Graphical Abstract

Restricting Video Games in China: Effects on Time Use, Educational Achievement, and Health

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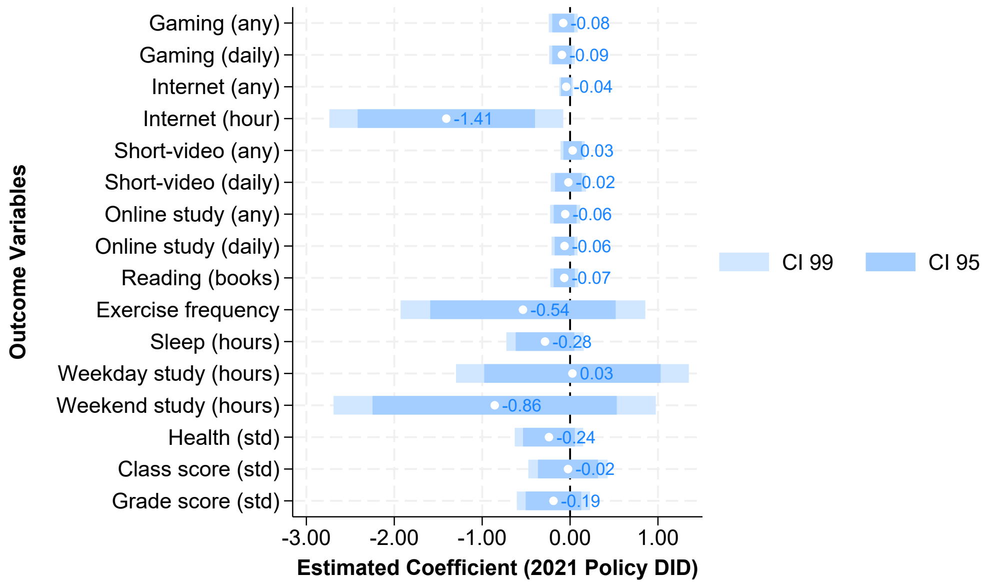

Highlights

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- The policy reduced minors' daily Internet use by 56 minutes but had no measurable effect on study effort, grades, or health
- Regression-kink evidence shows that relaxing the restriction after the age cutoff slightly lowers test scores in exam-track schools with high Internet access
- Behavioral regulation alone is insufficient without complementary supports that help adolescents reallocate time productively

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Abstract

In August 2021, the Chinese government implemented a nationwide restriction banning all weekday gaming for minors and limiting weekend access to a single hour (8:00–9:00 p.m.). Using nationally representative survey data and a difference-in-differences design, I find that the policy sharply reduced minors' daily gaming time and overall Internet use, reflecting high compliance. However, these behavioral changes did not translate into measurable improvements in academic performance, study effort, or health. Complementary evidence from city-level administrative data and a regression-kink design likewise shows no robust effects on exam outcomes, suggesting that while the 2021 ban effectively curtailed online activity, its intended second-stage benefits for learning were not realized.

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.[1] With this surge in Internet usage, especially in mobile gaming, debates have intensified, particularly in China, about the potential

[1] Between 2014 and 2021, China's video game industry expanded at an average annual rate of about 14.5%, surging from 114.48 billion RMB to 296.51 billion RMB right as the policy interventions were beginning to take shape (Statista, 2024).

impacts of gaming on users' health, productivity, and human capital accumulation. Concerns about gaming addiction, especially among minors, have prompted policymakers, educators, and parents to question whether restrictions on access to video games are necessary to mitigate possible harm. This study aims to quantify the impact of gaming restrictions on high school students and the counterpart effect of additional gaming hours on high school students.

In October 2019, the Chinese government introduced its first nationwide regulation capping minors' weekday gaming at 90 minutes—a measure explicitly aimed at curbing digital addiction among adolescents. This policy was further intensified in August 2021, limiting minors to a single hour of online gaming (8:00–9:00 p.m.) on weekends and banning it altogether on weekdays. Enforcement relies on real-name registration and automatic server cut-offs once a user hits the time cap, although potential loopholes (such as minors borrowing adult IDs) underscore ongoing practical challenges. By imposing a sharp age cutoff and strict gaming limits, these regulations create a quasi-experimental framework through which one can examine both immediate and broader implications of constraining digital usage among youth.

Empirically, I combine two complementary data sources and identification strategies. The main analysis uses nationally representative data from the China Family Panel Studies (CFPS) to estimate intent-to-treat effects of the 2021 restriction through a time-and-age difference-in-differences framework that compares minors and adults before and after the policy. To examine cumulative exposure at the policy threshold, I further exploit a city-level administrative dataset from Binzhou, Shandong Province, where students' exact birthdates allow a regression-kink design centered on the 18-year legal cutoff. Across both settings, I study outcomes including Internet use and gaming participation, other dimensions of time allocation (study, sleep, exercise), academic performance, and mental and physical health.

I find that the 2021 restriction sharply curtailed minors' online activity. Average daily Internet use fell by about 56 minutes (approximately 21 percent) and the probability of daily gaming dropped by 8–9 percentage points, indicating strong compliance. Yet these behavioral adjustments produced no measurable improvements in study effort, academic performance, or physical health, while self-reported well-being declined slightly. Complementary evidence from the Binzhou administrative dataset shows that relaxing the restriction just after the legal cutoff is associated with modest declines in test scores among final-year high-school students in high-Internet-coverage areas. Taken together, the results suggest that strict enforcement can effectively reduce screen time but alone cannot generate human-capital gains; behavioral regulation must be paired with supports that help adolescents reallocate time productively and manage the stress from lost digital leisure.

This study complements Barwick et al. (2024), who exploit random roommate assignment and shocks to mobile-app supply (e.g., the launch of *Genshin Impact* and minors' gaming limits) to examine how own and peer usage affect college students' outcomes. While their setting focuses on young adults, my analysis targets minors under a nationwide, age-based restriction, providing population-representative intent-to-treat effects. I extend their insights in three ways: (i) measuring broad reallocations of time and well-being in a younger cohort; (ii) documenting null educational and health effects despite large first-stage behavioral responses; and (iii) uncovering within-school peer spillovers around the legal cutoff that echo peer-contagion mechanisms in a more tightly regulated context.

More broadly, this work connects to evidence that social-media exposure harms mental health (Braghieri et al., 2022), that school-level device bans improve outcomes mainly for lower-performing students (Beland and Murphy, 2016; Patterson and Patterson, 2017), and that expanding computer or broadband access often fails to raise test scores (Cristia et al., 2017; Fairlie and Robinson, 2013; Malamud et al., 2019). Related research highlights that the effects of digital interventions are context dependent (Suziedelyte, 2021; Derksen et al., 2022; Berkhout et al., 2024; Cardim et al., 2023; Bahia et al., 2024), and experimental work on digital detox and self-control frictions (Allcott et al., 2022, 2020) underscores why behavioral restrictions alone rarely improve welfare without complementary supports.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background and the timeline of China's 2019 and 2021 gaming restrictions. Section 4 introduces the empirical strategies, beginning with a time-and-age DID framework using CFPS data and then turning to the RK design for the Binzhou exam setting. Section 3 details the datasets. Section 5 presents the national survey results, and Section 6 examines the city-level evidence. Section 7 concludes.

2. Institutional Background

In October 2019, the Chinese government introduced its initial regulation on the duration of online gaming for minors (under the age of 18 years).

The regulation imposed a maximum limit of 90 minutes on weekdays and 3 hours on weekends and holidays for minors' daily playtime. Before the implementation of this policy, there were no specific limitations on the amount of time minors could spend playing games. Subsequently, in August 2021, a more stringent regulation was introduced, prohibiting minors from online gaming on weekdays. Instead, they were only allowed to play for an hour between 8:00 PM and 9:00 PM on weekends and holidays. These policies are summarized in Table 1.

The gaming industry responded quickly by implementing software-based enforcement measures. Under these rules, each player must register with a certified ID (often a resident identity card) to confirm eligibility and age. Demo or guest accounts grant only limited game access, and once minors reach their prescribed time limit, they are automatically logged out and cannot log back in until the next allowable window. These rules were updated nationwide, with no staggered rollout by region or game application. Although firms required time to update servers, public information suggests they reacted swiftly. As a result, China's gaming restrictions became one of the strictest approaches worldwide to curb excessive gaming among minors. Nonetheless, some minors circumvent the policy by using their parents' or other adults' IDs. These regulations apply broadly to online games on smartphones, personal computers, and consoles but do not affect offline or older gaming systems.

The government's stated rationale for these regulations is that excessive online gaming has had a detrimental impact on minors' physical health and mental well-being, particularly among individuals who develop 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 of the document is summarized as "resolutely preventing minors from becoming addicted to online games and effectively protecting their physical and mental health."

Being the first universal ban enacted in a major economy,[2] China's policy

[2] In the United States, many schools prohibit mobile-phone use throughout the school day, and some faculty ban electronic devices in class. The Taliban forbids music and restricts other media. Many major economies employ age-rated movies, explicit-lyrics warnings, and age-based restrictions on adult content. More recently, the U.S. passed legislation banning TikTok, reflecting broader concerns about social-media usage.

quickly stirred debate on its legitimacy and potential impact. Estimates suggest it directly affected around 107 million young gamers. By 2019, according to data from China Internet Network Information Center (2020), 93.1% of minors in China had internet access, with 59.2% playing mobile games and 27.2% using PCs. Notably, 24.0% exceeded 90 minutes of mobile gaming on weekdays and 14.7% surpassed three hours on weekends, already going beyond the 2019 time limits. In 2020, 73.17% of 10-to-19-year-olds still played online games, according to the *White Paper on Protection of Minors in the Game Field* (IResearch Consulting Group, 2021). Once the 2021 policy took full effect, the share of minors playing less than three hours a week (or none) increased to 75.49% from 67.76%, while the proportion of heavy players (over three hours weekly) declined (CNG, 2022).

