Restricting Minors' Online Gaming: Compliance, Evasion, and Market Responses in China

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Abstract

In September 2021, China limited minors' online gaming to one hour on Fridays, weekends, and holidays. Using a difference-in-differences analysis of mobile app data, this study finds that the policy significantly reduced gaming app usage while increasing engagement with alternative digital content such as social media and e-books. Although it successfully curtailed gaming time, it also led to unintended outcomes, including gains for book-related apps and declines for education apps. By examining compliance, substitution, and economic consequences, this research advances understanding of digital regulation, time use, and market dynamics in developing economies.

Keywords: Video Games, Anti-Gaming-Addiction Policy, Regulatory Evasion, Digital Regulation

1. Introduction

In recent decades, the mass production of smartphones and widespread internet access have transformed daily life, leading to unprecedented levels of online activity. With this surge in internet usage, especially in mobile gaming, debates have intensified—particularly in China—about the potential impacts of gaming on users' health and productivity. Concerns about gaming addiction, especially among minors, have prompted policymakers, educators, and parents to question whether restrictions on video game access are necessary to mitigate possible harms. This paper explores these issues by analyzing regulatory interventions in China and examining how users and developers adapt to policy restrictions, shedding light on both intended and unintended effects of such policies.

In this study, I analyze the impact of China's recent regulatory restrictions on video gaming by examining a large dataset of mobile app usage and revenue data. Using a difference-in-differences approach, I compare app engagement and revenue patterns before and after the policy implementation, focusing on differences between gaming and non-gaming apps. By leveraging granular data on user engagement, I am able to identify shifts in usage patterns and revenue flows, providing a detailed view of how users respond to imposed restrictions.

The findings reveal significant behavioral shifts in response to the policy. While overall usage of gaming apps decreased among restricted age groups, users simultaneously increased engagement with other digital content categories, such as social media and e-books. This substitution suggests that some users maintained their overall online leisure time by reallocating attention to less regulated apps. Additionally, these non-gaming categories saw an unexpected increase in engagement, illuminating how limiting one aspect of digital consumption may channel user attention—and potentially revenue—elsewhere. These results highlight the complexities of regulating digital consumption and underscore the unintended market dynamics that can emerge from well-intentioned policies.

In addition to its policy relevance—given that China's anti-gaming-addiction measures potentially affect around 300 million minors—this study makes three related contributions to the economics literature. Although previous research addresses many aspects of digital regulation, it has not concentrated extensively on the three areas I highlight: users' adaptive responses to narrowly targeted restrictions, the reshuffling of demand across differentiated digital services, and the implications for time allocation and productivity in an evolving online ecosystem.

First, existing research on digital regulation and user adaptation has made notable progress examining broad restrictions, censorship, and their welfare effects. For example, Allcott et al. (2022) highlight the complexity of self-regulation in digital settings, and Chen and Yang (2019) show how censorship reshapes information flows. Other studies explore the welfare outcomes associated with social media and internet access (Allcott et al., 2020; Malamud et al., 2019), demonstrating economists' skill in using rigorous methods and innovative data. In contrast, my study focuses on a rapidly evolving Chinese market where a narrowly defined, age-targeted policy restricts online gaming specifically. By examining a distinct population (minors) and a single, well-defined activity (gaming), I offer new insights into how precisely targeted interventions can alter user decisions and consumption patterns.

Second, my analysis relates to the extensive literature on substitution and demand estimation in differentiated product markets. Foundational works in industrial organization and applied microeconomics show that when one option becomes less accessible, consumers reoptimize and shift toward close substitutes (Berry, 1994; Berry et al., 1995; Nevo, 2001; Petrin, 2002). Theoretical contributions on differentiated duopolies (Singh and Vives, 1984) underscore how substitutability and strategic interaction determine equilibrium outcomes. Building on these insights, I demonstrate that policy-induced constraints on gaming apps trigger reallocations in digital engagement, revealing substitution dynamics consistent with differentiated product theory but situated in an online environment with numerous competing platforms.

Third, economists have produced a rich body of research linking external conditions and technological shifts to time allocation and productivity (Aguiar and Hurst, 2006; Aguiar et al., 2013; Krueger and Mueller, 2012). Earlier studies often examine broad shocks or general changes in digital access, but I focus on a policy that targets a single online activity. By showing that restricting gaming leads users to redistribute their digital leisure time, rather than increase offline pursuits, I illustrate how fine-grained interventions shape the composition of digital consumption. This contributes to the ongoing dialogue on how policy, consumption choices, and productivity are intertwined, extending the scope of previous work and offering a more nuanced understanding of how digital regulation impacts overall time use.

The paper proceeds as follows. Section I provides background on China's anti-gaming-addiction policy and the characteristics of the mobile app market. Section II describes the data sources, sample selection, and key measures. Section III presents the empirical analysis in three parts: aggregate analysis of usage and revenue patterns, event study methods to examine policy dynamics and validate pre-trends, and difference-in-differences estimates to assess compliance, substitution, and time allocation effects. Finally, Section IV concludes by summarizing the main findings, discussing policy implications, and suggesting directions for future research.

2. Background

2.1. Policy on Minor Access to Online Games in China

Table 1: Legal Maximum Hours Allowed in China for Minors and Adults

Time Period	Weekdays (Minors)	Weekends & Holidays (Minors)	Adults
Before 2019 Oct.	No restriction	No restriction	No restriction
2019 Oct. –2021 Aug.	1.5 hours per day Prohibited 10:00 p.m.–8:00 a.m.	3 hours per day Prohibited 10:00 p.m.–8:00 a.m.	No restriction
After 2021 Aug.	0 hours per day	Only available 8:00 p.m.–9:00 p.m. (Fri.-Sun. and Holidays)	No restriction

Notes: These restrictions apply to online gaming across smartphones, PCs, and consoles, with enforcement via real-name registration and automatic logouts once time expires. Estimates suggest the policy directly affected approximately 107 million minors.
Source: China's National Press and Publication Administration (2021).

The Chinese government has implemented a series of increasingly strict regulations on online gaming for minors in response to concerns over digital addiction, academic performance, and the physical and mental health of youth. The initial regulation, introduced in October 2019, imposed a daily playtime limit for minors (under 18) of 90 minutes on weekdays and 3 hours on weekends and public holidays. Prior to this policy, there were no formal restrictions on gaming time for minors. This was followed by a more stringent policy announced by China's National Press and Publication Administration (NPPA) on August 30, 2021, and effective from September 1, 2021. Notably, this policy was implemented simultaneously nationwide, with uniform restrictions across all provinces, platforms (iOS, Android, and other consoles), and producers. Under the new rules, minors were prohibited from playing online games on weekdays and allowed only one hour of play between 8 PM and 9 PM on Fridays, Saturdays, Sundays, and public holidays (summarized in Table 1). Due to data constraints, this paper primarily focuses on analyzing the impact of the 2021 policy.

The government stated that these regulations were necessary to combat the negative impact of excessive online gaming on minors' physical health and mental well-being, especially for those developing digital addiction. In the notice issued by National Press and Publication Administration of China (2021), titled "Notice on Strengthening the Management and Preventing Minors from Becoming Addicted to Online Games," the purpose is summarized as "resolutely preventing minors from becoming addicted to online games and effectively protecting their physical and mental health." Even though minor online gaming addiction is a consistently debated topic across society, there was no expectation that the policy would be implemented on the announced day; minors, parents, financial markets, and firms had no anticipation effect, and thus no agents responded in advance.

The 2021 policy mandated that online gaming companies implement real-name verification systems integrated with a national database to restrict minors to designated play hours. Companies were required to strictly monitor and enforce these restrictions, with penalties for non-compliance, including fines and license suspensions. The policy also promoted parental involvement by providing monitoring tools, restricted game promotions targeting minors, and intensified content reviews to align with national values and discourage excessive gaming. To comply, the gaming industry introduced software measures requiring certified ID registration, automatic logouts upon reaching time limits, and restricted access to demo or guest accounts. However, minors sometimes circumvent these rules by using adults' IDs, changing their iOS region to locations like Hong Kong or North America, or using VPNs. Given the relatively low cost of evasion, it remains an empirical question whether minors fully comply and reduce gaming time as intended, or instead adapt by leveraging these evasion methods.

2.2. The Mobile App Market in China: iOS Focus within Gaming Regulations

This study focuses on the iOS mobile app sector, a subset of the broader mobile phone market subject to China's Online Game Minor Usage Policy. Although iOS users make up a smaller segment of smartphone users, mobile phones are the primary devices for internet access in China, making this group reasonably representative of the broader population. It is important to note, however, that iOS users may differ in certain demographics, especially among minors.

