(0.566)	(0.611)	(0.752)
$LagOutcome_{it}$	5.122**	4.867**	4.884**	5.184**	5.210**	0.176**	0.163**	0.091**	0.114*	0.109*
	(0.296)	(0.304)	(0.304)	(0.231)	(0.231)	(0.031)	(0.031)	(0.029)	(0.051)	(0.050)
$TR_{it} \times LagOutcome_{it}$		1.572*	1.560*	1.249*	1.226[a]		0.211	0.111	0.088	0.078
		(0.698)	(0.698)	(0.626)	(0.626)		(0.108)	(0.100)	(0.112)	(0.111)
$LagSpending_{it}/LagTime_{it}$			–0.004	–0.001	–0.001			5.333**	5.756**	6.000**
			(0.008)	(0.009)	(0.009)			(0.610)	(0.839)	(0.859)
$UserTenure_{it}$			–0.006	–0.008	0.001			0.043	0.066	0.014*
			(0.007)	(0.007)	(0.007)			(0.029)	(0.037)	(0.041)
Intercept	2.600**	2.635**	2.763**	2.873**	2.651**	–10.662**	–10.625**	–11.824**	–13.280**	–15.145**
	(0.064)	(0.066)	(0.137)	(0.145)	(0.188)	(0.901)	(0.898)	(1.160)	(1.553)	(1.931)
Propensity score matching	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Time fixed effects	No	No	No	No	Yes	No	No	No	No	Yes
Adjusted R^2 (log-likelihood)	0.334	0.338	0.338	0.326	0.337	(–864.992)	(–863.219)	(–807.110)	(–564.353)	(–558.868)
Observations	5,488	5,488	5,488	4,784	4,784	5,488	5,488	5,488	4,784	4,784

Notes. Standard errors in the total play time models are clustered at the individual level and robust to heteroscedasticity. Total spending is modeled using a Tobit I model.
[a] $p = 0.051$.
*$p < 0.05$; **$p < 0.01$.

Figure 4. Heterogeneous Regulation Effects in Total Play Time by Past Game Usage



Note. The regulation effects are computed at the 5th, 25th, 50th, 75th, and 95th percentiles of past game usage.

significant influence in the current context, and (3) the results remain consistent even after applying some recent developments in econometrics dealing with the SUTVA issue. We elaborate on this as follows.

First, we reestimate the DID models on gamers who *did not* participate in peer-versus-peer matches ($N_{\text{treatment}} = 195$; $N_{\text{control}} = 214$). Because the participation to interact with other gamers is optional, we can safely rule out violation of the SUTVA at least within those gamers who never played with others. We keep the original empirical model in our main analyses and report the results in Table 7. We can see that our main results remain qualitatively robust; that is, as in Table 6, the shutdown law does not seem to have its intended negative effect on heavy gamers, whereas it does on light gamers. Clearly, our main findings are not attributable to the SUTVA violation.

Second, we additionally test whether network effects exist in our context. If they do, then the number of gamers on a specific day would positively influence game usage. To examine the association between network traffic and game usage, we revisit the raw data and construct session-level panel data in the preregulation period (a total of 188,557 play sessions). Then we define *network traffic* as the number of gamers active within a five-minute window prior to gamer *i* beginning his or her play session.[18] We then regress game usage on *network traffic* (i.e., the number of active gamers) and include control variables.[19] A significant positive coefficient would suggest that a gamer's propensity to play depends on the number of gamers active at the time. We report the estimation results in Table 8. We find that the extent of network traffic does not significantly influence game usage. Thus, network effects (the SUTVA violation) may not be a significant concern in our context.

Finally, we address the potential SUTVA violation by applying recent developments in econometrics to mitigate biases using proximity-based measures of interference (Miguel and Kremer 2004, Almond et al. 2009). Specifically, as in Clarke (2017), we parse out the spillover effect of the DID estimator using the interaction log between treatment and control groups. We find qualitatively consistent results for this alternative approach; that is, even after controlling for potential interference, the regulation effect remains negative and moderated by gamers' past game usage. Note that this individual-level analysis works as a stronger test than the aggregate counterpart as in Table 8 because it directly controls for network/neighbors' influence. We present the details of the methodology as well as estimation results in Section H of the online appendix.

This discussion suggests that our results are not influenced by the potential violation of the SUTVA. We further demonstrate in the online appendix that our results remain robust to alternative definitions of dependent variables (Section D), independent variables (Section E), different RD age bounds (Section F), and pseudo–treatment effects (Section I).