Recent national evidence provides a detailed picture of minors' online gaming patterns and how they evolved after the 2021 restriction. According to the 5th National Report on Internet Use among Minors in China (CNNIC), 97.2% of Chinese minors are internet users and 62.8% play mobile games, most commonly *action* or *role-playing* titles such as *Honor of Kings* and *Peacekeeper Elite*. These multiplayer, team-based games rely on real-time coordination with friends, reflecting gaming's strong social and interactive character. The 2021 policy not only limited total playtime to three hours per week but also confined access to a fixed one-hour window (8–9 p.m. on weekends and holidays), effectively synchronizing when minors could play together. Rather than dispersing participation, this schedule appears to have concentrated gaming activity into collective sessions, allowing players to coordinate more easily within the imposed time frame (China Youth and Children Research Center and Tencent Growth Guardian Platform, 2023; The Beijing News, 2024; China Briefing, 2024).

Survey and micro-level evidence suggest that minors largely continued to play the same mainstream multiplayer games after the policy rather than shifting to offline or non-digital substitutes. For example, Yang et al. (2023) document a substantial decline in weekly smartphone gaming time among rural adolescents but find no evidence of substitution into offline gaming or other leisure activities. Similarly, parental and student reports indicate that about one-third of minors used parental accounts to circumvent login restrictions, implying that most sought to maintain their previous game preferences under tighter time limits. Overall, the available evidence points to continuity in the types of games played and the social nature of play, with the main behavioral adjustment occurring through reduced total time rather than through changes in game genre or mode of play.

3. Data

Before outlining the empirical strategy, I describe the two key datasets that form the basis of the analysis.

3.1. National Survey Data: The China Family Panel Studies (CFPS)

The first data source is the China Family Panel Studies (CFPS) (Peking University, 2022), a nationally representative biennial survey launched in 2010 by the Institute of Social Science Survey (ISSS) of Peking University. The CFPS adopts a multi–stage probability sampling design, with an initial target of around 16,000 households, and interviews all core household members aged nine and above (Xie and Lu, 2015). It collects extensive socioeconomic, demographic, educational, and health information, and has become a key infrastructure for empirical social science research in China. In terms of scale, the CFPS has maintained tens of thousands of respondents per wave. The 2018 survey interviewed roughly 37,000 individuals, the 2020 wave about 28,500, and the 2022 wave around 27,000 individuals.[3]

Following the standard Chinese education schedule, students typically enter high school at ages 15–16 and graduate around 18–19. Table D.20 confirms that the majority of respondents in the CFPS fall within this range, with a small share of 19-year-olds still enrolled in the final year of high school. Accordingly, I restrict the analysis sample to individuals aged 16–19 to maintain comparability and exclude atypical late entrants or repeaters. Focusing on this age range also ensures that all respondents provide self-reported measures of time use and outcomes, as children younger than 16 are typically surveyed by proxy. The key subsample further restricts to general high school students within this range, offering a more comparable educational setting for analyzing detailed patterns in Internet and gaming behavior, study and leisure time, academic outcomes, and self-reported health—making the CFPS particularly well suited for the difference-in-differences framework used in the analysis.

[3]The CFPS sampling unit is the household. According to public reports, the 2022 CFPS covered approximately 22,585 households (Peking University Education Foundation (2022)), which correspond to roughly 27,000 interviewed individuals in the cleaned micro data (Peking University (2022)).

Although the CFPS is designed as a household and individual level panel, its biennial structure and my focus on a narrow age range (high school students aged between 16 and 19) make it infeasible to exploit panel properties. For example, a student who was in high school in the 2020 wave is very likely to have graduated by 2022. I therefore use the CFPS in repeated cross–section form, pooling individuals of the relevant ages across waves.

Table 2 reports descriptive statistics for high school students aged 16–19 in the 2018, 2020, and 2022 waves of the CFPS, distinguishing minors (<18) from adults (≥18). Panel A presents demographic characteristics (age, gender, and urban status); Panel B summarizes Internet use, including mobile and PC access as well as total usage minutes (note that several usage variables are unavailable in 2018 due to changes in the Internet module); Panel C describes gaming behaviors; Panel D covers time allocation (sleep, study, exercise, and TV/movies); Panel E reports educational outcomes (class and grade ranks); and Panel F shows self-assessed health. The 2018 wave extends the pre-policy horizon, whereas the 2020 and 2022 waves provide directly comparable measures of Internet and gaming activity surrounding the 2021 policy change.

For reference, Appendix Table B.10 reproduces the same statistics for the **full sample of respondents aged 16–19**, including those not currently enrolled in high school. While the appendix table offers broader population coverage, the high-school-only sample in the main text provides a cleaner and more comparable basis for analyzing policy effects on educationally active adolescents.

3.2. Citywide Administrative Data: Binzhou Mock College Entrance Exam

The CFPS provides nationally representative panel data with rich measures of minors' Internet and gaming behavior, making it well suited for analyzing behavioral responses to the 2021 policy. However, it is less ideal for studying short-run academic impacts for three reasons. First, it lacks standardized exam scores and records only coarse, self-reported academic rankings on a five-point scale, which are subjective and not comparable across schools or regions. Second, its subsample of high school–age respondents (ages 16–19) is small—fewer than one thousand per wave—reducing statistical power. Third, respondents' exact birthdates are not publicly released, preventing precise age-based designs that exploit the regulatory cutoff at age 18.

To address these limitations, I complement the CFPS with a citywide administrative dataset from Binzhou, Shandong Province—the Mock College Entrance Exam data (Binzhou City Education Bureau, 2022). This dataset contains detailed subject-level scores from a standardized exam administered under identical conditions to all senior (Grade 12) students across the city, providing objective measures of academic performance. It covers 19,203 students from 31 regular high schools across seven counties,[4] with complete information on exact birthdates and school identifiers, allowing precise age-based exposure measurement and regression kink analyses. The exam was held on January 17, 2022—five months after the policy took effect—providing a clean snapshot of academic performance under the new restrictions.

Binzhou's curriculum follows the national standard: students study three compulsory subjects (Chinese, Math, and English) and choose three electives from six options (Physics, Biology, Chemistry, Politics, History, and Geography). Children typically enter primary school at age six; if they progress without delay, they are between 17.4 and 18.4 years old at the time of the exam.[5] Absenteeism for this mock exam is only around 5%; the test is designed for practice and self-assessment rather than formal placement.[6] Summary statistics are reported in Table J.40.

While individual addresses are unavailable, Internet quality is measured using the county-level Internet Coverage Rate (ICR), defined as $ICR_c = (NIAH_c)/(POP_c)$, where $NIAH_c$ is the number of Internet account holders and POP_c is the total county population. Together, the CFPS and Binzhou datasets provide complementary identification strategies: the CFPS captures temporal variation in Internet and gaming behavior using a difference-in-differences framework, whereas the Binzhou administrative data exploit cross-sectional age-based variation through a regression kink design.

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[4] In China, a prefecture-level "city" typically includes an urban core plus multiple suburban or rural counties under its jurisdiction. Binzhou, for instance, encompasses seven such counties.

[5] Binzhou's education system includes six years of primary and three years of middle school, comprising nine years of compulsory education.

[6] The exam allows students to experience a simulated high-stakes setting and use the results for self-evaluation and goal planning.

4. Empirical Strategies

4.1. Time-and-Age DID Framework (CFPS)

I analyze a range of outcome variables that differ in scale and support. Binary and standardized outcomes include any Internet use, gaming participation, daily gaming, standardized physical health, and standardized academic performance measures such as class and grade ranks. Continuous and nonnegative outcomes capture the intensive margins of time use, including total Internet use and hours spent on specific activities such as sleep, study, exercise, watching TV, watching short videos, and online study. Accordingly, I use linear probability models (LPM) or ordinary least squares (OLS) for binary and standardized outcomes, and Poisson pseudo–maximum likelihood (PPML) for continuous time-use measures throughout the analysis. The PPML estimator naturally accommodates zero outcomes and heteroskedasticity, providing consistent estimates in the presence of skewed or non-normal time-use distributions.