The 2019 and 2021 policies impacted online games across various platforms, including smartphones, personal computers, and game consoles, but did not apply to offline games or older consoles. This study focuses on the mobile app sector, a subset of the regulatory framework for the Online Game Minor Usage Policy. Android and iOS are the two dominant mobile operating systems. By Q2 2021, Android held a 72.58% global market share, while iOS accounted for 26.6% Statista (2021a). As of July 2021, in China, Android's market share was 78.4% compared to iOS's 20.84% Statista (2021b). Unfortunately, demographic data for both systems are unavailable.

In practice, consumers typically download and install apps from online stores (e.g., the Apple App Store for iOS and various Android app stores). Each mobile app has a dedicated page providing details such as its functions, category, update history, ratings, reviews, and pricing.

2.3. Internet Access and Mobile Usage Patterns in China

Mobile phones are the primary device for internet access in China. As of June 2021, 99.6% of Chinese internet users accessed the internet via mobile phones, compared to 34.6% using desktop computers, 30.8% using laptops, 25.6% using TVs, and 24.9% using tablets China Internet Network Information Center (CNNIC) (2021a). These statistics highlight the dominant role of mobile devices in internet usage, which directly impacts mobile app usage patterns and user engagement in China.

By June 2021, individuals aged 6–19 accounted for 15.7% of Chinese internet users. Among all internet users, 93.4% watched online videos (including short video clips), 63.1% used live-streaming services, 50.4% played online games, and 32.1% accessed online education platforms China Internet Network Information Center (CNNIC) (2021b). These demographics highlight how regulatory changes—such as restrictions on online gaming—could significantly affect young users' interaction with both gaming and educational platforms, which are central to this study.

3. A Simple Model of Downloads, Active Users, and Revenue

3.1. User Dynamics

Let U_t denote the number of active users in period t. I assume that the active user base consists of:

1. A fraction of the previous period's users who remain active.
2. Newly acquired users (downloads) in the current period.

Formally, I write:

$$U_t = \rho\, U_{t-1} + D_t,$$

where

- D_t is the number of *new downloads* (i.e., first-time app installs) in period t.
- ρ ($0 \leq \rho \leq 1$) is the *retention rate*, indicating the fraction of the previous period's users who remain active.

3.2. Revenue as a Proxy for Active Users

Let R_t denote the total revenue generated by the app in period t. Suppose each active user contributes an average of α in revenue per period, encompassing in-app purchases, ads, subscriptions, or other monetization methods. If α is assumed to be constant over time, I can write:

$$R_t = \alpha\, U_t.$$

Substituting from the user dynamics equation above:

$$R_t = \alpha\big(\rho\, U_{t-1} + D_t\big).$$

3.3. Policy Shock: Restricting Younger Users

Suppose a policy aimed at restricting younger users (e.g., limiting access or usage time) takes effect at period $t = t_0$. This policy will effectively reduce some combination of:

- *Retention*: The fraction of younger users who remain active from one period to the next may drop, reducing ρ.
- *Monetization*: Younger users who do remain active might spend less, reducing α.

For simplicity, let us model this as a drop in the overall retention rate from ρ to $\rho' < \rho$ in periods $t \geq t_0$. Then, for $t \geq t_0$,

$$U_t = \rho' \, U_{t-1} + D_t, \quad R_t = \alpha' \, U_t,$$

where α' could also differ from α if the policy restricts in-app spending by minors. This adjustment captures the intuition that the policy shock decreases overall usage among younger users, causing a noticeable shift in both user retention and revenue.

3.4. Interpretation and Assumptions

- **Downloads** (D_t) represent *new user inflow*, since only first-time installations are counted, not updates.
- **Active Users** (U_t) evolve based on retained users (ρU_{t-1} or $\rho' U_{t-1}$ after the policy) plus new downloads D_t.
- **Revenue** (R_t) scales with the total number of active users, modulated by average monetization power α. After the policy, α' may be lower if younger users account for a significant share of in-app spending.
- **Policy Shock** ($t \geq t_0$) changes one or both of ρ and α. This shift can be used to identify how restricting usage by minors affects user growth and revenue.

In this framework, *downloads* track new user acquisition, while *revenue* proxies the total active user base and average monetization. The policy shock thus manifests as a structural change in parameters (ρ, α) in the post-policy period.

4. Empirical Implications with Limited Observability

Following the conceptual model in Section 3, suppose the researcher only observes *revenue* (R_t) and *downloads* (D_t), but not the actual number of active users (U_t). This section summarizes how meaningful empirical analysis can still proceed under these conditions.

4.1. Leveraging Revenue as a Proxy for Active Users

From the model, recall that

$$U_t = \rho\, U_{t-1} + D_t, \qquad R_t = \alpha\, U_t,$$

where ρ is the retention rate, α is the average monetization per active user, D_t denotes new user inflow, and U_t represents the (unobserved) active users. Under the simplifying assumption that α remains constant over time, revenue serves as a close proxy for the number of active users:

$$U_t \approx \frac{R_t}{\alpha}.$$

Thus, substantial movements in R_t may be interpreted as changes in overall engagement or usage.

4.2. Identifying Policy Effects

When a policy restricts younger users at time t_0 (e.g., limiting their usage or imposing curfews), one or both of the parameters ρ (retention) and α (monetization power) may shift. Even without direct data on U_t, this change can be detected through:

1. **Event Study:** Examine trends in R_t and D_t around t_0. A post-policy drop in revenue or retention would appear as a structural break in the time series, revealing how the policy impacts user engagement or spending.
2. **Difference-in-Differences (DiD):** If some apps or user groups are unaffected by the policy, compare changes in R_t (and D_t) for the treated versus control units pre- and post-t_0. This helps isolate the policy's causal effect under parallel trends assumptions.

4.3. Estimating or Inferring Model Parameters

Although U_t is unobserved, one can still estimate ρ and α by exploiting time-series relationships in R_t and D_t. Before the policy (when parameters are assumed stable), the equation

$$R_t = \alpha\big(\rho\, U_{t-1} + D_t\big)$$

can be approximated by

$$R_t \approx \rho\, R_{t-1} + \alpha\, D_t,$$

if $R_{t-1}/\alpha \approx U_{t-1}$. Fitting this relationship for $t < t_0$ identifies baseline values for ρ and α. Shifts after t_0 indicate how the policy affects user retention or per-user monetization.

4.4. Conclusion

In summary, while active user counts (U_t) are unobserved, the combination of downloads (D_t) and revenue (R_t) remains sufficient for a robust empirical strategy. *Downloads* reflect the inflow of new users, and *revenue* serves as a proxy for both the size of the user base and its spending intensity. Policy impacts that alter user retention or monetization power can therefore be inferred by identifying changes in the dynamic relationship between revenue and downloads across the policy boundary.

4.5. Connecting the Difference-in-Differences to the Simple Model

Recall from the conceptual model that an app's revenue in period t, R_{it}, can be expressed as:

$$R_{it} = \alpha_i\Big(\rho_i\, U_{i,t-1} \;+\; D_{i,t}\Big),$$

where $U_{i,t-1}$ denotes the previous period's active users, ρ_i is the fraction of those users who remain active, $D_{i,t}$ captures new downloads, and α_i is the average monetization (revenue per active user).

The policy shock (i.e., the Minor Usage Policy in August 2021) potentially reduces either or both of these parameters for *treated* apps (i.e., game apps):

- $\rho_i \to \rho_i' < \rho_i$: A drop in the retention rate if minor users exit or reduce usage.

- $\alpha_i \to \alpha_i' < \alpha_i$: A drop in the monetization power if minor users are high spenders or if usage time is curtailed.

Hence, for *treated* apps after the policy date, the revenue equation might become:

$$R_{it} = \alpha_i'\Big(\rho_i' U_{i,t-1} \ + \ D_{i,t}\Big), \quad \text{for } t \geq t_0,$$

where t_0 is August 31, 2021.

Link to Regression.. To empirically measure how much the policy affects revenue, I estimate the difference-in-differences (DiD) regression:

$$Y_{it} = \beta \left(\text{Treat}_i \times \text{Post}_t\right) + \gamma X_{it} + \delta_t + \lambda_i + \varepsilon_{it},$$

where $\text{Treat}_i = 1$ for game (treated) apps, and $\text{Post}_t = 1$ for periods after the policy is enacted. In the simplest specification, Y_{it} is $\ln(R_{it})$ (the log of revenue) or revenue levels. The coefficient of interest, β, captures the *additional change* in revenue for treated apps relative to non-treated apps after t_0.