Table 7. Model Results Considering Gamers Who Never Played Against Others

	Total play time	
Variable	Estimate	Std. error
TR_{it}	−0.330*	0.140
$LagOutcome_{it}$	8.896**	0.829
$TR_{it} \times LagOutcome_{it}$	9.839**	2.221
$LagSpending_{it}$	−0.017**	0.006
$UserTenure_{it}$	−0.003	0.007
Intercept	1.692**	0.194
Time fixed effects	Included	
Adjusted R^2	0.283	
Observations	3,272	

Note. Standard (Std.) errors in the total play time model are clustered at the individual level and robust to heteroscedasticity.
*$p < 0.05$; **$p < 0.01$.

Table 8. The Impact of Network Traffic on the Level of Game Usage

	(1)		(2)	
Network traffic observation window:	[–5 min, 0]		[–5 min, +5 min]	
Variable	Estimate	Std. error	Estimate	Std. error
Network traffic$_{it}$	–0.064	0.061	0.012	0.049
LagTime$_{it}$	0.065**	0.017	0.065**	0.017
LagSpending$_{it}$	0.428**	0.096	0.428**	0.096
UserTenure$_{it}$	–0.005	0.009	–0.005	0.009
Intercept	2.646**	0.148	2.617**	0.147
Adjusted R^2	0.289		0.289	
Observations	188,557		188,557	

Notes. Standard (Std.) errors are clustered at the individual level and robust to heteroscedasticity. All models include individual-, day-, and hour-level fixed effects. min, Minutes.
$^{**}p < 0.01$.

Additional Regulation Effects

Note that throughout the modeling process, we ensure that the (main) policy effect and the moderating effect of past user behavior (interaction effect) remain consistent. To deepen our understanding of the regulation effect, we conduct the following additional checks: (1) regulation effects on new gamers, (2) regulation effects on exit rates, and (3) long-term regulation effects.

Regulation Effects on New Gamers. Although the focus of this research is to assess the impact of usage restriction on existing gamers' behavior, a possible side effect of the policy is that it may change the gaming behavior of new gamers who join after the regulation is implemented. To study this, we apply the RD design to compare gamers who join a few weeks just before the regulation ($N_{\text{treatment}} = 218$; $N_{\text{control}} = 248$) with those who join just after the regulation ($N_{\text{treatment}} = 174$; $N_{\text{control}} = 191$). Then we examine their game usage for four weeks after sign-up. Using the same three-year bandwidth and the same definition for the treatment and control groups, we define a dummy variable $PostJoin_i$ equal to 1 for gamers who join within two months after the regulation and 0 otherwise. To study whether the regulation has an effect on new gamers, we run the following regression:

$$\begin{aligned} Outcome_{it} = \beta_0 &+ \beta_1 TreatmentGroup_i + \beta_2 PostJoin_i \\ &+ \beta_3 TreatmentGroup_i \times PostJoin_i \\ &+ \boldsymbol{\theta Z_{it}} + \tau_t + \varepsilon_{it}, \end{aligned} \tag{2}$$

where β_1 and β_2 capture the regulation effect and the gaming behavior of postregulation sign-ups, respectively, and β_3, the main parameter of interest, measures differences in game usage among treated gamers who join before and after the regulation. If β_3 is significant, it shows that the regulation does influence new sign-ups. As before, total play time is modeled in log-linear form.

Table 9 shows that although new gamers who join after the regulation play more, this increase dissipates in the treatment group. Thus, we can conclude that there is no significant impact of the regulation on gaming behavior among treated gamers who join in the preregulation and postregulation periods. Note that one may still expect the regulation to affect the number of new sign-ups. We thus explore the possibility that because of the regulation, gamers under the age of 16 refrain from signing up for the game. In Section J of the online appendix, we provide evidence that no such regulation effect exists.

Regulation Effects on Exit Rates. We now explore whether the regulation induces gamers to exit at a higher rate. If gamers in the treatment group tend to quit at a faster rate than those in the control group, then perhaps the policy intervention is working to wean treated gamers from gaming. By contrast, if there were no such significant difference in exit rates, it would strengthen our result that the usage restriction does little to prevent overusage.