Baseline specification. I begin with a baseline difference-in-differences (DID) design that exploits both temporal variation (pre- vs. post-policy) and age-based variation (minor vs. adult status). Formally, I estimate

$$y_{ipt} = \beta_1 \left(Minor_{ipt} \times Post_t\right) + \beta_2\, Minor_{ipt} + \beta_3\, Post_t + \boldsymbol{\gamma}\, \mathbf{X}_{ipt} + \delta_p + \epsilon_{ipt}.$$

where y_{ipt} denotes the outcome for individual i in province p and survey wave t. The coefficient of interest, β_1, captures the differential post-policy change in the outcome for minors relative to adults, while β_2 measures the average pre-policy difference between minors and adults.[7] Province fixed effects λ_p absorb time-invariant regional heterogeneity, and $Post_t$ is an indicator for the 2022 (post-policy) wave. Because the DID design uses only two comparable survey waves (2020 and 2022), $Post_t$ is perfectly collinear with year fixed effects. To display the common post-policy shift explicitly, I therefore keep $Post_t$ in the baseline specification and omit separate year dummies. In robustness checks, I instead include province-by-year fixed effects, in which case $Post_t$ is dropped and the estimated effects remain virtually unchanged.

where y_{ipt} denotes the outcome for individual i in province p and survey wave t. The coefficient of interest, β_1, captures the differential post-policy

[7]Note that $Minor_{ipt}$ is time-varying because the same respondent may switch from minor to adult status across waves.

change in the outcome for minors relative to adults, while β_2 measures the average pre-policy difference between minors and adults.[8] Province fixed effects λ_p absorb time-invariant regional heterogeneity, and $Post_t$ is an indicator for the 2022 (post-policy) wave. Because the DID design uses only two comparable survey waves (2020 and 2022), $Post_t$ is perfectly collinear with year fixed effects. To display the common post-policy shift explicitly, I therefore keep $Post_t$ in the baseline specification and omit separate year dummies. In robustness checks, I instead include province-by-year fixed effects, in which case $Post_t$ is dropped and the estimated effects remain virtually unchanged.

For continuous and nonnegative outcomes, I estimate the corresponding DID specification using Poisson pseudo–maximum likelihood (PPML):

$$\begin{aligned} E[y_{ipt} \mid X_{ipt}] = \exp\Big(&\beta_1\big(Minor_{ipt} \times Post_t\big) + \beta_2\, Minor_{ipt} + \beta_3\, Post_t \\ &+ \boldsymbol{\gamma}'\mathbf{X}_{ipt} + \lambda_p\Big), \end{aligned} \tag{1}$$

where the coefficients β_1, β_2, and β_3 have analogous interpretations as in the linear model. Province fixed effects (λ_p) absorb time-invariant regional heterogeneity, and standard errors are clustered at the county level.

The treatment varies along the temporal and age dimensions: minors are affected by the policy only after its implementation. Year fixed effects are essential for identification, capturing the common post-policy shift across all individuals. Province fixed effects, by contrast, are included primarily for precision—absorbing time-invariant regional heterogeneity—but they are not necessary for identification. Because the main analysis uses two survey waves, the year indicators are perfectly collinear with the post-policy indicator. To present the post effect explicitly, I therefore report baseline specifications with province fixed effects and the post indicator, rather than separate year fixed effects. As a robustness check, I also estimate models with province-by-year fixed effects, which identify effects purely from within-province-year contrasts across ages. Such higher-order fixed effects are valuable insofar as they can capture differential local shocks to education or health around the policy dates. In a context of rapidly evolving digital infrastructure and pandemic disruptions, they help ensure that the estimated policy effects are not conflated with concurrent regional trends in human capital or health

[8]Note that $Minor_{ipt}$ is time-varying because the same respondent may switch from minor to adult status across waves.

services. The point estimates are virtually unchanged across the two specifications, indicating that province-year shocks are not driving the results. This approach is consistent with recent methodological discussions emphasizing that difference-in-differences estimators should not mechanically add unit-specific trends or excessive fixed-effect interactions as a default "fix," but instead rely on transparent baselines with targeted robustness checks (Kahn-Lang and Lang, 2020; Goodman-Bacon, 2021; Olden and Møen, 2022; Rambachan and Roth, 2023a).

Triple-difference specification. To account for potential heterogeneity by baseline Internet access, I extend the model to a triple-difference (DDD) specification:

$$\begin{aligned} y_{ict} = \theta_1 \left(Minor_{ict} \times Post_t \times ICR_c\right) \\ + \theta_2 \left(Minor_{ict} \times Post_t\right) + \theta_3 \left(Minor_{ict} \times ICR_c\right) + \theta_4 \left(Post_t \times ICR_c\right) \\ + \theta_5 \, Minor_{ict} + \theta_6 \, Post_t + \theta_7 \, ICR_c \\ + \boldsymbol{\gamma} \, \mathbf{X}_{ict} + \lambda_{p(c)} + \varepsilon_{ict}. \end{aligned} \tag{2}$$

where subscripts i, c, and t denote individual, county, and year, respectively. The baseline Internet coverage ratio ICR_c is measured at the county level in 2020. Province fixed effects $\lambda_{p(c)}$ absorb broader time-invariant regional heterogeneity, so treatment heterogeneity is identified relative to baseline county-level Internet penetration rather than province-wide or national shocks. For interpretability, I standardize ICR_c to have mean zero and unit variance, which facilitates coefficient comparison across specifications.

For continuous and nonnegative outcomes, I estimate the corresponding triple-difference specification using Poisson pseudo–maximum likelihood (PPML):

$$\begin{aligned} E[y_{ict} \mid X_{ict}] = \exp\Big(\theta_1\big(Minor_{ict} \times Post_t \times ICR_c\big) \\ + \theta_2\big(Minor_{ict} \times Post_t\big) + \theta_3\big(Minor_{ict} \times ICR_c\big) + \theta_4\big(Post_t \times ICR_c\big) \\ + \theta_5 Minor_{ict} + \theta_6 Post_t + \theta_7 ICR_c \\ + \boldsymbol{\gamma}'\mathbf{X}_{ict} + \lambda_{p(c)}\Big), \end{aligned} \tag{3}$$

where the coefficients θ_j have analogous interpretations as in the linear DDD model. Subscripts i, c, and t denote individual, county, and year, respectively.

The baseline Internet coverage ratio ICR_c is measured at the county level in 2020, while province fixed effects $\lambda_{p(c)}$ absorb broader time-invariant regional heterogeneity. Standard errors are clustered at the county level.

As a complementary check, I also implement a minors-only version of this design, which is logically equivalent to the triple-difference specification but focuses exclusively on within-minor variation (see Section A.4.4 in the Appendix).

Dynamic specification. Where longer CFPS histories are available (2012–2022), I estimate the following event-study specifications. For binary or standardized outcomes, I estimate a linear probability model (LPM):

$$\begin{aligned} Y_{ipt} = \alpha + \delta \,\text{Minor}_{ipt} + \sum_{t \neq 2020} \beta_t \big(\text{Minor}_{ipt} \times \mathbb{1}\{\text{Year} = t\}\big) \\ + X'_{ipt}\gamma + \lambda_p + \lambda_t + \varepsilon_{ipt}, \end{aligned} \tag{4}$$

where Y_{ipt} is a binary or standardized outcome for individual i in province p and year t. The coefficient δ captures baseline differences between minors and adults in 2020, while β_t measures the relative change for minors in each subsequent survey year. X_{ipt} denotes the same set of demographic controls (gender, urban residence, and parental education). Province and year fixed effects, λ_p and λ_t, are included, and standard errors are clustered at the county level.

For continuous and nonnegative time-use variables, I employ a Poisson pseudo–maximum likelihood (PPML) estimator:

$$\begin{aligned} E[Y_{ipt} \mid X_{ipt}] = \exp\Big(\alpha + \delta \,\text{Minor}_{ipt} + \sum_{t \neq 2020} \beta_t \big(\text{Minor}_{ipt} \times \mathbb{1}\{\text{Year} = t\}\big) \\ + X'_{ipt}\gamma + \lambda_p + \lambda_t\Big), \end{aligned} \tag{5}$$

where Y_{ipt} denotes total minutes of Internet use or other continuous measures of time allocation. Minor_{ipt} is an indicator for individuals under age 18, and $\mathbb{1}\{\text{Year} = t\}$ are year dummies (with 2020 as the base year). The coefficients β_t trace dynamic differences in intensity for minors relative to adults over time, conditional on demographic controls and province and year fixed effects.

4.2. Regression Kink (RK) Design for Cumulative Exposure

While the CFPS analysis uses a time-and-age DID to study time use and self-reported outcomes, the citywide exam dataset—combining exact birth dates with a common exam date (January 17, 2022)—is ideal for studying *cumulative* exposure via a regression–kink (RK) design. Because the 2021 regulation imposes a *daily* gaming cap, effects on study time (and thus exam performance) should accumulate over the fall semester rather than appear as an abrupt level shift at age 18. Over the 139-day window from 2021-09-01 to 2022-01-17, a student's date of birth deterministically maps to the number of days under the cap: those who remain minors for the entire window are fully restricted; those who turn 18 within the window are partially restricted; and those already 18 for most of the window are essentially unrestricted. This piecewise exposure mapping implies slope changes with respect to age at the legal threshold(s), motivating an RK rather than an RD focus.[9]

To clarify how individual exposure to the 2021 gaming restriction is computed, I calculate each student's expected gaming hours based on the number of days subject to the old (1.23 hours/day) versus new (0.43 hours/day) policy limits over the 139-day window from September 2021 to January 2022. Appendix Table J.41 provides illustrative examples for students with different birthdates, showing how small daily differences in allowed playtime accumulate over the semester.[10]

Naturally, I adopt a piecewise regression framework centered on two kink points at age 18 and age 18.3808. These cutoffs divide the sample into three distinct groups based on the level of policy exposure during the fall semester. First, individuals aged between $[17, 17.9973]$ at the time of the exam remain fully subject to the new, more restrictive 2021 policy throughout

[9] Appendix A.8 reports complementary CFPS evidence for senior high school students, showing that even this senior cohort experienced measurable declines in gaming participation under the 2021 policy.