Interpretation of β.. Within the conceptual framework, β reflects the net effect of any shifts in ρ_i or α_i that occur *only* for treated apps (games) after the policy date:

$$\beta \approx \ln\!\Big(\tfrac{\alpha_i'}{\alpha_i}\Big) + \ln\!\Big(\tfrac{\rho_i'}{\rho_i}\Big) \quad \text{(in log-linear form)},$$

or more generally, it indicates whether $\alpha_i' < \alpha_i$, $\rho_i' < \rho_i$, or both, which would cause a drop in revenue among treated apps.

If β is statistically significant and negative, it suggests the policy has *reduced* revenue for game apps relative to non-game apps, consistent with partial or full compliance (fewer minor users or less monetization from minors). Conversely, if β is near zero, it indicates little change in monetization or retention for treated apps, suggesting that minors (or other users) circumvent or are otherwise unaffected by the policy.

Thus, although I only observe revenue (and possibly downloads), our simple model implies that policy-driven changes in these unobserved parameters (ρ_i and α_i) manifest as changes in the treated group's *observed* revenue relative to controls. This relationship underpins the empirical identification in the DiD setup.

5. Data

5.1. Data Sources and Key Variables

The primary dataset used in this study consists of app-level (product-level) time series data, including (estimated) revenue, (estimated) downloads, version updates, and various app characteristics.

This study uses daily app-level estimates of downloads and revenue obtained from Qimai Technology (Qimai Technology, 2024).[1] Qimai aggregates and estimates these metrics using a combination of publicly available data and proprietary modeling. The company, which specializes in mobile growth solutions, derives its revenue from data services and advertising through data analytics. Qimai estimates download and revenue figures by integrating actual data from its partner apps with attributes available on app stores (e.g., rankings, categories, and number of reviews), as well as other proprietary sources. In other words, Qimai's daily download and revenue metrics are modeled from Apple's published ranking history and additional partner data. Qimai also assembles the majority of the information displayed on an app's download page—such as categories, reviews, and ratings—though it does not specify which portions of the data are precise and which are estimates. The authors gratefully acknowledge Qimai for providing access to its platform.

Downloads refer to the number of times an app is initially downloaded and installed by users from an app store (i.e., Apple's App Store). In the dataset, download data is available from August 2016 onwards, capturing the daily number of new downloads for each app. It is important to note that app updates are not counted as new downloads. Therefore, **in the theoretical model, the "download" metric reflects the acquisition of new users rather than repeated interactions by existing users**. Consequently, the download data can be interpreted as a function of new user inflow, helping to distinguish true growth in user base from engagement by existing users.

Revenue refers to the total income generated by an app through various monetization strategies, such as in-app purchases, subscriptions, or paid downloads. This revenue includes all income earned within the iOS system;

[1]Because the data collected from Qimai involve proprietary estimation methods, the authors cannot guarantee exact correspondence to official reported metrics. All analyses, interpretations, and any errors or omissions in this paper are solely the responsibility of the authors and do not reflect the views or guarantees of Qimai.

for some apps, it may also encompass earnings from advertisements for other apps. Typically, Apple retains 30% of the total revenue, while the remaining 70% is divided between the app developers and any distributors involved. In the dataset, revenue data are available starting from July 2019, providing time series of daily revenue figures for each app.[2]

Even though apps have multiple monetization methods, modeling the relationship between revenue and app usage theoretically requires understanding the distribution of different types of users. Each user type is associated with varying levels of engagement (time use) and payment behavior. However, because individual-level data on user types is not available, in this study's setup, revenue can be viewed as a function of the number of active users and the average monetization power for that app. If I assume monetization power is constant overtime, then **revenue effectively serves as a proxy for the number of active users, allowing us to use revenue as an indirect measure of engagement within the app**.

5.2. Sample Construction and Panel Formation

Sampling. This study focuses on popular and actively used apps, as these dominate the market in terms of downloads and revenue. Among the millions of apps available for users, a small number of top-ranked apps account for a disproportionately large share of both metrics. As the app ranking becomes lower (i.e., ranking number increases), the accuracy of the estimated time series data declines. Therefore, a reasonable trade-off between the number of apps and data accuracy is achieved by selecting the top 1,500 apps.

Based on aggregate revenue data from the Apple App Store as of August 1, 2021 (prior to the implementation of the 2021 policy restricting minors' gaming time), I selected the top 1,500 apps in China for analysis. This study focuses solely on app information from the iOS platform, excluding data from the Android system due to the high fragmentation of the Android market in China. The absence of Google Play and many Google services has led to a diverse landscape of major distributors, including Huawei, Xiaomi, VIVO, OPPO, Meizu, Tencent's App Store, Baidu, 360, and other hardware manufacturers and large apps, which complicates data aggregation. By concentrating on iOS, this research requires the assumption that no significant

[2]Alternatively, historical download and revenue ranking data could be used instead of estimates. Other data sources, such as Sensor Tower and DataAI, also provide historical download and revenue data.

substitution effects exist between iOS and Android following policy interventions. This assumption seems reasonable, as gaming restriction policies are unlikely to immediately impact minors' choice of device between Android and iOS. [3]

Balanced Panel. Starting with the top 1,500 revenue-generating apps in China as of August 1, 2021, I construct a balanced panel by applying an inactivity threshold: any app with fewer than 1 download or less than $1 in revenue for seven consecutive days within the two-year study window (one year before and one year after the cutoff) is dropped from the sample. After applying this rule, 791 apps remain in the panel. While somewhat arbitrary, this threshold ensures a sustained level of engagement throughout the study period.

This stable sample composition is particularly beneficial for applying difference-in-differences (DiD) or event study methodologies, as it minimizes biases from sample turnover that could otherwise confound results. By focusing on a consistent set of high-revenue apps, I can better control for selection bias, reducing the impact of temporary popularity spikes. Although this approach limits the analysis to apps with sustained high revenue, which may not fully capture behaviors of lower-revenue or intermittently popular apps, it allows for more accurate attribution of observed changes to the policy intervention, enhancing the robustness and clarity of causal effect estimates.

5.3. Descriptive Statistics and Classification

Classification as a Game and ISBN. In China, game apps must undergo a strict approval process. According to the National Press and Publication Administration (NPPA), all online games, including mobile and PC games, must obtain a publishing license (Banhao, an ISBN for games) before release. [4] In my dataset, I cross-reference the apps with the list of game approvals from the NPPA. If an app has a game ISBN, it is identified as a game. Approximately 53.9% of the apps in my dataset are classified as games.

[3] However, if there is evidence that the Android platform allows for greater jail-breaking capabilities or other methods to circumvent regulations, a substitution effect from iOS to Android could potentially arise.

[4] A game cannot simply label itself as an "educational app" to bypass this requirement. Even if a game has educational content, it still requires a license if it includes game-like elements, such as competition, rankings, or virtual currency.

Apps are also classified by Apple's age ratings: 4+, 9+, 12+, and 17+, indicating suitability for different age groups. For instance, 4+ is suitable for young children, 9+ for older children, 12+ for teens, and 17+ for mature audiences. In addition to the "Game" category, the dataset includes a variety of app categories provided by iOS, such as Books, Business, Education, Entertainment, Finance, Food & Drink, Graphics & Design, Health & Fitness, Lifestyle, Magazines & Newspapers, Medical, Music, Navigation, News, Photo & Video, Productivity, Reference, Shopping, Social Networking, Sports, Stickers, Travel, Utilities, and Weather.[5]

Time-invariant Characteristics. A comprehensive list of app characteristics considered in the empirical analysis is provided in Table 2, which includes both Panel A (Top 1500 Apps) and Panel B (Balanced Panel). Panel B represents the subset of apps that meet the continuity threshold, excluding apps inactive for seven consecutive days (defined as having fewer than 1 download or less than $1 in revenue) within the study period.

[5]Within the "Game" category, iOS subcategories include Action, Adventure, Arcade, Board, Card, Casino, Casual, Educational, Family, Music, Puzzle, Racing, Role-Playing, Simulation, Sports, Strategy, Trivia, and Word. These subcategories aid users in finding games of interest and provide insight into the app's primary features.