To explore differences in the exit rates, we assume that a user exits if he or she does not log into the game

Table 9. Regulation Effects on New Gamers

	Total play time	
Variable	Estimate	Std. error
TreatmentGroup$_i$	–0.454**	0.125
PostJoin$_i$	0.326**	0.126
TreatmentGroup$_i$ × *PostJoin*$_i$	0.090	0.180
LagOutcome$_{it}$	4.280**	0.220
Intercept	3.336**	0.106
Time fixed effects	Yes	
Adjusted R^2	0.311	
Observations	3,324	

Note. Standard (Std.) errors are clustered at individual level and robust to heteroscedasticity.
$^{**}p < 0.01$.

during the four-week postintervention period. We then compute the percentage of such gamers in the treatment and control groups using the RD sample (the same bandwidth of three years of age). A chi-squared test shows no significant difference between treatment and control groups' exit rates ($\chi^2 = 0.721$, $p = 0.396$). To further check for the robustness of this result, we employ a binary logit model treating $exit_i = 1$ as the dependent variable such that $p_i = P(exit_i = 1)$ denotes the probability that a user exits. We estimate the following logit model:

$$\log\left(\frac{p_i}{1-p_i}\right) = \beta_0 + \beta_1 TreatmentGroup_i + \boldsymbol{\theta Z_i}, \qquad (3)$$

where $TreatmentGroup_i = 1$ if user i belongs to the treatment group (age < 16 years) and 0 otherwise, and $\boldsymbol{Z_i}$ is a vector of gamer-level control variables including average play time and spending amount in the preregulation period and the number of weeks since sign-up. As with the chi-squared test, Table 10 shows that exit rates between the treatment and control groups are not significantly different. This suggests that the regulation does not affect the rate at which users exit from the focal game.

Long-Term Regulation Effects on Game Usage. We show that the regulation has a significant and immediate effect on gaming behavior; however, this effect may diminish (or increase) over time. We thus explore whether the regulation effect persists over time. Although we do not have a long enough time series to estimate time-varying coefficients, we investigate the regulation impact one, two, and three months into the regulation implementation.[20] In keeping with the previous identification strategy, we use the same RD sample and estimate the following:

$$\begin{aligned} Outcome_{it} = \beta_0 &+ \sum_{m=1}^{3} \beta_{1m} TR_{it} \times PostMonth_m \\ &+ \sum_{m=1}^{3} \beta_{2m} TR_{it} \times PostMonth_m \times HeavyGamer_i \\ &+ \boldsymbol{\theta Z_{it}} + \tau_t + \varepsilon_{it}. \end{aligned} \qquad (4)$$

Table 10. Regulation Effects on Exit Rates

	Postregulation exit rates	
Variable	Estimate	Std. error
$TreatmentGroup_i$	−0.351	0.211
$PreTime_i$	−24.251**	3.401
$PreSpending_i$	0.031	0.065
$UserTenure_i$	−0.006	0.016
Intercept	0.204	0.361
Log-likelihood	−279.488	
Observations	686	

**$p < 0.01$.

Table 11. Long-Term Regulation Effects on Game Usage

	Total play time	
Variable	Estimate	Std. error
$TR_{it} \times PostMonth1_t$	−0.512**	0.133
$TR_{it} \times PostMonth2_t$	−0.352**	0.136
$TR_{it} \times PostMonth3_t$	−0.295*	0.136
$TR_{it} \times PostMonth1_t \times HeavyGamer_i$	0.824**	0.223
$TR_{it} \times PostMonth2_t \times HeavyGamer_i$	0.568*	0.252
$TR_{it} \times PostMonth3_t \times HeavyGamer_i$	0.595*	0.248
$LagOutcome_{it}$	4.983*	0.334
$LagSpending_{it}$	−0.008	0.007
$UserTenure_{it}$	0.007	0.008
Intercept	2.552**	0.195
Propensity score matching	Yes	
Time fixed effects	Yes	
Adjusted R^2	0.324	
Observations	9,568	

Note. Standard (Std.) errors are clustered at the individual level and robust to heteroscedasticity.
*$p < 0.05$; **$p < 0.01$.

The outcome of interest is total play time in log form. The terms TR_{it} and $HeavyGamer_i$ are dummy variables indicating the treatment group (age < 16 years) and the top 20th percentile of gamers according to past game usage, respectively, and $\boldsymbol{Z_{it}}$ includes control variables. Next, we define the indicator variables for each month ($m = 1, 2, 3$) in the postregulation period such that $PostMonth_m$ is equal to 1 if t falls in the mth month in the postregulation period and 0 otherwise. The terms β_{1m} and β_{2m} capture the average regulation effect for light gamers and the difference in the regulation effect between heavy and light gamers, respectively, for each month in the postregulation period. Statistical significance in β_{1m} and β_{2m} would suggest persistent regulation effects.