[10] According to China Internet Network Information Center (2020), under the 2019 restriction policy, a typical Chinese high school student spent around 0.87 hours gaming on weekdays and 1.70 hours on weekends—a pattern that remained mostly stable until the updated 2021 policy took effect in September 2021. Over the roughly five-month period leading up to the January 17, 2022 exam date, the new policy prohibited gaming on weekdays and allowed only one hour (from 8:00 p.m. to 9:00 p.m.) on weekends. Consequently, students' average daily gaming time fell from approximately 1.23 hours per day (calculated as $(1.70 \times 3 + 0.87 \times 4)/7$) to 0.43 hours per day ($(1 \times 3 + 0 \times 4)/7$). This reduction could materially affect overall academic performance. In Section 5, I find daily Internet usage decreased by about 77.4 minutes, broadly aligning with these estimates. Some discrepancy is expected due to (1) different measures between Internet usage and gaming hours, (2) varying compliance with the policy, and (3) using a representative-student calculation instead of the full usage distribution.

the semester. Second, those in the $[18, 18.3808]$ interval experience a partially restricted semester: they spend some initial portion of the term under the new policy before turning 18 and then transition to less restrictive conditions once they cross the threshold. Finally, individuals older than 18.3808 at the exam date enjoy effectively no restriction for most or all of the semester. By separating the sample in this manner, I can capture how partial or complete exposure to the policy translates into different trajectories in exam outcomes.

The specification for the piecewise regression model is as follows:

$$Y_i = \beta_0 + \beta_1\, \mathit{seg1}_i + \beta_2\, \mathit{seg2}_i + \beta_3\, \mathit{seg3}_i + \gamma_1\, \mathit{jump18}_i + \gamma_2\, \mathit{jump18.38}_i + \epsilon_i \quad (6)$$

where Y_i is the standardized major subject score for individual i. The variable $\text{seg1}_i = \min(\text{age}_i, 18) - 17$ defines the portion of age in the $[17, 18)$ range, so its coefficient β_1 is the slope over that interval. Similarly, $\text{seg2}_i = \max(0, \min(\text{age}_i - 18, 0.38))$ identifies the additional age segment $[18, 18.38)$, and $\text{seg3}_i = \max(0, \text{age}_i - 18.38)$ identifies $[18.38, 19]$, with β_2 and β_3 capturing the respective slopes in those ranges. The indicator $\text{jump18}_i = 1$ if $\text{age}_i \geq 18$ (0 otherwise) flags an instantaneous jump at age 18, and $\text{jump18.38}_i = 1$ if $\text{age}_i \geq 18.38$ (0 otherwise) flags an instantaneous jump at age 18.38. The coefficients γ_1 and γ_2 thus measure these discrete shifts, whereas β_1, β_2, and β_3 reflect the continuous, segment-specific slopes. This piecewise construction follows a regression-kink framework by separating ongoing (cumulative) effects from immediate jumps. In the subsequent analysis, I focus primarily on the slope coefficients, which capture day-to-day changes in gaming exposure once individuals pass the 18-year cutoff.

To analyze these effects, I employ a Regression Kink (RK) design that exploits changes in the slope of the outcome–age relationship at the policy threshold. The RK framework captures the marginal treatment effects near the cutoff, allowing me to assess how policy incentives alter behavioral responses at the intensive margin. This approach provides a consistent and credible strategy to estimate the local causal effects of the policy.

The Mock Exam dataset covers the full population of high school seniors in a prefecture-level city, with verified birth dates from official identity records. A McCrary-type density test (Cattaneo et al., 2020) detects no evidence of manipulation around the 18-year-old cutoff, supporting the validity of the Regression Kink design (see Appendix A.10.3 for details and Figure J.13).

As discussed in Calonico et al. (2014b), estimating kink treatment effects is conceptually equivalent to estimating regression discontinuity (RD) effects on the *derivatives* of the outcome function. In other words, kink RD (RK) designs seek to identify shifts in the first derivatives of regression functions (or their ratios) rather than level jumps at a cutoff. In this study, the fundamental components of the RD/RK design are the running variable (age), the cutoff (18 years old), and the treatment (legal freedom to enjoy online gaming):

$$y_{is} = f\left(age_i\right) + \zeta\,\mathbf{X}_i + \eta_s + \epsilon_{is}, \tag{7}$$

In this specification, y_{is} represents the exam score for individual i attending school s. The function $f(\text{age}_i)$ is a piecewise mapping of the student's age (derived from birth and exam dates), while $\mathbf{X}_i$ may include controls such as gender and birthplace. Although both $\mathbf{X}_i$ and a possible school fixed effect η_s appear in the equation for completeness, the baseline specification omits them; I later explore specifications incorporating additional controls and school fixed effects in the online appendix to ensure robustness.

Regression Kink (RK) design is more appropriate than RD for this policy context. Because the policy imposes a *daily* gaming cap rather than a one-time cutoff, its effects on students' study time—and hence exam performance—are likely to accumulate gradually rather than manifest as an abrupt shift at age 18. In other words, the treatment is essentially "binomial" at the daily level but not at the cumulative level: once a student turns 18, each additional day of unrestricted gaming can incrementally reduce study hours, suggesting a *change in slope* in exam performance over time. By contrast, a standard RD design detects only an *immediate* discontinuity, which risks overlooking these cumulative dynamics across the semester. Although I also present RD results in Appendix A.9.1, the RK estimates remain my primary focus for capturing this incremental mechanism.

To implement local polynomial methods for Regression Discontinuity (RD) and Regression Kink (RK) settings, three key ingredients are required: a chosen kernel function $K(\cdot)$, a polynomial order p, and a bandwidth h. Discussed in Cattaneo et al. (2024a), the triangular kernel is a common choice in these settings. It applies a linear decrease in weights from the central point c out to the boundary $[\,c-h,\,c+h\,]$, where the weight hits zero. Paired with an MSE-optimal bandwidth, this kernel tends to yield desirable finite-sample performance. For the choice of K, a uniform kernel is also a common practice and produces results similar to the triangular kernel (Card et al. (2015)). Choosing the polynomial order p involves balancing approximation accuracy and variability. Higher orders improve fit but increase variance and risk overfitting near boundaries. Calonico et al. (2014b) recommend employing a local-quadratic estimator ($p = 2$) for RK designs to address boundary bias and a local-linear estimator ($p = 1$) for RD designs. However, the choice of p is not universally preferred, as the optimal choice in the mean squared error sense depends on the sample size and the derivatives of the conditional expectation function (Card et al. (2015)). The bandwidth h is critical, as it defines the interval used for the polynomial fit, directly influencing the estimator's reliability. Given that RD/RK results are sensitive to h, data-driven methods are essential for selecting bandwidth to avoid arbitrary choices and ensure robust and replicable results. In this paper, I follow the approaches outlined in (Calonico et al., 2014b,a, 2017; Cattaneo and Titiunik, 2022).

Due to data limitations, an RD-Diff-in-Diff methodology cannot be applied to leverage variation before and after policy implementation. Moreover, I cannot observe the exact duration of online gaming for this semester, which effectively reduces the fuzzy RD framework to a reduced-form design, capturing only an intention-to-treat effect. I will formally investigate the first-stage compliance problem in a subsequent paper using a different sample and methodology, but that analysis is not directly applicable in the present context.

The canonical Regression Discontinuity (RD) design is based on the assumption that the running variable determining treatment assignment is continuous. However, in this study, the running variable, age, is inherently discrete, as it is derived from the date of birth, resulting in clustering at specific mass points (Cattaneo et al., 2024a). Local polynomial estimators regard each distinct mass point as a single grouped observation. Under plausible assumptions, they remain suitable, provided that there is a sufficiently large and dense set of mass points near the threshold. For this dataset, continuity-based RD analysis is feasible, with 995 mass points in the full sample and 685 in the BC subsample, sufficient to approximate a continuous running variable.[11] However, careful consideration is necessary when selecting the bandwidth, as overly narrow bandwidths may result in too few mass points

[11] In the BC county subsample, the age range $[17.38, 18.38]$ contains 359 unique observations, which is modestly sufficient for robust analysis.

for precise estimation, while overly wide bandwidths risk introducing bias from extrapolation. [12]

The use of Regression Kink (RK) design for this analysis alleviates concerns about the discrete nature of the running variable, as RK design focuses on estimating changes in the slope of the outcome variable at the kink point rather than a sharp discontinuity. This makes RK less sensitive to the exact density of observations at the kink. With 995 mass points in the full sample and 685 in the BC subsample (or 359 in the narrower age range of $[17.38, 18.38]$), the dataset provides sufficient resolution to estimate slope changes robustly. Unlike continuity-based Regression Discontinuity (RD), RK relies on smooth transitions around the kink and is inherently aligned with the gradual nature of the policy under study.

Consequently, RK is well-suited to capture the marginal effects of the policy without being significantly impacted by the presence of mass points. In the subsequent section, I present the empirical results based on this approach, discussing both the full-sample and Bincheng subsample analyses in detail.

5. Policy Impact of the 2021 Gaming Restriction

5.1. Effects on Internet Use and Gaming Behavior

Table 3 reports the estimated effects of the 2021 gaming restriction on minors' internet and gaming behavior. The four columns correspond to progressively refined specifications that vary by sample definition and fixed effects. Columns (1)–(2) use the full sample of individuals aged 16–19, whereas Columns (3)–(4) emphasize high school students as the focal group of analysis. The policy applied to all minors under 18, but restricting attention to this cohort ensures a more comparable setting and facilitates linkage to downstream educational outcomes. All models include demographic controls (gender, hukou, and both parents' education) and cluster standard errors at the county level. Within each sample, Columns (1) and (3) include province fixed effects, while Columns (2) and (4) further add province-by-year fixed effects to absorb province-specific time shocks such as local changes in education policy, broadband expansion, or pandemic-related school closures.