Table 2: Summary Statistics of App Data

Characteristics	(1) N	(2) Mean	(3) SD	(4) Min	(5) Max
Panel A: Top 1500 Apps					
Content Ratings (sum to 100%)					
Content Rating Age: 4+	1,450	0.277	0.448	0	1
Content Rating Age: 9+	1,450	0.143	0.351	0	1
Content Rating Age: 12+	1,450	0.268	0.443	0	1
Content Rating Age: 17+	1,450	0.311	0.463	0	1
App Categories (sum to 100%)					
Game	1,450	0.539	0.499	0	1
Recreation	1,450	0.243	0.429	0	1
Non-Game & Non-Recreation	1,450	0.219	0.414	0	1
Developer Information					
Number of Unique Developers	988	-	-	-	-
Top Developer: Tencent Mobile Games	86	-	-	-	-
Top Developer: NetEase Mobile Games	59	-	-	-	-
Top Developer: Shenzhen Tencent Tianyou Technology Ltd	17	-	-	-	-
Top Developer: Thunder Games	10	-	-	-	-
Top Developer: Shanghai Youzu Information Technology Co., Ltd.	10	-	-	-	-
Top Developer: Easy Tiger Apps, LLC.	10	-	-	-	-
Top Developer: Shanghai Hode Information Technology Co., Ltd.	9	-	-	-	-
Top Developer: Tencent Technology (Shanghai) Co., Ltd	8	-	-	-	-
Top Developer: X.D. Network Inc.	8	-	-	-	-
Panel B: Balanced Panel					
Content Ratings (sum to 100%)					
Content Rating Age: 4+	791	0.276	0.447	0	1
Content Rating Age: 9+	791	0.110	0.313	0	1
Content Rating Age: 12+	791	0.271	0.445	0	1
Content Rating Age: 17+	791	0.344	0.475	0	1
App Categories (sum to 100%)					
Game	791	0.516	0.500	0	1
Recreation	791	0.259	0.438	0	1
Non-Game & Non-Recreation	791	0.225	0.418	0	1
Developer Information					
Number of Unique Developers	509	-	-	-	-
Top Developer: Tencent Mobile Games	86	-	-	-	-
Top Developer: NetEase Mobile Games	59	-	-	-	-
Top Developer: Shenzhen Tencent Tianyou Technology Ltd	17	-	-	-	-
Top Developer: Thunder Games	10	-	-	-	-
Top Developer: Shanghai Youzu Information Technology Co., Ltd.	10	-	-	-	-
Top Developer: Easy Tiger Apps, LLC.	10	-	-	-	-
Top Developer: Shanghai Hode Information Technology Co., Ltd.	9	-	-	-	-
Top Developer: Tencent Technology (Shanghai) Co., Ltd	8	-	-	-	-
Top Developer: X.D. Network Inc.	8	-	-	-	-

Notes: This table provides a summary of the descriptive statistics for various app characteristics. Panel A represents the Top 1500 Apps, while Panel B represents the Balanced Panel. Content ratings and app categories sum to 100%, indicating complete representation within each group. The table also includes the number of unique developers and the top developers based on app counts. Data are collected from Qimai, iOS, and the official websites of apps.

6. Reduced-Form Evidence of Effect

This section examines the impact of China's 2021 gaming policy on app market dynamics. The aggregate analysis highlights overall trends in revenue and downloads across categories. The event study analysis allows readers to observe the dynamics of policy effects on games and spillovers across app types. The difference-in-differences analysis provides tabular results summarizing compliance and spillover effects, offering a concise evaluation of the policy's impact.

6.1. Aggregate Analysis

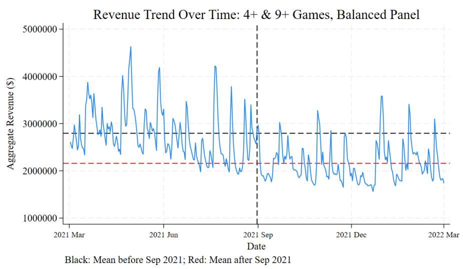
The figures in this section provide a comprehensive view of the aggregate trends in revenue and downloads for various categories of mobile apps in response to the policy change introduced on September 1, 2021. Specifically, the analysis focuses on games with different age ratings, recreational apps, and non-recreational non-game apps, allowing us to observe the distinct impacts across categories.

Figures 3a, 3c, and 1e reveal a clear decrease in aggregate revenue for games rated 4+, 9+, and 12+ following the policy's implementation. This reduction in revenue suggests that the policy targeting gaming behavior among minors effectively curtailed spending within these age-rated game categories. Prior to the policy's enactment, revenue levels for these categories were relatively stable or even increasing, as seen in the figures. However, the introduction of restrictions led to a noticeable decline in aggregate revenue, depicted by the red lines, which represent the mean revenue after September 1, 2021, consistently falling below pre-policy levels. These trends underscore the effectiveness of the policy in reducing revenue in games that cater primarily to younger audiences.

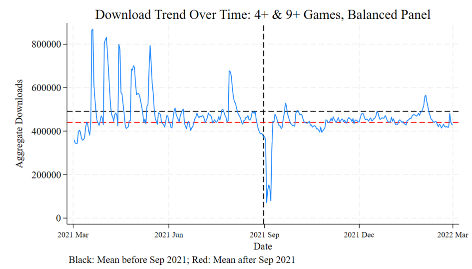
In contrast, Figures 2a and 2b depict an increase in both revenue and downloads for recreational apps post-policy. This upward trend can be attributed to a potential substitution effect, where users who previously engaged with games may have shifted their time and spending toward recreational apps as an alternative form of entertainment. The aggregate analysis shows that, unlike games with age restrictions, recreational apps experienced growth in response to the policy, suggesting that some consumer behavior shifted from gaming to other recreational activities in the app market.

Lastly, Figures 2c and 2d show that non-recreational, non-game apps maintained relatively consistent revenue and download levels before and after the policy. This stability indicates that the policy specifically impacted gaming and recreational sectors, while other categories of apps remained unaffected. The non-recreational apps, which include productivity, utility, and educational apps, did not exhibit significant changes in response to the restrictions, highlighting the targeted nature of the policy's effect on digital entertainment.

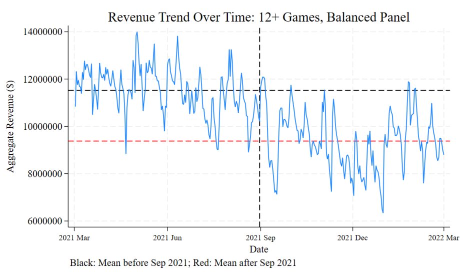
In summary, these figures illustrate a differential impact across app categories: games targeting younger users experienced revenue declines, recreational apps benefited from a substitution effect with increased engagement, and non-recreational apps remained largely unaffected. This analysis provides reduced-form evidence of the policy's targeted effects on app usage and revenue patterns in the mobile app ecosystem.



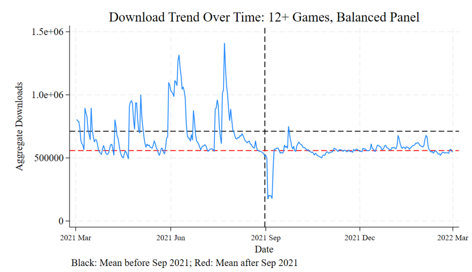
(a) 4+ & 9+ Games: Revenue Trend



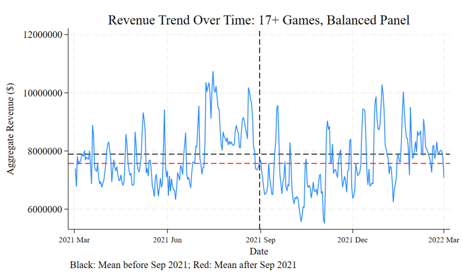
(b) 4+ & 9+ Games: Download Trend



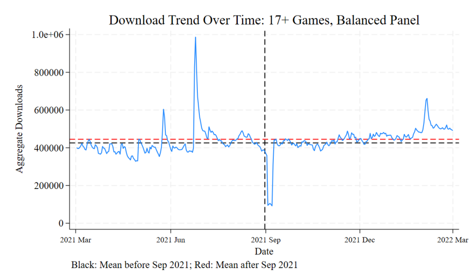
(c) 12+ Games: Revenue Trend



(d) 12+ Games: Download Trend

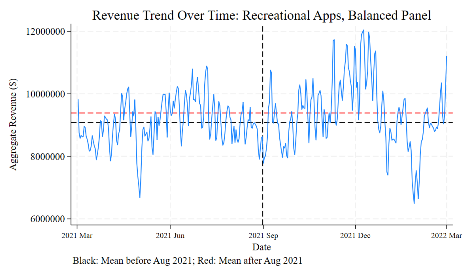


(e) 17+ Games: Revenue Trend

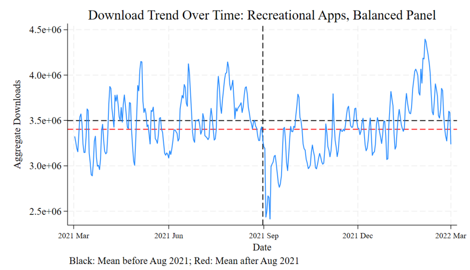


(f) 17+ Games: Download Trend

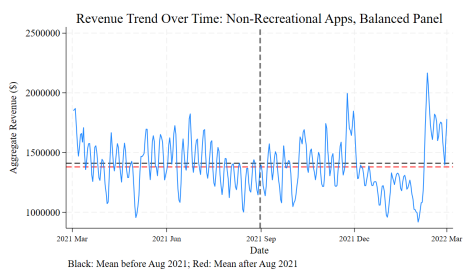
Figure 1: Aggregate Trends in Revenue and Downloads for Game Categories (Balanced Panel)