Table 11 presents some interesting findings. First, as before, we find that there is a significant reduction in gaming in the first month after the regulation. This main effect of regulation persists in the second and third months as well. Their magnitudes, however, decrease over time from a 51% reduction in play time in the first month to 35% and 24% in the second and third months for light gamers in the treatment group. Second, the differences in the regulation effects between heavy and light gamers are significant and also persist over time. Third, the estimates suggest that the regulation effects stay positive for heavy gamers in the treatment group throughout the three-month postregulation period, with 31%, 22%, and 30% increases in play time in the first, second, and third months, respectively. Taken together, the average regulation effects persist over three months, as does the increased gaming among heavy gamers. This further underscores the need to reconsider usage restrictions as a vehicle to control excessive gaming.

Summary. Overall, we find that the new gamers who are under the purview of the regulation do not change their behavior before and after the implementation; that is, although there are significant differences between the treatment and control groups, those in the treatment group show no difference based on when they sign up. Second, we find that the regulation does not impact the rate at which gamers exit from the focal game. Despite the concern in the online gaming industry, this result implies that the regulation does not induce gamers to leave. Third, we find that the regulation impact persists for the three-month post-regulation period. Specifically, the regulation effect for light gamers remains negative, suggesting that for this group it is effective in restricting overusage. However, the effect is positive and persistent for heavy gamers, suggesting that the regulation does not have the intended negative effect on heavy gamers.

Discussion and Implications

In this section, we summarize the main findings and introduce some theoretical underpinnings that may drive our results. Then we discuss the implications and avenues for future research.

Main Findings

In this research, we study the impact of regulatory bans on consumer behavior in the context of the online gaming industry. Specifically, we examine the impact of Korea's shutdown law on usage and spending behaviors. Empirically, we combine the DID models with the RD design and PSM, address the SUTVA issue, and conduct a battery of robustness checks. Novel and substantive findings emerge. First, the regulation bans do not impact all gamers' behavior uniformly—although the overall game usage for the treatment group decreases significantly, this reduction diminishes as gamers spend more time on game play. This suggests that heavy gamers who actually require intervention to play less find ways to circumvent the regulation. The fact that the effect flips for the heaviest gamers suggests that the ban may actually be counterproductive for reducing gaming.

Second, contrary to expectations, firms do not experience any revenue loss because of the regulation. Because firm revenue comes from cash expenditures within the game and heavy players are the ones making cash expenditures, it stands to reason that the firms do not lose revenue. Third, although the regulation does not affect new gamers' behaviors or gamers' propensity to quit, the regulation effect persists even three months after the ban is implemented. In other words, the negative regulation effect and the positive interaction effect of past usage are significant and persistent in the three-month post-regulation period.

Potential Drivers

Several drivers can be proposed as providing the theoretical underpinnings of our findings. We suggest a driver that research in psychology, marketing, communications, and health has suggested, namely, that individuals react negatively when they perceive a restriction to choices or freedoms (Brehm 1966, Stewart and Martin 1994). More specifically, the reactance theory proposes that a threat to or the elimination of an expected freedom results in attempts to reassert the freedom in the form of "psychological reactance."[21] We conjecture that a similar negative reaction forms the underlying psychological mechanism driving our results. The extent of this reactance depends on how important the freedom is to individuals (Clee and Wicklund 1980); that is, individuals who value the freedom would have a greater negative response to the constraint relative to those who value it less.

Our current study may well be suited to the context of the reactance theory: the restriction on freedom is the regulated nighttime play, and its importance is revealed in past behavior. Thus, the restriction on game play should lead heavy gamers (for whom the freedom being restricted has greater importance) to resist the regulation, thereby resulting in the increase in usage behavior (higher psychological reactance). Light gamers, by contrast, may end up playing less, thus leading to lower psychological reactance. We find evidence for both heavy and light gamers in our empirical analysis.[22]

Another possible driver that may be at play is the conventional utility theory.[23] That is, treating the problem as a constrained time allocation model (Bhat 2005, Jara-Díaz et al. 2008), heavy gamers deriving greater utility from online gaming would first allocate hours toward it, whereas light gamers would simply spend less time on it, thus supporting our empirical results. One could develop a full-fledged analytical model and study comparative statics for regulation changes and the corresponding effects on heavy versus light gamers. In fact, one could even explore the possibility of folding the conventional utility theory explanation within the reactance theory. The development and evidence of such underpinnings, although beyond the scope and purpose of this research, offer a potential avenue for future work.