[12] A local randomization approach can serve as a robustness check by examining only those observations within a tight band around the cutoff—treating them as if they were randomly assigned—thereby sidestepping the usual continuity requirement.

Panel A shows that the policy did not materially alter the extensive margin of internet access. Across all specifications, the coefficients on Minor $\times$ Post are close to zero and statistically insignificant (ranging from -0.006 to -0.042), indicating that almost all minors remained online after the policy. Given the high pre-policy mean of 0.93, extensive-margin adjustments were naturally limited.

Panel B documents a sizable and robust reduction in total daily internet use—the intensive margin. Across the four specifications, the estimated decline ranges from 42 to 56 minutes per day, all significant at the 1% level. The preferred specification in Column (3), which focuses on high school students with province fixed effects, implies a reduction of roughly 56 minutes, or about 21% of the pre-policy average (259 minutes). The similarity of results across samples and fixed-effect structures suggests that the estimated effects are not driven by regional composition or omitted shocks. These magnitudes are broadly consistent with the observed compliance to the official gaming curfew, as internet time—a broader usage measure—declined even where the restriction targeted only game play.

Panels C and D examine gaming participation. The coefficients on Minor $\times$ Post are uniformly negative, showing that minors' gaming activity fell after the policy, but the magnitude and precision vary by sample. For any gaming (Panel C), the high-school subsample yields significant declines of 10–11 percentage points from a pre-policy mean of 0.58 ($p < 0.05$), while estimates for the full sample remain small and insignificant. For daily gaming (Panel D), effects range from -0.04 to -0.09 and are significant at the 10% level in the high-school sample, corresponding to reductions of 8–9 percentage points from a mean of 0.24. These patterns indicate that the policy's behavioral impact concentrated among in-school minors and along the intensive margin of frequent gaming.

Taken together, the results indicate that the 2021 restriction led to a clear contraction in minors' online activity, driven primarily by reductions in total internet time and daily gaming intensity rather than by large changes in access itself. The strong and consistent intensive-margin effects suggest substantial behavioral adjustment and compliance with the policy, whereas the moderate and less precise effects on gaming participation likely reflect substitution toward non-gaming internet uses or uneven enforcement across platforms. Overall, these findings point to broad policy efficacy in curbing digital exposure among minors, with the largest effects appearing among high school students who faced the most direct institutional and parental oversight.

Table 4 reports triple-difference (DDD) estimates that interact the treatment effect with counties' Internet Coverage Rate (ICR), allowing the policy's impact to vary with local internet infrastructure.[13] Columns (1)–(2) use the full sample of individuals aged 16–19, while Columns (3)–(4) focus on high school students. All regressions include the same demographic controls as in Table 3, with province or province-by-year fixed effects to absorb regional time shocks such as changes in provincial education policy, broadband expansion, or pandemic-related school closures.

Across outcomes, the estimated DDD coefficients (Minor × Post × ICR) are small and statistically indistinguishable from zero.[14] For instance, in Panel B (total daily internet minutes), the coefficients range from 1.0 to −5.4 (s.e. 17–18), indicating that a one–standard-deviation increase in county ICR corresponds to an additional change of only about 5–6 minutes in minors' daily internet use after the policy—economically trivial and statistically insignificant. In Panel A, the modest negative estimates (around −0.02) for any internet usage are only marginally significant at the 10% level, while all other outcomes show no systematic interaction with ICR. By contrast, the main DID terms (Minor × Post) remain large and negative, closely matching those in Table 3: minors' total internet time fell by roughly 45–55 minutes and their gaming activity by 10–12 percentage points, regardless of local connectivity.

Substantively, these findings imply that the 2021 gaming restriction reduced minors' online engagement broadly across counties, with little heterogeneity by pre-existing internet access. The absence of significant moderation by ICR suggests that the policy's enforcement and behavioral response were largely uniform nationwide rather than concentrated in digitally advanced areas. This pattern is consistent with a centrally administered and uniformly enforced regulation, where local differences in broadband coverage or user penetration did not materially condition the magnitude of behavioral change. Complementary micro-survey evidence from Yang et al. (2023) reinforces this interpretation: even among rural adolescents—who typically

[13]ICR is the share of registered Internet users in the county population, standardized to a z-score based on the 2020 distribution. Because ICR data are unavailable for a subset of counties, the DDD estimation sample is smaller than in the baseline DID models.

[14]Related robustness checks appear in Tables D.26 and D.25, which report specifications restricted to minors only and a placebo test among adults, respectively.

face weaker parental supervision and lower digital connectivity—the authors report a significant decline in weekly smartphone gaming time following the 2019 and 2021 restrictions. Together, these results indicate that the policy's behavioral impact was widespread and not limited to urban or high-ICR regions.

Appendix Table E.27 examines gender heterogeneity among academic high school students. Both female and male students experienced comparable absolute reductions in Internet use, though the decline is proportionally larger among females due to their lower pre-policy baseline. Gaming-related responses, however, remain statistically indistinguishable across genders.

Beyond gender differences, additional subsample checks highlight heterogeneity by household structure. Appendix Table E.28 shows that the policy's behavioral effects were substantially stronger among students from smaller households (four or fewer members). For this group, total daily Internet use declined by about 89 minutes and the PPML estimate implies an average reduction of roughly 31%, while effects on gaming frequency are also larger in magnitude. In contrast, for students from larger families (five or more members), the estimated coefficients are smaller and statistically insignificant across all outcomes. These findings suggest that family size conditions the strength of the restriction's impact—perhaps because parental monitoring or shared-device constraints differ by household structure—and highlight household context as an important moderating factor of policy effectiveness. The precise mechanism remains uncertain: smaller households may face tighter parental supervision or have fewer opportunities to borrow adult accounts from siblings or extended family members, but other unobserved social or economic factors could also play a role.

Appendix Table E.29 examines heterogeneity by baseline family income using a triple-difference specification that interacts the treatment effect with standardized 2020 income. The interaction terms ($\text{Minor} \times \text{Post} \times \text{Income}^{std}_{2020}$) are small and statistically insignificant across all outcomes, indicating that the 2021 restriction affected students from higher- and lower-income families similarly. In Panel B, for example, a one–standard-deviation increase in income corresponds to an additional change of only 3–5 minutes in daily Internet use after the policy—trivial compared to the average decline of about 78 minutes. No systematic heterogeneity appears for gaming participation, and the signs of the income interactions are inconsistent across specifications. These results suggest that enforcement and compliance were broadly uniform across the income distribution, reinforcing that differences in household context—rather than economic resources—mainly shaped the magnitude of behavioral adjustment.

5.2. Beyond Internet Use: Broader Effects on Behavior, Education, and Health

When focusing on the 2021 nationwide policy—which imposed a near-complete weekday gaming ban for minors— Table 5 examines a broad set of downstream behavioral, educational, and health outcomes among academic high school students. Despite the substantial contraction in online activity documented in earlier sections, there is little evidence of compensatory or spillover adjustments in other domains.

Panel A shows that the interaction effects on short-video use and online study are small and statistically indistinguishable from zero. Both the extensive (any use) and intensive (daily use) margins are close to zero, indicating that students did not reallocate their digital time from gaming to other online activities such as short-video browsing or study-related use.

Panel B reports results for daily schedules and lifestyle indicators. The coefficients for study time and exercise frequency are small and imprecise, while the coefficient for sleep duration is marginally negative (-0.045, $p < 0.05$), corresponding to a reduction of about 0.05 hours, or roughly three minutes per day—economically trivial. Thus, there is no meaningful evidence that the gaming ban altered students' overall time allocation or physical activity patterns.

Panel C turns to reading, self-reported health, and academic outcomes. The point estimates for reading frequency and test scores are close to zero and not statistically significant, and the negative coefficients on the standardized health index (-0.24 sd) and grade score (-0.17 sd) are modest in magnitude—less than one-quarter of a within-sample standard deviation—and statistically insignificant.

Overall, none of the estimated second-stage effects survive multiple-testing adjustment: as shown in Table C.18, all Romano–Wolf stepdown and Holm-Bonferroni adjusted p-values exceed conventional significance thresholds, and no null hypotheses are rejected at the 5% level.

Taken together, these results suggest that even among high school students—who face strong parental supervision and heavy study pressure—the 2021 gaming restriction did not produce detectable improvements in study time, health, or learning performance. The policy effectively curtailed online leisure but yielded no offsetting gains in other aspects of adolescent well-being. This pattern underscores that restricting gaming alone, without complementary measures addressing study habits, mental health, or broader digital engagement, is unlikely to translate into measurable educational or health benefits.

5.3. Validation of Identification Assumptions

5.3.1. Event-Study Evidence and Parallel-Trend Validation

Figure 1 presents event–study estimates that trace the evolution of internet use among high–school students before and after the 2021 restriction. Panel (a) shows the extensive margin (any internet use), and Panel (b) the intensive margin (total internet minutes). The coefficients for the pre–policy years are close to zero, suggesting that minors and adults followed similar trends prior to the restriction. After the policy, minors' total internet time fell sharply in 2022, by roughly one–quarter relative to adults, whereas the likelihood of having any internet access changed little. Appendix Figure C.6 replicates the analysis for all individuals aged 16–19 and shows equally flat pre–trends and a similar drop in total minutes. Together, these patterns support the parallel–trend assumption underlying the DID design and indicate that the observed declines reflect genuine behavioral responses to the gaming restriction rather than continuations of earlier trends.