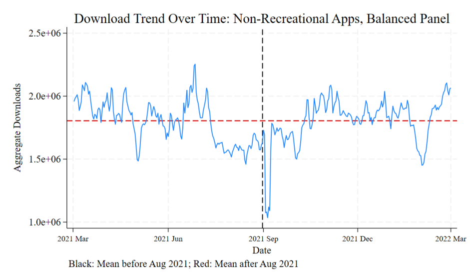
(a) Recreational Apps: Revenue Trend



(b) Recreational Apps: Download Trend



(c) Non-Recreational Apps: Revenue Trend



(d) Non-Recreational Apps: Download Trend

Figure 2: Aggregate Trends in Revenue and Downloads for Non-Game Categories (Balanced Panel)

6.2. *Event Study Analysis*

6.2.1. *Game vs. Non-Game*

The event study analysis in Figures 3b and 3c reveals that the revenue for games rated 9+ and 12+ experienced a significant decline following the implementation of the 2021 minor policy in China, suggesting the policy's effectiveness in curbing gaming activity among minors. Specifically, revenue for 9+ games decreased by approximately 20%, while revenue for 12+ games saw a decline of about 10%. These findings highlight the policy's targeted impact on reducing engagement within age-restricted gaming categories. Conversely, the effect on revenue for 4+ and 17+ games, shown in Figures 3a and 3d, was not statistically significant, indicating that the policy's influence was more pronounced in the intermediate age groups.

It is important to note that the age ratings (4+, 9+, 12+, 17+) do not necessarily reflect the actual ages of the app users but rather the recommended content suitability for those age groups. For instance, 4+ indicates that the app is suitable for all ages, not that the majority of users are 4 years old. Different content age ratings may be associated with varying age distributions among app users, potentially influencing the observed revenue trends.

6.2.2. *Market Spillover Effect*

The event study analysis in Figure 4a reveals a clear upward trend in revenue for social media apps following the 2021 minor policy implementation, suggesting a robust spillover effect. This indicates that social media apps acted as substitutes for gaming apps, as users likely reallocated their time to social platforms when access to gaming was restricted.

In Figure 4b, book apps also demonstrate a significant upward trend in revenue post-policy, reflecting another prominent substitution effect. Users may have shifted their engagement to book apps as an alternative leisure activity, further showcasing the policy's influence on user behavior.

Conversely, Figure 4c shows that entertainment apps experienced only minor increases in revenue post-policy, with changes that were not statistically significant. This finding implies that entertainment apps were not major substitutes for gaming apps, and no substantial spillover effect was observed in this category.

Finally, Figure 4d indicates that revenue for education apps remained relatively flat, showing no discernible changes following the policy. This suggests that users did not reallocate their time toward educational platforms in response to the restrictions on gaming apps.

In summary, the policy-induced restrictions on gaming apps resulted in significant spillover effects for certain app categories, particularly social media and book apps. These categories absorbed displaced gaming app users, while entertainment and education apps showed minimal or no spillover effects, highlighting the nuanced impact of regulatory interventions on digital market dynamics.

6.2.3. Discussion

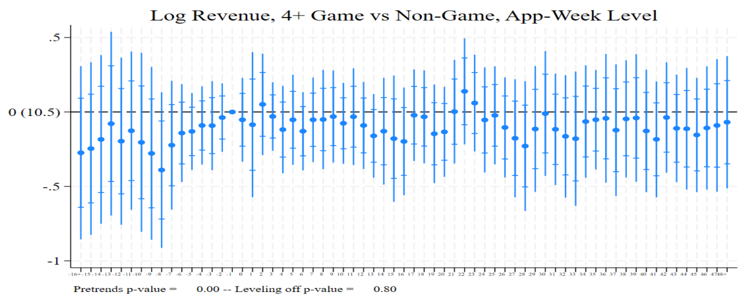
Revenue, being a function of active users and monetization power, serves as a proxy for changes in active user numbers under the assumption that monetization power remains consistent over time. The observed revenue trends, therefore, reflect shifts in user engagement across app categories following the policy's implementation.

The analysis in this section focuses on revenue trends. While I have included all the download trend analyses in the appendix, I deliberately prioritize revenue as it serves as a better proxy for user engagement. Download trends primarily capture new user behavior, whereas revenue reflects the activity of aggregate (active) users. This distinction makes revenue inherently more suitable for evaluating engagement. Additionally, download trends are more volatile compared to revenue, making them less reliable for capturing consistent patterns over time.

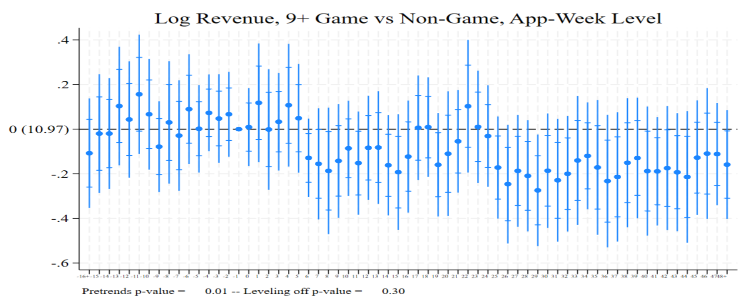
Interestingly, the trend of downloads does not necessarily align with the trend of revenue. This divergence suggests that the policy's impact was primarily concentrated on aggregate active users rather than new user acquisition. In other words, the number of new users appears to be relatively unaffected by the policy. Instead, the policy influenced the behavior of existing users, likely by limiting their usage.

A plausible mechanism for this phenomenon is that children and teenagers, who were the primary targets of the policy, may have continued using apps already installed on their devices instead of seeking out new ones. This behavioral shift would explain why downloads did not decrease significantly while revenue—driven by the engagement of active users—experienced noticeable changes. This finding underscores the role of existing app usage patterns in mitigating the policy's impact on new user acquisition while amplifying its effect on the activity levels of existing users.

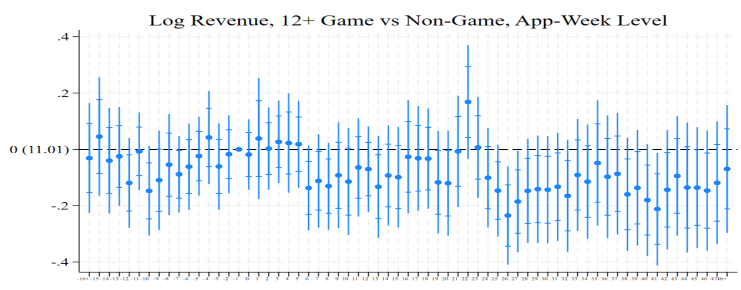
Regarding the window selection, I focus on this specific time-frame because the 2021 minor usage policy (implemented in September 2021) falls within the broader regime of the COVID-19 pandemic. The data indicates a significant regime shift around April 2021, five months before the policy, which I attribute to the spread of the Delta variant and subsequent lockdown measures across China. Additionally, it is important to note that China ended its strict "zero COVID" policy in December 2022, fifteen months after the implementation of the 2021 minor usage policy. To ensure the validity of the pretrend analysis, I only include data after the April 2021 regime shift, capturing trends that are directly comparable within the transformed regional and behavioral context. This approach minimizes confounding effects and aligns the analysis with a stable pre-policy environment.