Implications

This research has implications for policymakers, managers, and academics. Clearly, given that such regulations are intended to prevent excessive gaming, more may need to be done to ensure that the policies being implemented serve their purposes.

For Consumer Protection Policies. From the perspective of policymakers, the design and implementation of an effective public policy will require a deeper understanding of user behaviors and responses. We show that usage restriction laws have different effects on different segments of consumers depending on their past usage behaviors. Our results highlight two important phenomena. First, by preventing light gamers from advancing to heavy gamers, usage restriction laws work well to reduce future potential overconsumption. Second, the restrictions do not work well for heavy gamers to reduce excessive gaming and may well require a more nuanced approach to control already-formed habits. In fact, firms may find increased incentives to compete for heavy users, resulting in further exacerbation of the excessive gaming problem. In order to design effective policies, policymakers will have to gain a deeper understanding of the heterogeneous nature of consumers and the differential impact of such policies across them.

For the Gaming Industry. From the standpoint of the online gaming industry, this research furthers our understanding of consumers in the online gaming context. Specifically, we show that in the event of an external restriction to gaming, heavy users would continue gaming, and little impact would be felt on firm revenue. This has interesting normative implications for industry in that the cultivation of customer loyalty among their consumers may limit the impact of regulatory restrictions.

For Academia. Our research adds to our theoretical understanding in marketing, communication, and psychology in an ever-increasingly digitized world and highlights the importance of considering heterogeneous regulation effects when assessing the effectiveness of consumer protection policies. Our findings could be extended to other excessive consumption contexts, such as mobile phone and social network usage (Alter 2017). For example, the use of mobile phones while driving has been illegal since 2014 in most states in the United States; however, teenagers, the highest-risk candidates, have disregarded the ban, sometimes hiding their phones from view (Halsey 2010). This may well be the same psychological reactance operating, calling for a review of this policy regulation.

In summary, as consumers operate in a more digitized, engaged, and connected world, marketers and policymakers are scrambling to assess the impacts of various policy interventions on consumer behavior. To the best of our knowledge, this study is the first attempt to empirically address the prevalent approach used for curbing excessive gaming from a consumer protection perspective. We believe that our study serves as a foundation for developing appropriate policies for excessive gaming and for other digital overconsumption behaviors such as mobile phone usage.

Limitations and Future Research

This research is subject to limitations, which, in turn, could suggest areas for future research. First, our results are specific to our data context. Future research could validate the findings using other genres of online games, such as massive multiuser online role playing games, and determine whether the results hold for longer time periods. Despite varying magnitudes of the regulation effect, we expect its direction to be consistent. Second, it is plausible that the regulation induces online gamers, especially light ones, to transfer to other forms of games such as video or mobile games. Thus, it would be vital to examine whether there are any spillover effects from the online game regulation to other gaming industries. Third, although this study only scratches the surface in understanding the effectiveness of usage restriction bans as a public policy instrument, there could be several interesting variables that may moderate the regulation effect. Potential candidates include the size of gamers' peer groups, the extent of social interactions between regulated and nonregulated gamers, the average duration of a single game play, and so ob. Fourth, specific to the context at hand (the shutdown law), it might be interesting to investigate the change in behavior among users who turned 16 just after the ban was implemented. Last, given that users rarely make purchases in online gaming, spending data are quite sparse. Although we mitigate this issue through modeling approaches for censored dependent variables and find qualitatively consistent results, we acknowledge this as a limitation. We hope future research helps academia and policymakers to better understand consumers' response to usage restriction laws and thus better protect consumers from excessive usage.

Acknowledgments

The authors thank Jeeyeon, Mingyung, Sanghwa, Yeohong, Yan, Hyeasinn, Yeolim, Jiyeon, Gimun, Yiling, and Youshin for their valuable help on this paper. In addition, the authors gratefully acknowledge insightful comments and suggestions from the two anonymous reviewers, the associate editor, and the senior editor. This paper streams from work started during the first author's master's studies at Yonsei University. All other authors contributed equally.