Panel (a) of Figure 2 traces the evolution of minors' self-reported health relative to adults. Health levels remained broadly stable through 2018, followed by a noticeable improvement in 2020—likely reflecting a temporary rebound in perceived well-being after the initial COVID-19 lockdowns. Although the 2022 coefficient suggests a modest decline from that peak, minors' average health continued to exceed pre-2018 levels, indicating a net improvement over the policy period. These dynamics should be interpreted with caution, as the 2020 CFPS wave coincided with school closures and reduced physical activity that may have temporarily distorted health reporting. Viewed over the full 2012–2022 window, minors' health converged toward adult levels, suggesting that the gaming restriction did not harm—and may have modestly improved—adolescents' perceived well-being once pandemic effects are taken into account.

Panel (b) of Figure 2 reports the evolution of standardized class-level scores. Pre-policy estimates show a flat trend, confirming that minors and adults followed similar trajectories before 2021. The post-policy coefficient for 2022 is slightly negative (around –0.05 SD) but statistically indistinguishable from zero, indicating no measurable change in academic performance.

This lack of effect aligns with the second-stage regression results, where both class and grade scores show precisely estimated zeros. The findings suggest that while the 2021 restriction effectively curtailed gaming and internet time, it did not translate into detectable gains—or losses—in academic outcomes during the short run.

Appendix Figures C.7–C.9 provide further validation of the parallel-trend assumption by extending the event–study analysis to additional behavioral outcomes—including study hours, sleep, exercise, reading, and leisure activities. All panels display flat and statistically insignificant pre-policy coefficients, confirming that minors and adults followed parallel trajectories prior to 2021. Post-policy estimates reveal small and heterogeneous adjustments—such as a slight reduction in sleep duration and moderate increases in study hours or exercise frequency—but none are statistically robust. Taken together, these appendix figures reinforce that the 2021 restriction primarily affected online activity, generating a sharp contraction in Internet use without inducing systematic shifts in other aspects of adolescents' daily routines, leisure, or educational behaviors.

To assess the credibility of the parallel–trends assumption, I apply the HonestDiD framework of Rambachan and Roth (2023b) to the Internet-time estimates. The results show that the estimated reduction in Internet use is economically large and remains statistically robust under modest relaxations of the parallel–trends assumption. Only when allowing for substantial deviations from parallel trends do the confidence intervals begin to include zero. Appendix Figure C.10 reports the full sensitivity analysis.

While these event–study estimates provide direct visual support for the parallel–trend assumption, their interpretation should be considered in light of the underlying data structure and external shocks discussed in Section 5.3.2. In particular, the limited pre-2020 comparability of Internet-use measures and the pandemic context of the 2020 and 2022 waves warrant caution when interpreting long-run dynamics. The next subsection elaborates on these data and contextual considerations in greater detail.

5.3.2. Data Consistency, Common Trends, and COVID-19 Effects

Building on the above evidence, this subsection clarifies the consistency of the Internet-use measures, the plausibility of common trends, and the influence of the COVID-19 pandemic on observed dynamics. These considerations help contextualize the event–study estimates and delineate the limits of their interpretation.

While merging earlier CFPS waves (2012–2018) could in principle extend the pre-policy window, differences in how Internet use was measured before 2019 limit their usefulness for establishing parallel trends. Before 2020, the CFPS reported only weekly hours of leisure Internet use, whereas the 2020 and 2022 waves introduced daily-minute measures and disaggregated between mobile and PC access.[15] Furthermore, the pre-2020 surveys contained no questions on online gaming or short-video platforms, making it impossible to construct consistent pre-policy dynamics for these outcomes. Combined with the survey's biennial schedule, these differences constrain the granularity required to identify reliable pre-policy trends.

Despite these measurement limitations, several features of the institutional setting support the plausibility of parallel trends. First, the 2021 gaming restriction was implemented nationwide at a uniform time and was not triggered by prior shifts in students' gaming or Internet habits, reducing the likelihood of endogenous treatment timing. Second, descriptive evidence from earlier CFPS waves shows that minors and adults followed broadly similar trajectories in digital behaviors prior to 2021. Third, enforcement intensity and the surrounding institutional environment varied little across provinces, minimizing the risk that unobserved regional shocks would differentially affect one group but not the other. Together, these factors make it reasonable to assume that, absent the policy, the treatment and comparison groups would have evolved along similar paths.

Interpreting results from the 2020 and 2022 waves requires caution because both were fielded during the COVID-19 pandemic, which profoundly altered students' daily routines, screen exposure, and physical activity. These pandemic-related shocks may have temporarily amplified the estimated effects or produced transitory spikes in Internet usage, making the 2020–2022 comparisons less representative of long-term dynamics. Consequently, ex-

[15] In the 2018 CFPS wave and earlier rounds, the Internet usage questions collected weekly totals, measured in hours of "leisure" Internet use (question U250M). Respondents were asked to report how many hours per week they spent online during their free time, allowing fractional hours of up to 168. In contrast, in the 2020 and 2022 waves, the CFPS more precisely tracked daily Internet usage in minutes: respondents first reported whether they used mobile devices (U201) or computers (U202) to access the Internet, and if so, they provided the typical number of minutes spent online each day using mobile devices (U201A) and computers (U202A). Because the 2018 survey did not disaggregate between device types nor measure usage in daily minutes, the 2020 and 2022 data are not strictly comparable to earlier rounds.

trapolating either the DID results or the earlier 2019 analysis to a non-pandemic environment should be done carefully, as part of the observed responses may reflect pandemic-induced lifestyle adjustments rather than stable behavioral changes.

Finally, the available CFPS measures preclude analyzing the intensity or duration of gaming. The survey aggregates all Internet activity into a single total and only records whether respondents played games daily in the past week—a binary indicator too coarse to serve as a valid instrument for gaming time. Similarly, identifying marginal exposure effects (e.g., each additional week under the policy) would require precise birthdates, which the CFPS does not collect. These data limitations restrict the analysis to average treatment effects rather than continuous dose–response relationships.

Although merging pre-2019 CFPS waves might help check for parallel trends, differences in how Internet use were measured before 2019 limit their usefulness for establishing robust pre-policy dynamics. CFPS's biennial administration constrains the granularity needed to confirm common trends. The 2020 survey also introduced major changes to Internet-use and gaming questions, with pre-2020 rounds reporting total Internet hours in ways that are noncomparable to the new daily-minute measures[16]. Furthermore, the "Internet Module" before 2020 had no questions about online gaming or short video platforms. Consequently, there are not enough consistent pre- and post-policy observations to identify a pretrend or implement an event-study approach for gaming variables.

Although the low sampling frequency and the shifting survey instruments prevent a full event-study approach, there are still reasons to expect parallel trends to hold in this setting. First, the policy was introduced nationwide at a uniform time, without being triggered by prior changes in students' gaming or internet habits, which reduces the likelihood of treatment timing being en-

[16]In the 2018 CFPS wave and earlier waves, the Internet usage questions collected weekly totals, measured in hours of "leisure" Internet use (question U250M). Respondents were asked to report how many hours per week they spent online during their free time, allowing fractional hours of up to 168. In contrast, in the 2020 and 2022 waves, the CFPS more precisely tracked daily Internet usage in minutes: respondents first reported whether they used mobile devices (U201) or computers (U202) to access the Internet, and if so, they provided the typical number of minutes spent online each day using mobile devices (U201A) and computers (U202A). Because the 2018 survey did not disaggregate between device types, nor measure usage in daily minutes, the 2020 or 2022 data are not strictly comparable to earlier rounds.

dogenous to past outcomes. Second, before the policy, there is little evidence from available descriptive statistics that the targeted groups were already on systematically different trajectories compared to unaffected counterparts. Finally, the environment and enforcement surrounding gaming restrictions did not vary substantially across regions—further minimizing the risk that unobserved shocks might differentially affect one group but not the other. These points bolster the plausibility that, absent the policy, the outcome trends for the treatment and comparison groups would have followed similar paths.

It is important to note that the 2020 and 2022 CFPS waves were conducted in the second half of each respective year, both of which fell under the COVID-19 pandemic. The widespread disruptions caused by the pandemic may have temporarily altered the daily routines of individuals, potentially amplifying the effects of the policy under study. Consequently, extrapolating the DID findings to a non-pandemic setting should be approached with caution, as the observed outcomes might reflect an unusually high level of policy adherence driven by pandemic-related lifestyle changes rather than a stable, long-term response.

The 2019 analysis, which relies on the 2018 and 2020 CFPS waves, is more susceptible to pandemic-related disruptions. Because the 2020 survey was conducted during COVID-19, the comparability with the pre-pandemic 2018 data is limited, and part of the estimated effect may capture temporary increases in online activity rather than genuine policy responses. For this reason, I report the 2019 results only in the Appendix for reference.

Although one might wish to explore how the total duration of gaming affects time use or other outcomes (for instance, asking whether 10 hours of gaming per week displaces certain activities), or how each additional week under the policy influences individual behavior, these questions cannot be answered with the available data. In particular, measuring total gaming duration would require more precise indicators of game-specific usage, but the survey aggregates all internet time into a single measure, and the dummy variable indicating daily gaming within the last week does not survive as a strong instrument in this dataset. Consequently, using that dummy fails to yield a reliable coefficient that isolates the effect of gaming time. Similarly, identifying a marginal policy effect—for example, an extra week of policy enforcement—would require knowing respondents' precise birthdates, which the data do not provide. Hence, while these are compelling avenues for investigation, the current data constraints preclude analyzing them here.