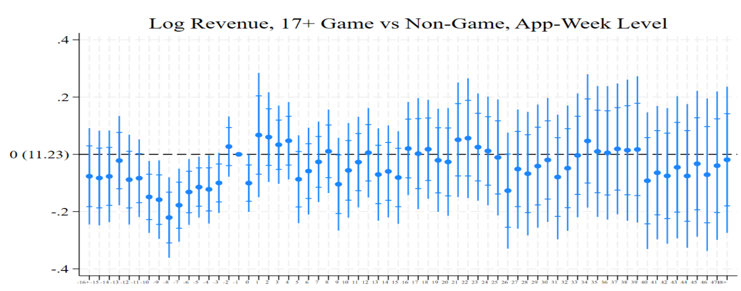
(a) 4+ Games vs. Non-Games



(b) 9+ Games vs. Non-Games



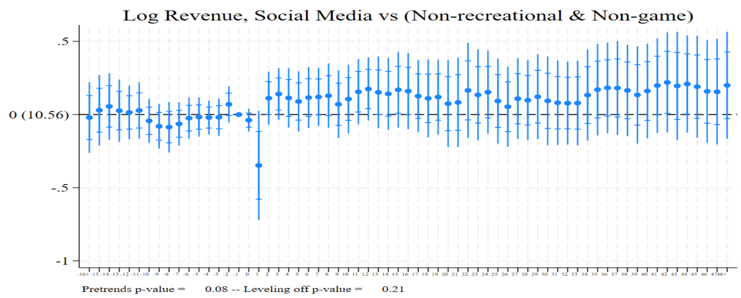
(c) 12+ Games vs. Non-Games



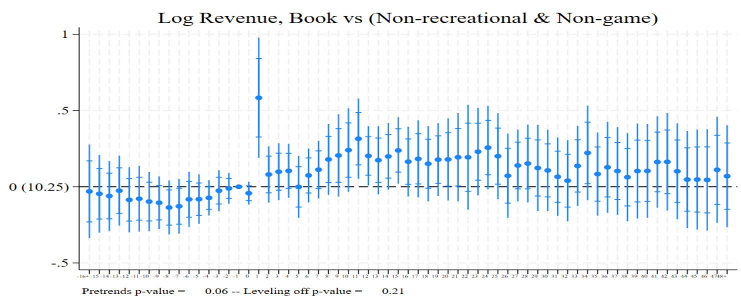
(d) 17+ Games vs. Non-Games

Figure 3: Event Study Results for Log Revenue

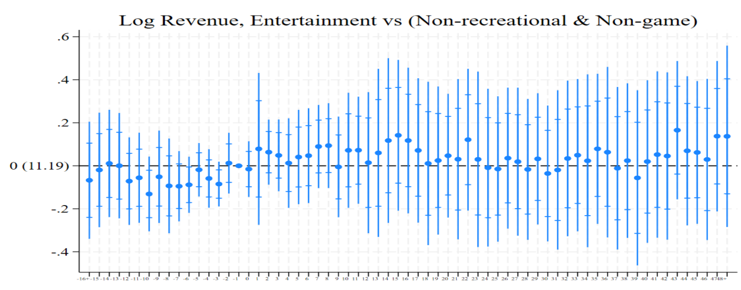
This figure displays event study results for age-rated games compared to non-games at the app-week level. Each panel shows log revenue differences over time relative to policy implementation, with 95% sup-t and pointwise confidence intervals.



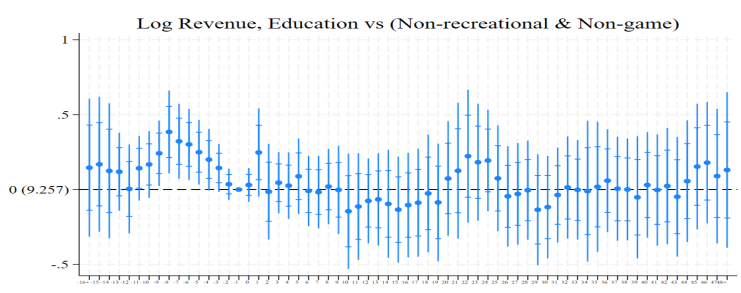
(a) Social Media vs. Non-Recreational Apps



(b) Books vs. Non-Recreational Apps



(c) Entertainment vs. Non-Recreational Apps



(d) Education vs. Non-Recreational Apps

Figure 4: Event Study Results for Log Revenue Across App Categories
This figure displays event study results for social media, books, entertainment, and education apps compared to non-recreational and non-game apps. Each panel shows log revenue differences over time relative to policy implementation, with 95% sup-t and pointwise confidence intervals.

6.3. Difference-in-Differences Analysis

In this section, I use the August 2021 Minor Usage Policy to test whether compliance exists, i.e., restriction on minor usage has effect on consumer demand and usage on gaming and spillover effect on other app market. If null hypothesis is hold, it means that compliance does not exist and minors in general have ability to evade the restriction. If null hypothesis is rejected, it means at least partial compliance exists and the policy has intended effect. My main demand outcomes are aggregate and app-level revenue, since i do not have direct data on app usage. As discussed in previous subsection, I believe the revenue is a better proxy compared to download.

The main strategy uses a treatment effects difference-in-differences approach.

$$Y_{it} = \beta \text{Treat}_i \times \text{Post}_t + \gamma X_{it} + \delta_t + \lambda_i + \varepsilon_{it}, \quad (1)$$

where δ_t represents time fixed effects, λ_i denotes individual-app fixed effects, Post_t is a dummy variable equal to one for periods after the policy implementation on August 31, 2021, and zero otherwise, and Treat_i is a dummy variable equal to one for game apps and zero for non-game apps.

The key parameter of interest in this regression is β, which captures the differential effect of the policy on treated apps relative to the non-treated apps. This specification uses the non-treated group (non-game and non-recreational apps) as the baseline for comparison, enabling the identification of the policy's impact on revenue or other outcome variables.

6.3.1. Compliance Results

The results in Table 3 provide insights into the differential impacts of policy changes across various age-rated game categories when compared to all non-game apps. For 9+ games, the revenue shows a significant decrease of approximately 12.1% (coefficient = -0.121, $p = 0.0089$), indicating strong compliance with the policy, as this age group likely represents a substantial portion of games popular among minors. Similarly, 12+ games also exhibit a decline in revenue of about 3.2% (coefficient = -0.032, $p = 0.3676$), though the effect is not statistically significant. In contrast, revenue for 4+ games and 17+ games increases by approximately 9.1% and 7.6% (coefficients = 0.091, $p = 0.402$, and 0.076, $p = 0.395$, respectively), but neither result is statistically significant, suggesting that these categories were less affected by the policy or cater to a different demographic.

The sharp and significant decline in revenue for 9+ games aligns with the idea that these games are the primary attractors for minor gamers, and the policy successfully curtailed their engagement. The smaller and statistically insignificant decrease for 12+ games further supports this interpretation. Together, these results suggest a degree of compliance among users, particularly minors, with the imposed regulations. The insignificant increases for 4+ and 17+ games further reinforce that the policy's primary impact was concentrated on game categories most associated with younger users.

Table 3: Reduced-Form Compliance Evidence

Outcome Variable	Log Revenue (1)	Log Revenue (2)	Log Revenue (3)	Log Revenue (4)
Games × Post	0.091 (0.107)	-0.121 (0.046)	-0.032 (0.036)	0.076 (0.038)
Sample	4+ Games vs All Non-Games	9+ Games vs All Non-Games	12+ Games vs All Non-Games	17+ Games vs All Non-Games
Unit of Observation	App-week	App-week	App-week	App-week
Sample Period	05/11/2021–03/15/2022	05/11/2021–03/15/2022	05/11/2021–03/15/2022	05/11/2021–03/15/2022
Year/Week Fixed Effects	•	•	•	•
App Fixed Effects	•	•	•	•
App Controls				
Observations	19,109	20,039	23,715	22,635
R^2	0.938	0.942	0.945	0.949

Notes: This table presents the results of compliance evidence using reduced-form analysis. Columns (1), (2), (3), and (4) provide estimates for games × post interaction in a 16-week pre and 32-week post window for 4+, 9+, 12+, and 17+ Games vs All Non-Games, respectively. Standard errors are clustered at the app level and reported in parentheses. Data sources include Qimai, iOS, and official app websites.

6.3.2. Market Spillover Results

The results in Table 4 provide evidence of market spillovers from policy changes across various app categories compared to non-recreational apps.

In Panel A, the books category experienced a significant revenue increase of approximately 23.0% ($p < 0.001$), which is both economically and statistically significant. This finding highlights a notable positive spillover effect, potentially driven by user attention or spending being redirected toward this category in response to the policy. Conversely, revenue increases for the music, social media, and entertainment categories ($p = 0.3313$, $p = 0.1287$, and $p = 0.2466$, respectively) are not statistically significant, suggesting inconclusive evidence of spillovers in these areas.

In Panel B, the education category exhibited a significant revenue decline of 17.2% ($p = 0.0168$), which is both economically and statistically significant. This result reflects a negative spillover effect, likely caused by users reallocating their attention or spending away from education-related apps following the policy change. In contrast, the photo and video and health and fitness categories showed revenue declines ($p = 0.2686$ and $p = 0.2694$, respectively), but these effects are not statistically significant, indicating limited evidence of negative spillovers for these categories.