Endnotes

[1] We use *policy interventions* and *regulations* interchangeably to describe actions by public policy entities.

[2] There are several examples of this at the local, state, and federal levels. For example, in 2011, the Los Angeles City Council enacted laws to restrict the opening of new fast-food restaurants in certain neighborhoods in response to rising obesity (Medina 2011). At the

regional/state government level, South Australia enforced a ban in 2009 on lightweight single-use plastic bags to encourage eco-friendly consumption among consumers (see http://www.epa.sa.gov.au/data_and_publications/all_publications/for_councils/plastic_bag_ban).

[3] See Decree No. 72/2013/ND-CP of July 15, 2013 (https://vnnic.vn/sites/default/files/vanban/Decree%20No72-2013-ND-CP.PDF), on the management, provision, and use of internet services and online information from the Ministry of Information and Communications of the Socialist Republic of Vietnam and *BBC News* (2003).

[4] The collaborating firm is left anonymous to maintain confidentiality.

[5] After the regulation, the firm did not conduct any targeted marketing on its user base, especially to young teenagers, thus ruling out any confounding effects on the firm's side that may be influencing game spending.

[6] For robustness, we also examine gamers who played the game for both pre- and postregulation periods. The results remain qualitatively consistent.

[7] We define heavy and light gamers according to their gaming behavior during the preregulation period not used in the analysis (i.e., eight to five weeks prior to the regulation). Alternative definitions (reported as robustness checks) and the corresponding empirical results are available in Section A of the online appendix. Although we dichotomize users into heavy and light gamers using past game usage for expositional purposes here, the formal empirical analysis treats past game usage as continuous and is robust to other definitions as well.

[8] For the distribution of each variable, please see Section B of the online appendix.

[9] We confirm that this pre- to postregulation difference is statistically significant between the treatment and control groups. We also show visual evidence that such differences exist with the heavy and light gamers as well in Section C of the online appendix.

[10] We estimate alternative dependent variables for usage (per-visit time and visit frequency) and spending (purchase frequency and per-purchase spending) as robustness checks. We present these results in Section D of the online appendix.

[11] As a robustness check, we explore alternative measurements of past behavior such as discretizing it into high versus low usage levels. We report the results in Section E of the online appendix.

[12] RD designs have been used extensively in economics for program evaluation (Imbens and Lemieux 2008, Lalive 2008, Lee and Lemieux 2010) and in marketing (Hartmann et al. 2011, Narayanan and Kalyanam 2015) as well. We thank the associate editor and the review team for suggesting this approach.

[13] We make two key assumptions in the RD design. First, we assume continuity on observables; that is, we assume that individuals just above the threshold can act as a counterfactual to those just below the threshold. Although we cannot observe all the characteristics, the similarity of the observables (such as gaming behavior, tenure, etc.) between the treatment and control groups would support our assumption. The second assumption is that individuals cannot manipulate the assignment variable (age). This manipulation of age is highly unlikely in the current context because game play depends on a unique social security identifier that is tied to a gamer's age. As long as these two assumptions hold, we will be able to isolate the *local* regulation effect.

[14] We examine robustness for one- and two-year bandwidths as well. Even though the sample sizes reduce considerably, the results remain qualitatively consistent. Results are available in Section F of the online appendix.

[15] We thank the senior editor for suggesting this additional analysis.

[16] We compute the size of the effect (−8.3%) by substituting the mean value of $LagOutcome_{it}$ in Equation (1). Thus, this can be interpreted as a treatment effect for a treated gamer with an average past usage.

[17] Recall that the formal empirical analysis treats past game usage as continuous.

[18] The five-minute observation window seems appropriate because gamers spend roughly five minutes in the setup phase, reviewing performance, updating team rosters, and opponent searching for peer-versus-peer matches. Furthermore, as a robustness check, we rerun the analysis by relaxing the *network traffic* definition to include the five-minute windows both before and after the starting time of gamer *i*'s play session.

[19] The control variables include the following three sets of fixed effects. First, individual fixed effects account for unobserved time-invariant heterogeneity across gamers. Second, daily fixed effects control for market factors that can affect all gamers on the same day. Third, hourly fixed effects within a day additionally account for common factors for every gamer in each hourly slot of the day. The control variables also include the effects of gamers' past usage, spending, and tenure.