5.3.3. Measurement of Digital Time Use and Reporting Error

Measurement error in this study primarily concerns the dependent variable self-reported digital time use. Under the classical outcome-error assumption, where reporting noise is uncorrelated with treatment or covariates, the DID/FE slopes remain unbiased but less precise. Using the meta-analytic reliability between self-reports and device logs ($r \simeq 0.38$; Parry et al., 2021), a simple calibration indicates that the true confidence intervals would be narrower, implying even stronger statistical significance of the main effects. However, the data do not permit testing whether reporting errors are strictly classical. If non-classical components such as systematic under- or over-reporting exist, the estimated coefficients should be interpreted as effects on self-reported rather than actual time use, and the direction of bias depends on the nature of misreporting. Therefore, while the main results are robust under the classical benchmark, the analysis remains subject to the broader challenge of measurement error. Readers interested in the calibration procedure and sensitivity to alternative reliability values ($r \in [0.33, 0.42]$) can refer to A.1.

5.4. Peer Effects and Network Spillovers

The analysis in this subsection examines a potential but conceptually important channel—peer interactions. While the primary design focuses on the direct treatment effect of the 2021 gaming restriction on minors, it is possible that behavioral adjustments propagated through social networks within schools. If treated minors curtail gaming or internet use, their older classmates—especially those sharing the same dormitory or class—may also modify their own behavior, creating *peer spillovers* that extend beyond the legally targeted group.

Because the CFPS does not contain detailed information on classroom composition or friendship networks, this study cannot fully identify peer effects in the structural sense. Instead, the cohort-comparison approach below provides indirect evidence on their presence and potential magnitude. This limitation also implies that peer interactions represent a plausible source of concern for the main DID estimates: if minors' behavioral changes influence nearby untreated peers, the estimated policy effects may partly reflect network diffusion rather than purely individual-level responses. Hence, this subsection aims not to isolate causal peer effects, but rather to assess their likely direction, magnitude, and implications for the interpretation of the main results.

To explore this possibility, I compare adjacent cohorts around the legal age cutoff, using 19-year-olds as the reference group. Tables D.21–D.24 summarize these results. On the extensive margin, there is little evidence that older peers reduced overall participation in online activities: the estimates of $Post \times Cohort$ are small and statistically insignificant across cohorts (e.g., -0.049 [0.035] for any internet use and -0.009 [0.054] for any gaming; Tables D.21 and D.23). These near-zero effects suggest that broad spillovers to legal-age students were limited.

By contrast, the intensive-margin outcomes reveal more meaningful network responses. For total internet minutes, 16- and 17-year-olds show substantial post-policy declines relative to 19-year-olds (-0.197^{**} and -0.175^{*}; Table D.22), corresponding to reductions of roughly 45–50 minutes per day from their 2020 baselines. The attenuation is especially pronounced in boarding schools, where students spend most of their time together (e.g., -0.270^{*} for 17-vs-19; s.e. = 0.140). A similar pattern appears for gaming: "any gaming" remains stable in the full sample, but large negative effects emerge in boarding schools (-0.199^{**} for 16-vs-19; Table D.23). For daily gaming, Post$\times$Cohort coefficients reach -0.199^{**}, -0.263^{***}, and -0.178^{**} for 16-, 17-, and 18-year-olds, respectively (Table D.24), implying 20–25 percentage-point drops from baseline participation. That 18-year-olds—who are legally unaffected—also reduced daily gaming in boarding schools supports the presence of spillovers from restricted minors to near-adult peers.

To verify that such interactions do not confound identification, I conduct a placebo test restricting the sample to 18–19-year-olds (Table D.25). None of the interaction terms between post-policy exposure and county-level internet coverage are significant, indicating no spurious association between local internet access and behavioral changes among adults. This strengthens the interpretation that the observed network responses reflect genuine within-school diffusion rather than omitted regional shocks.

Overall, the evidence suggests that peer effects are (i) concentrated in high-contact environments such as boarding schools and (ii) more pronounced on intensive rather than extensive margins. These network responses help explain why the main difference-in-differences estimates show stronger effects in boarding environments and for high-frequency behaviors. Because such settings represent a limited share of the population, the overall estimates are best viewed as lower bounds of the direct policy effect. Notably, the negative but insignificant coefficient for 18-year-olds' daily gaming relative to 19-year-olds—who were not legally restricted—suggests that peer influence likely spilled over to untreated students. If so, the main DID estimates for daily gaming may be biased toward zero, implying that the true behavioral contraction in the broader adolescent population could be even larger. Rather than biasing identification in an arbitrary direction, peer spillovers likely *amplify* behavioral adjustments, as minors' restricted gaming reshapes collective norms within tightly knit school networks. From a policy perspective, this endogenous amplification implies that enforcement effectiveness depends on the structure of peer interactions: regulations targeting individual behavior can yield larger aggregate effects where students share dormitories, routines, or online networks, consistent with social-multiplier mechanisms observed in other education and youth-policy contexts.

5.5. Mechanisms Behind the Null Effects: Substitution and Psychological Responses

A fundamental principle in behavioral economics holds that restricting one form of leisure activity does not inherently increase engagement in productive alternatives; individuals often substitute restricted behaviors with equally non-productive (but still utility-yielding) activities. Although the gaming restrictions successfully reduced minors' gaming time and overall internet usage, this decline alone does not guarantee improvements in academic performance or health. Adolescents may simply redirect leisure time to other unproductive activities, experience elevated stress from losing a favored pastime, or lack the support structures needed to channel newly freed time into educational gains.

Table 5 shows no significant uptake of alternative online platforms (e.g., short-video services), but the CFPS survey might not capture all possible substitutes—especially offline or idle leisure with minimal academic or health value. The substantial drop in total internet usage indicates that minors did not simply replace gaming with other digital entertainment; instead, their leisure might have shifted to equally unproductive offline routines, limiting the benefits one might expect from reduced screen time.

Beyond straightforward substitution effects, removing a favored leisure outlet can trigger *psychological or emotional responses* (see Figure 3) that undercut potential benefits. When adolescents lose a frequent source of enjoyment, they may experience *frustration, boredom, or stress*, all of which can hamper concentration and overall well-being. The estimates across various mental health outcomes show a consistent pattern: positive-affect measures such as happiness, life satisfaction, and enjoyment decline significantly

(–0.23 to –0.28 SD), while negative-affect indicators—including feelings of sadness, loneliness, or low mood—tend to increase, albeit insignificantly. These aligned coefficient signs suggest that the policy may have modestly worsened adolescents' subjective well-being, even if the effects are imprecisely estimated due to limited sample size and measurement noise. Moreover, the decrease in reported sleep hours for some students hints at heightened academic pressures, pandemic uncertainties, or the loss of gaming as a stress-relief mechanism. In these scenarios, cutting screen time alone does not translate into better health or learning outcomes if underlying stressors remain unaddressed. Thus, *psychological impacts* represent one possible mechanism—alongside simple substitution of time use—that may reduce or negate the policy's benefits. Furthermore, these psychological responses may also drive adolescents to reallocate their freed-up time toward *non-productive activities*, such as passive social media browsing or aimless internet use, rather than towards academically or physically enriching pursuits.

Although some teenagers likely circumvented the policy via adult IDs, the pronounced reduction in internet time use suggests that widespread noncompliance is not the main reason for the null results. Instead, the lack of notable improvements in educational or health metrics likely stems from a confluence of *behavioral substitution*, *stress responses*, and the inherently weak marginal returns of further reducing gaming. As a policy implication, these findings underscore that limiting access to digital leisure—without accompanying measures such as structured extracurricular programs, mental-health support, or digital literacy—may be insufficient for improving academic or health outcomes.

Turning to the city-level evidence from Binzhou's mock exam (see next Section 6), I complement the national CFPS analysis with a setting that offers more precise measurement of academic performance and a clearer definition of policy exposure. Although the CFPS-based triple-difference estimates show little systematic heterogeneity by local Internet coverage, the Binzhou data provide an important opportunity to assess whether the behavioral changes documented above translate into observable differences in standardized test outcomes. This administrative dataset includes detailed subject-level scores and exact birthdates, allowing a sharper regression–kink design that exploits age-based variation in cumulative policy exposure. Accordingly, the Binzhou analysis serves as a complementary test of the policy's educational implications under a uniform institutional environment and with more reliable outcome and exposure measures.

6. City-level Evidence from Binzhou's Mock Exam

6.1. Piecewise Regression Analysis

Figure J.12 displays the raw relationship between age and standardized major subject scores, with each point representing one of the 19,203 student-level observations in the dataset. While this scatter plot is useful for detecting outliers and assessing the data distribution, its effectiveness for visualizing regression discontinuity (RD) or regression kink (RK) designs is limited. The smooth distribution of scores across the threshold (age 18) suggests no abrupt changes; however, a formal RD/RK design analysis is necessary to identify any nuanced causal effects of exposure to gaming on educational outcomes.

The piecewise regression results presented in Table 6 reveal the relationship between age and standardized major subject scores, capturing both slopes across age segments and instantaneous jumps at key thresholds. In Segment 1 (ages [17, 18)), the slope is -0.161, indicating that scores decline by 0.161 standard deviations for each additional year within this restricted gaming period. In Segment 2 (ages [18, 18.38)), the slope steepens to -0.444, suggesting a more rapid decline in scores during the partial restriction period. In Segment 3 (ages [18.38, 19)), the slope flattens to 0.121, implying a stabilization of scores as students enter the exempt restriction period. Additionally, there is a marginally significant positive jump of 0.056 standard deviations at age 18, but no substantial jump at age 18.38. These results, derived without control variables or fixed effects, underscore the varying academic impacts of gaming exposure during key age transitions.