These findings underscore the heterogeneous impacts of the policy changes, with economically and statistically significant positive effects observed for books and negative effects for education. The results for other categories, while suggestive, remain inconclusive and highlight the complexity of market dynamics in response to regulatory interventions.

Table 4: Reduced-Form Market Spillover Evidence

		Panel A: Primary Categories		
Outcome Variable	Log Revenue	Log Revenue	Log Revenue	Log Revenue
	(1)	(2)	(3)	(4)
Treat × Post	0.230	0.094	0.108	0.093
	(0.054)	(0.096)	(0.071)	(0.080)
Unit of Observation	App-week	App-week	App-week	App-week
Sample Period	05/11/2021–03/15/2022	05/11/2021–03/15/2022	05/11/2021–03/15/2022	05/11/2021–03/15/2022
Sample	Books vs Non-Recreational	Music vs Non-Recreation	Social Media vs Non-Recreation	Entertainment vs Non-Recreation
Year/Week Fixed Effects	•	•	•	•
App Fixed Effects	•	•	•	•
App Controls				
Observations	9,946	9,015	10,338	9,750
R^2	0.903	0.905	0.911	0.930
		Panel B: Secondary Categories		
Outcome Variable	Log Revenue	Log Revenue	Log Revenue	
	(5)	(6)	(7)	
Treat × Post	-0.072	-0.108	-0.172	
	(0.065)	(0.097)	(0.071)	
Unit of Observation	App-week	App-week	App-week	
Sample Period	05/11/2021–03/15/2022	05/11/2021–03/15/2022	05/11/2021–03/15/2022	
Sample	Photo & Video vs Non-Recreational	Health & Fitness vs Other Non-Recreation	Education vs Other Non-Recreation	
Year/Week Fixed Effects	•	•	•	
App Fixed Effects	•	•	•	
App Controls				
Observations	9,946	8,084	8,084	
R^2	0.896	0.882	0.882	

Notes: This table presents the results of market spillover evidence using reduced-form analysis. **Panel A** includes Books, Music, Social Media, and Entertainment categories, while **Panel B** includes Photo & Video, Health & Fitness, and Education categories. Each column provides estimates for the respective categories vs Non-Recreation in a 16-week pre and 32-week post window. Standard errors are clustered at the app level and reported in parentheses. Data sources include Qimai, iOS, and official app websites.

7. Weekday–Weekend Variations

An important advantage of using daily app-level data is the ability to distinguish between weekdays and weekends, which is critical for understanding the heterogeneous effects of China's 2021 gaming restriction. Because the policy explicitly limited minors' gaming hours to *one hour per day only on Fridays, weekends, and holidays* and imposed a complete ban on weekdays, it generates an asymmetric temporal pattern in potential treatment intensity. Therefore, examining weekday–weekend differences provides an additional layer of evidence on compliance and behavioral substitution.

To explore these temporal variations, I estimate separate difference-in-differences regressions for weekdays and weekends, using the same specification as Equation (1) but interacting the treatment indicator with weekday and weekend dummies. This approach isolates the policy's impact during periods when gaming is officially permitted (weekends) versus prohibited (weekdays). The identifying assumption remains that, absent the policy, treated and control apps would have followed parallel trends within each subset of days.

The results reveal strong weekday–weekend asymmetries consistent with the policy design. Revenue and downloads for game apps declined substantially on weekdays relative to control apps, indicating that minors largely complied with the weekday ban or that enforcement mechanisms (such as real-name login and automatic logouts) were effective in curbing weekday activity. In contrast, weekend effects were smaller or even positive, suggesting that minors concentrated their limited playtime during permitted hours. This temporal reallocation underscores that the policy reduced total playtime primarily through weekday restrictions rather than uniformly across the week.

Interestingly, non-game categories such as books and social media apps exhibit complementary patterns: their engagement increased on weekdays—when minors could not play games—but returned toward baseline levels on weekends. This finding reinforces the substitution mechanism identified earlier and shows that the temporal structure of the policy not only altered total time use but also reshaped the intraday and intraweek rhythm of digital engagement.

Taken together, these results highlight that digital restrictions can have highly time-specific behavioral consequences. For policymakers, this suggests that even when total screen time declines only modestly, the timing of digital consumption may shift substantially, potentially affecting study schedules, sleep patterns, or offline activities. Future research could build on this temporal dimension by combining app-level usage with survey or device-level time-use data to better quantify the welfare implications of redistributing online activity across days and hours.

Appendix A. Detailed Derivation (Single-Group Model)

Appendix A.1. 1. Preliminaries: Single-Group Model

I consider an app i whose total revenue in period t is given by:

$$R_{i,t} \ = \ \alpha_{i,t} \left(\rho_{i,t} U_{i,t-1} + D_{i,t}\right),$$

where:

- $\rho_{i,t}$ is the fraction of *all* users from period $t-1$ who remain active in period t.
- $\alpha_{i,t}$ is the average revenue per active user (i.e., monetization power).
- $D_{i,t}$ represents new downloads.
- $U_{i,t-1}$ is the lagged number of active users (unobserved, but implied).

Appendix A.2. 2. Policy Shock for Treated Apps

At time t_0, a policy targeting minors takes effect. - ρ_{0i} and α_{0i} denote the *pre-policy* retention and monetization for app i. - ρ_{1i} and α_{1i} denote the *post-policy* values.

Hence, if app i is *treated* (e.g., a game), I assume:

$$\rho_{i,t} = \begin{cases} \rho_{0i}, & t < t_0, \\ \rho_{1i}, & t \geq t_0, \end{cases} \qquad \alpha_{i,t} = \begin{cases} \alpha_{0i}, & t < t_0, \\ \alpha_{1i}, & t \geq t_0. \end{cases}$$

For *control* apps, I assume $\rho_{i,t}$ and $\alpha_{i,t}$ remain unchanged across t_0.

Appendix A.3. 3. Difference-in-Differences (DiD) Setup

I use a DiD regression of the form:

$$\ln(R_{i,t}) \ = \ \beta \left(\mathrm{Treat}_i \times \mathrm{Post}_t\right) \ + \ \delta_t + \lambda_i + \varepsilon_{i,t},$$

where:

- $\mathrm{Treat}_i = 1$ if app i is subject to the minor-usage policy (a "treated" game); 0 if not.
- $\mathrm{Post}_t = 1$ for $t \geq t_0$ (post-policy); 0 for $t < t_0$.
- δ_t and λ_i are time and app fixed effects.

If control apps experience no policy-induced changes in ρ and α, then β *approximately* measures the *log-change* in $R_{i,t}$ for treated apps, pre vs. post t_0, relative to control apps.

Log-Change for Treated Apps

In the simplest sense,

$$\beta \;\approx\; \Big[\ln\big(R_{i,\text{post}}\big) - \ln\big(R_{i,\text{pre}}\big)\Big]_{\text{treated}}.$$

I now connect this back to changes in ρ and α.

Appendix A.4. 4. Revenue Change from Pre- to Post-Policy

Focus on a treated app i moving from (ρ_{0i}, α_{0i}) to (ρ_{1i}, α_{1i}). Ignoring small shifts in downloads $D_{i,t}$ or assuming they move similarly across treated and control,[6] the *key* difference is:

$$R_{i,\text{pre}} \;\approx\; \alpha_{0i}\,\rho_{0i}\,U_{i,t-1}, \qquad R_{i,\text{post}} \;\approx\; \alpha_{1i}\,\rho_{1i}\,U_{i,t-1}.$$

The same $U_{i,t-1}$ is used for a simple "snapshot" argument, i.e., I compare how much revenue a given user base yields pre- vs. post-policy, ignoring new inflows or outflows aside from retention.

Appendix A.5. 5. Taking Logs and Forming the DiD Coefficient

Hence,

$$\ln\big(R_{i,\text{pre}}\big) \;\approx\; \ln(\alpha_{0i}\,\rho_{0i}) \;+\; \ln\big(U_{i,t-1}\big),$$
$$\ln\big(R_{i,\text{post}}\big) \;\approx\; \ln(\alpha_{1i}\,\rho_{1i}) \;+\; \ln\big(U_{i,t-1}\big).$$

Subtracting,

$$\ln\big(R_{i,\text{post}}\big) \;-\; \ln\big(R_{i,\text{pre}}\big) \;\approx\; \Big[\ln(\alpha_{1i}) + \ln(\rho_{1i})\Big] \;-\; \Big[\ln(\alpha_{0i}) + \ln(\rho_{0i})\Big].$$

This simplifies to:

$$\ln\big(\tfrac{\alpha_{1i}}{\alpha_{0i}}\big) \;+\; \ln\big(\tfrac{\rho_{1i}}{\rho_{0i}}\big),$$

which is precisely the **change in log** of $\alpha_i\rho_i$.