[20] Our data do not allow us to expand analyses beyond this three-month window. We recognize this as a potential avenue for future research, where one could study the persistence/dynamic effect of the regulation for a longer term.

[21] Such reactance can manifest in a variety of ways: restrictions on smoking (Miller et al. 2006), bans on product usage (Mazis et al. 1973), constraining piracy-related activities (Miyazaki et al. 2009), forced channel migration (Trampe et al. 2014), and stockouts (Fitzsimons 2000), among others.

[22] For robustness, we also investigate whether gamers who predominantly play in the nighttime are affected by the regulation and find qualitatively consistent results. Please see Section K of the online appendix for details.

[23] We thank an anonymous reviewer for suggesting this.

References

Almond D, Edlund L, Palme M (2009) Chernobyl's subclinical legacy: Prenatal exposure to radioactive fallout and school outcomes in Sweden. *Quart. J. Econom.* 124(4):1729–1772.

Alter A (2017) *Irresistible: The Rise of Addictive Technology and the Business of Keeping Us Hooked* (Penguin Press, New York).

Angrist JD, Pischke JS (2009) *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton University Press, Princeton, NJ).

Aronow PM, Samii C (2017) Estimating average causal effects under general interference, with application to a social network experiment. *Ann. Appl. Statist.* 11(4):1912–1947.

Athey S, Eckles D, Imbens GW (2018) Exact *p*-values for network interference. *J. Amer. Statist. Assoc.* 113(521):230–240.

BBC News (2003) Thailand restricts online gamers. *BBC News* (July 8), http://news.bbc.co.uk/2/hi/asia-pacific/3054590.stm.

Bhat C (2005) A multiple discrete–continuous extreme value model: Formulation and application to discretionary time-use decisions. *Transportation Res. Part B: Methodological* 39(8): 679–707.

Brehm JW (1966) *A Theory of Psychological Reactance* (Academic Press, Oxford, UK).

Bureau of Labor Statistics (2013) American time use survey. Accessed March 21, 2019, https://www.bls.gov/news.release/archives/atus_06182014.pdf.

Caplan S, Williams D, Yee N (2009) Problematic internet use and psychosocial well-being among MMO players. *Comput. Human Behav.* 25(6):1312–1319.

Charlton JP, Danforth IDW (2007) Distinguishing addiction and high engagement in the context of online game playing. *Comput. Human Behav.* 23(3):1531–1548.

Clarke D (2017) Estimating difference-in-differences in the presence of spillovers. Working paper, Universidad de Santiago de Chile, Santiago.

Clee MA, Wicklund RA (1980) Consumer behavior and psychological reactance. *J. Consumer Res.* 6(4):389–405.

Dhar T, Baylis K (2011) Fast-food consumption and the ban on advertising targeting children: The Quebec experience. *J. Marketing Res.* 48(5):799–813.

Dharmasena S, Capps O (2012) Intended and unintended consequences of a proposed national tax on sugar-sweetened beverages to combat the U.S. obesity problem. *Health Econom.* 21(6): 669–694.

Eckles D, Karrer B, Ugander J (2017) Design and analysis of experiments in networks: Reducing bias from interference. *J. Causal Inference* 5(1):1–23.

Fitzsimons G (2000) Consumer response to stockouts. *J. Consumer Res.* 27(2):249–266.

Gentile D (2009) Pathological video-game use among youth ages 8 to 18. *Psych. Sci.* 20(5):594–602.

Goldfarb A, Tucker CE (2011) Privacy regulation and online advertising. *Management Sci.* 57(1):57–71.

Halsey A (2010) Laws are not dissuading drivers from texting, report finds. *Washington Post* (September 29), http://www.washingtonpost.com/wp-dyn/content/article/2010/09/28/AR2010092805833.html.

Hartmann W, Nair HS, Narayanan S (2011) Identifying causal marketing mix effects using a regression discontinuity design. *Marketing Sci.* 30(6):1079–1097.

Heo J, Oh J, Subramanian SV, Kim Y, Kawachi I (2014) Addictive internet use among Korean adolescents: A national survey. *PLoS One* 9(2):e87819.

Imbens GW (2000) The role of the propensity score in estimating dose-response functions. *Biometrika* 87(3):706–710.

Imbens GW, Lemieux T (2008) Regression discontinuity designs: A guide to practice. *J. Econometrics* 142(2):615–635.

Jara-Díaz SR, Munizaga MA, Greeven P, Guerra R, Axhausen K (2008) Estimating the value of leisure from a time allocation model. *Transportation Res. Part B: Methodological* 42(10): 946–957.

Khan R, Misra K, Singh V (2016) Will a fat tax work? *Marketing Sci.* 35(1):10–26.

Korea Information Society Development Institute (2014) Video game usage report. KISDI STAT Report 14-04-01, Korea Information Society Development Institute, Jincheon.

Lalive R (2008) How do extended benefits affect unemployment duration? A regression discontinuity approach. *J. Econometrics* 142(2):785–806.

Lee DS (2008) Randomized experiments from non-random selection in U.S. house elections. *J. Econometrics* 142(2):675–697.

Lee DS, Lemieux T (2010) Regression discontinuity designs in economics. *J. Econom. Literature* 48(2):281–355.

Lee J (2011) South Korea pulls plug on late-night adolescent online gamers. *CNN* (November 22), https://www.cnn.com/2011/11/22/world/asia/south-korea-gaming/index.html.

Lo SK, Wang CC, Fang W (2005) Physical interpersonal relationships and social anxiety among online game players. *Cyberpsych. Behav.* 8(1):15–20.

Mazis MB, Settle RB, Leslie DC (1973) Elimination of phosphate detergents and psychological reactance. *J. Marketing Res.* 10(4):390–395.

Medina (2011) In South Los Angeles, new fast-food spots get a "no, thanks." *New York Times* (January 15), http://www.nytimes.com/2011/01/16/us/16fastfood.html.

Miguel E, Kremer M (2004) Worms: Identifying impacts on education and health in the presence of treatment externalities. *Econometrica* 72(1):159–217.

Miller CH, Burgoon M, Grandpre JR, Alvaro EM (2006) Identifying principal risk factors for the initiation of adolescent smoking behaviors: The significance of psychological reactance. *Health Comm.* 19(3):241–252.

Miyazaki AD, Rodriguez AA, Langenderfer J (2009) Price, scarcity, and consumer willingness to purchase pirated media products. *J. Public Policy Marketing* 28(1):71–84.

Narayanan S, Kalyanam K (2015) Position effects in search advertising and their moderators: A regression discontinuity approach. *Marketing Sci.* 34(3):388–407.

Ringold DJ (2002) Boomerang effects in response to public health interventions: Some unintended consequences in the alcoholic beverage market. *J. Consumer Policy* 25(1):27–63.

Robinson M (2015) Korea's internet addiction crisis is getting worse, as teens spend up to 88 hours a week gaming. *Bus. Insider* (March 25), http://www.businessinsider.com/south-korea-online-gaming-addiction-rehab-centers-2015-3.

Rosenbaum PR (1989) Optimal matching for observational studies. *J. Amer. Statist. Assoc.* 84(408):1024–1032.

Rosenbaum PR (2007) Interference between units in randomized experiments. *J. Amer. Statist. Assoc.* 102(477):191–200.

Seiler S, Yao S, Wang W (2017) Does online word of mouth increase demand? (And how?) Evidence from a natural experiment. *Marketing Sci.* 36(6):838–861.

Sharp A, Høj S, Wheeler M (2010) Proscription and its impact on anti-consumption behaviour and attitudes: The case of plastic bags. *J. Consumer Behav.* 9(6):470–484.

Stewart DW, Martin IM (1994) Intended and unintended consequences of warning messages: A review and synthesis of empirical research. *J. Public Policy Marketing* 13(1):1–19.

Trampe D, Konuş U, Verhoef PC (2014) Customer responses to channel migration strategies toward the e-channel. *J. Interactive Marketing* 28(4):257–270.

Van Rooij AJ, Schoenmakers TM, Vermulst AA, Van Den Eijnden RJJM, Van De Mheen D (2011) Online video game addiction: Identification of addicted adolescent gamers. *Addiction* 106(1): 205–212.

Wang Y, Lewis M, Singh V (2015) The unintended consequences of countermarketing strategies: How particular antismoking measures may shift consumers to more dangerous cigarettes. *Marketing Sci.* 35(1):55–72.

Young K (2009) Understanding online gaming addiction and treatment issues for adolescents. *Amer. J. Family Therapy* 37(5):355–372.

Zhang X, Zhu F (2011) Group size and incentives to contribute: A natural experiment at Chinese Wikipedia. *Amer. Econom. Rev.* 101(4):1601–1615.