[6] This assumption means I focus on how ρ and α shift, rather than on large changes in $D_{i,t}$. If $D_{i,t}$ also changes dramatically for the treated group, it can be incorporated as an additional channel.

Appendix A.6. 6. Interpreting β

In the DiD regression, if I assume no revenue shift for control apps, then β captures the *additional* log-change for treated apps. Consequently,

$$\beta \;\approx\; \ln\!\Bigl(\tfrac{\alpha_{1i}}{\alpha_{0i}}\Bigr) \;+\; \ln\!\Bigl(\tfrac{\rho_{1i}}{\rho_{0i}}\Bigr).$$

Equivalently,

$$\beta \;\approx\; \ln\!\Bigl(\alpha_{1i}\,\rho_{1i}\Bigr) \;-\; \ln\!\Bigl(\alpha_{0i}\,\rho_{0i}\Bigr).$$

- A *negative* β implies that $\alpha_{1i}\rho_{1i} < \alpha_{0i}\rho_{0i}$, suggesting either a drop in user retention (ρ) and/or monetization per user (α), or both.
- If minors were previously significant contributors to retention or spending, restricting them lowers ρ_{1i} (they leave) and possibly α_{1i} (those who remain pay less).

Appendix A.7. 7. Summary

Thus, under the single-group model, the **difference-in-differences coefficient** β naturally corresponds to the *combined log-change* of the retention rate (ρ_{1i}/ρ_{0i}) and monetization power (α_{1i}/α_{0i}). A significantly negative β indicates that the product $\alpha_{1i}\rho_{1i}$ is smaller post-policy, consistent with minors leaving, paying less, or both.

Appendix B. Detailed Derivation for the Approximate DiD Coefficient $\boldsymbol{\beta}$

This section provides a step-by-step derivation showing how a difference-in-differences (DiD) coefficient, β, can be approximated by the sum of (i) the log-change in minors' monetization (α'_m/α_m) and (ii) the log-change in minors' retention (ρ'_m/ρ_m), each weighted by the fraction of minors ω_m in the overall user base.

Appendix B.1. 1. Baseline Setup with Two User Groups

Consider an app with two user groups:

- *Minors*: Monetization α_m, retention ρ_m.
- *Adults*: Monetization α_a, retention ρ_a.

Let $U_{m,t}$ and $U_{a,t}$ be the active minors and adults at time t. Then the *total revenue* pre-policy is:

$$R_{\text{pre}}(t) \;=\; \alpha_m\,U_{m,t} \;+\; \alpha_a\,U_{a,t}.$$

Appendix B.2. 2. Policy-Induced Parameter Changes for Minors

At time t_0, a policy restricts minors, reducing:

$$\alpha_m \;\to\; \alpha'_m < \alpha_m, \quad \rho_m \;\to\; \rho'_m < \rho_m.$$

Adults remain unaffected. Hence, post-policy:

$$R_{\text{post}}(t) \;=\; \alpha'_m\, U_{m,t} \;+\; \alpha_a\, U_{a,t}.$$

In principle, $U_{m,t}$ also shrinks if minors drop out at a higher rate ($\rho'_m < \rho_m$).

Appendix B.3. 3. Difference-in-Differences Coefficient

In a DiD framework, a typical regression might be:

$$\ln\bigl(R_{i,t}\bigr) \;=\; \beta\,(\text{Treat}_i \times \text{Post}_t) \;+\; \delta_t + \lambda_i + \varepsilon_{i,t},$$

where Treat_i identifies apps subject to the policy (e.g., games), and $\text{Post}_t = 1$ after t_0. If the control group shows no revenue change, β approximates the *log-change* in revenue for treated apps:

$$\beta \;\approx\; \bigl[\ln(R_{\text{post}}) - \ln(R_{\text{pre}})\bigr]_{\text{treated}}.$$

Appendix B.4. 4. Fraction of Minors ω_m

Let

$$R_m \;=\; \alpha_m\, U_{m,t}, \quad R_a \;=\; \alpha_a\, U_{a,t}.$$

Then $R_{\text{pre}} = R_m + R_a$. Define

$$\omega_m \;=\; \frac{R_m}{R_m + R_a} \;=\; \frac{\alpha_m U_{m,t}}{\alpha_m U_{m,t} + \alpha_a U_{a,t}} \;\in\; [0,1].$$

Hence, $R_m = \omega_m\, R_{\text{pre}}$ and $R_a = (1-\omega_m)\, R_{\text{pre}}$.

Appendix B.5. 5. Log Ratio of Post- vs. Pre-Policy Revenue

$$\frac{R_{\text{post}}}{R_{\text{pre}}} \;=\; \frac{\alpha'_m\, U_{m,t} + \alpha_a\, U_{a,t}}{\alpha_m\, U_{m,t} + \alpha_a\, U_{a,t}}.$$

Using $R_m = \alpha_m\, U_{m,t} = \omega_m\, R_{\text{pre}}$ and $R_a = \alpha_a\, U_{a,t} = (1-\omega_m)\, R_{\text{pre}}$, define

$$X \;=\; \frac{\alpha'_m\, U_{m,t}}{\alpha_m\, U_{m,t}} \;=\; \frac{\alpha'_m}{\alpha_m} \times \frac{U_{m,t}}{U_{m,t}} \;\approx\; \frac{\alpha'_m}{\alpha_m}\,\frac{\rho'_m}{\rho_m},$$

where the ρ'_m/ρ_m factor accounts for a smaller active minor base post-policy.

Thus,

$$\frac{R_{\text{post}}}{R_{\text{pre}}} \;=\; (1-\omega_m) + \omega_m\, X, \quad \text{where } X = \tfrac{\alpha'_m}{\alpha_m}\,\tfrac{\rho'_m}{\rho_m}.$$

Taking logs and applying a *first-order approximation* around $X \approx 1$:

$$\ln\Bigl(R_{\text{post}}\Bigr) - \ln\Bigl(R_{\text{pre}}\Bigr) \;=\; \ln\bigl[(1-\omega_m) + \omega_m\, X\bigr] \;\approx\; \omega_m\, \ln(X).$$

Since $\ln(X) = \ln\bigl(\alpha'_m/\alpha_m\bigr) + \ln\bigl(\rho'_m/\rho_m\bigr)$, I have:

$$\beta \;\approx\; \omega_m\Bigl[\ln\Bigl(\tfrac{\alpha'_m}{\alpha_m}\Bigr) + \ln\Bigl(\tfrac{\rho'_m}{\rho_m}\Bigr)\Bigr] \quad\Longleftrightarrow\quad \beta \;\approx\; \ln\Bigl(\tfrac{\alpha'_m}{\alpha_m}\Bigr) \;+\; \omega_m\, \ln\Bigl(\tfrac{\rho'_m}{\rho_m}\Bigr).$$

Appendix B.6. 6. Interpretation

- ω_m = fraction of total revenue generated by minors pre-policy.
- $\alpha'_m/\alpha_m < 1$ captures lower spending or monetization by minors who remain.
- $\rho'_m/\rho_m < 1$ captures greater dropout of minor users.
- A large negative β arises if minors form a significant share ($\omega_m \approx 1$) and face big drops in ρ_m or α_m post-policy.
- If $\beta \approx 0$, it may suggest minors are a negligible share of total revenue or circumvent the policy.

Hence, I interpret β as the *combined impact* of minors' attrition and reduced spending, weighted by their baseline importance ω_m.

Appendix C. Data Source and Disclaimer

This study uses daily app-level download and revenue data from Qimai Technology (`https://www.qimai.cn`), a commercial mobile analytics platform that collects publicly available information from Apple's App Store and combines it with proprietary estimation methods. Qimai does not disclose details regarding its modeling or data partners, so the exact accuracy of the data cannot be independently verified. I rely on these estimates as they are widely used in industry for approximate market trends.

I note that Qimai retains intellectual property rights over its data and methodologies. The authors have received no sponsorship or endorsement from Qimai, nor do the findings in this paper represent Qimai's views. Furthermore, all errors or omissions are our own.

Use of Data Per Qimai's terms and standard academic practice, the authors publish only aggregated or summary-level analyses and do not disclose raw daily values for individual apps. Where necessary, apps are anonymized to preserve confidentiality. I encourage readers to interpret results in the light of potential estimation errors inherent in third-party analytics platforms.

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