The difference between the slope coefficients for Segment 1 (-0.161) and Segment 2 (-0.444) represents the Regression Kink (RK) estimate at the age threshold of 18. This RK estimate of -0.283 ($= -0.444 - (-0.161)$) reflects the change in the slope of standardized major subject scores as students move from the fully restricted gaming period (ages [17, 18)) to the partially restricted gaming period (ages [18, 18.38)). A negative RK estimate indicates an acceleration in the decline of scores as students gain access to unrestricted gaming. This estimate, calculated using a uniform kernel function, a local polynomial order of 1, and a bandwidth selection of $[-1, +0.38]$, quantifies the academic cost of transitioning from restricted to unrestricted gaming environments. Notably, one full year of unrestricted gaming freedom corresponds to a decline of 0.283 standard deviations in standardized major subject scores. A semester corresponds to 0.38 years, and based on native assumptions from historical representative time use of high school students, a representative student without restrictions plays 170.98 hours of online games during that semester, compared to 59.77 hours for a student with restrictions. The results can therefore be interpreted as follows: for 111.21 additional hours of online gaming (the difference between restricted and unrestricted gaming), a student's educational outcome declines by approximately $0.283 \times 0.38 = 0.1075$ standard deviations.

For the BC County analysis, a similar RK estimate is observed based on the difference in slopes between Segment 1 (-0.333) and Segment 2 (-0.899). This suggests that for one semester of unrestricted gaming, a student's educational outcome declines by approximately 0.2151 standard deviations. These findings emphasize the critical role of gaming policies in shaping educational outcomes during pivotal developmental stages, with stronger effects observed in high Internet Coverage Rate (ICR) areas like BC County.

6.2. Main Regression Kink (RK) Results

Figure 4 illustrates the visual results of the RD/RK design using global polynomial fits and local sample means for standardized major subject scores. The age distribution is centered around 18, with sparse density outside the 17.38–18.38 range. Quantile-spaced bins (non-overlapping intervals) are used to partition the running variable's support, ensuring that each bin contains the same number of observations within each treatment assignment status. Note that the length of quantile-spaced bins varies: bins are wider in regions of the running variable with fewer observations, and narrower where data density is higher. Panel (a) displays results for the full sample, which includes students from all seven counties in Binzhou City. Panel (b) focuses on Bincheng County, a high-Internet-access area with a higher urban population and administrative importance, as it houses the prefecture government. The global polynomial fit (solid line) provides a smooth approximation to the underlying regression function, while the local sample means (dots) represent the average outcomes within the bins. This combination of global and local perspectives offers a detailed visualization of the treatment effect at the cutoff (age 18) while retaining information about local variations in the data.

Table 7 presents my estimates of the slope discontinuity in exam scores across seven counties, capturing how the age-based policy may alter the trajectory of student performance near the critical age. Panel A employs a first-order polynomial ($p = 1$), while Panel B uses a second-order polynomial ($p = 2$). In each panel, I consider both an MSE-optimal bandwidth selection and a manually specified bandwidth. Across most specifications, the point estimates of the slope discontinuity are negative, implying that crossing the policy cutoff is associated with a downward change in the growth rate of exam scores. Although some estimates have non-trivial magnitudes, the imprecision—reflected in relatively large standard deviations—makes it difficult to reject the hypothesis of no effect.

Turning to Bincheng County, which features high Internet coverage, Table 8 reports a narrower geographic analysis. In Panel C (with $p = 1$), the estimated slope discontinuity ranges from approximately -0.71 to -4.94. In Panel D (with $p = 2$), the estimates stretch from about -0.73 to -9.36. These more substantial negative point estimates suggest that students in Bincheng County might experience a notably steeper decline in exam score growth once passing the threshold. However, large standard deviations in these estimates mean that the true effect is highly uncertain. Furthermore, the variability in point estimates across bandwidth selections and polynomial orders indicates that the Regression Kink estimates can be sensitive to modeling choices, particularly in a setting with discrete age data and potential heterogeneity across different regions.

In sum, while these results do not yield robustly significant evidence of a slope discontinuity in exam scores upon crossing the policy cutoff, they do highlight intriguing patterns that could motivate further investigation—especially in high-coverage settings like Bincheng County. Additional research might explore potential mechanisms behind these negative estimates or test alternative specifications to see whether the policy exerts a more pronounced influence for certain subgroups of students.

To illustrate how Regression Kink (RK) estimates vary with the bandwidth choice, I present local-linear estimates over a range of potential bandwidths for both high- and low-Internet-coverage regions, marking key bandwidths with vertical reference lines. Figure 5 compares these estimates across two distinct settings: panel (a) focuses on Bincheng County, where Internet coverage is high, and panel (b) includes the other six counties with relatively lower coverage. In each panel, the bandwidth varies from 0.1 to 0.5 years, with vertical lines marking (i) the MSE-optimal choice following Calonico et al. (2014b) and (ii) a 0.38-year bandwidth corresponding to a full semester. Notably, the RK estimates in panel (a) cluster between approximately -1 and -0.5, whereas panel (b) hovers closer to zero. This gap suggests that the gaming restriction policy exerts a substantially larger (negative) effect on student performance in high-coverage regions, but shows only a minimal impact in areas with lower Internet penetration. Hence, these patterns underscore that the policy's ultimate effectiveness is critically tied to the level of local Internet infrastructure.

7. Conclusion

This paper evaluates the causal short-run effects of China's 2021 gaming restriction on adolescents' digital behavior, time allocation, and well-being. Using nationally representative survey data from the CFPS and a complementary city-level administrative dataset, I combine difference-in-differences and regression–kink designs around the age-18 cutoff. The first-stage effects are strong and precisely estimated: minors substantially reduced total Internet use and gaming participation after the restriction, indicating strong compliance and providing credible exogenous variation in digital exposure. However, these behavioral changes did not translate into measurable improvements in academic achievement or health.

The adjustments appear largely mechanical, with limited substitution into other productive or restorative activities. Time spent studying, sleeping, exercising, or watching short videos changes little, suggesting weak reallocation away from gaming. Survey responses also indicate small declines in self-reported happiness and mental well-being among minors; while these estimates are *suggestive* rather than definitive, they point to potential short-run welfare costs alongside reduced gaming.

I also document indicative spillovers consistent with peer effects. In boarding schools—where minors and 18-year-olds live and study together—gaming declines are visible even among those just above the cutoff. By contrast, I find no systematic gradient by county-level Internet coverage; given measurement limits and smaller samples, these null heterogeneity results should be interpreted cautiously.

From a policy perspective, command-and-control restrictions can suppress high-frequency gaming when enforcement is credible (e.g., real-name registration and ID verification), but the evidence here indicates limited substitution toward studying, sleep, or exercise in the short run. Without complementary interventions—such as programs that build study habits, self-regulation, or access to counseling—the reduction in gaming does not automatically translate into higher academic performance or better health. Because the available data cover only the first academic year after implementation, the analysis captures short-run behavioral and academic responses rather than potential long-run deterrent effects. Delaying the formation of gaming habits at earlier ages could plausibly have positive long-term consequences—an avenue that remains for future research. Future research should combine longer panels and higher-frequency digital traces with additional administrative outcomes to gauge persistence, unpack mechanisms, and identify which school environments (e.g., boarding versus day schools) amplify or dampen policy effects.

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).

Table 2: Descriptive Statistics: CFPS High School Students Aged 16–19

	2018		2020		2022	
Variable	**<18**	**≥18**	**<18**	**≥18**	**<18**	**≥18**
Panel A: Demographics						
Age	16.56	18.33	16.57	18.33	16.57	18.27
	(0.50)	(0.47)	(0.50)	(0.47)	(0.49)	(0.44)
Male (0/1)	0.52	0.47	0.45	0.57	0.53	0.50
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Urban (0/1)	0.48	0.50	0.48	0.45	0.45	0.46
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Panel B: Internet Usage						
Mobile Internet (0/1)	0.87	0.93	0.91	0.93	0.93	0.98
	(0.33)	(0.26)	(0.28)	(0.26)	(0.25)	(0.14)
Mobile Internet (min/day)			220.76	232.45	216.82	285.27
			(198.47)	(205.30)	(201.20)	(216.09)
PC Internet (0/1)	0.47	0.45	0.31	0.37	0.26	0.38
	(0.50)	(0.50)	(0.46)	(0.48)	(0.44)	(0.49)
PC Internet (min/day)			35.35	39.55	20.64	42.55
			(103.04)	(104.53)	(58.57)	(99.93)
Panel C: Gaming						
Online Gaming (0/1)			0.59	0.57	0.60	0.63
			(0.49)	(0.50)	(0.49)	(0.48)
Gaming Daily (0/1)			0.23	0.25	0.19	0.26
			(0.42)	(0.44)	(0.39)	(0.44)
Panel D: Time Use						
Sleep (hours/day)	7.72	7.60	7.67	7.71	7.42	7.75
	(1.07)	(1.08)	(1.25)	(1.18)	(1.21)	(1.27)
Study (hours/day)	8.42	8.90	8.51	8.78	8.41	8.75
	(2.96)	(3.10)	(2.91)	(3.22)	(3.14)	(3.16)
Exercise (times/week)	2.93	3.28	2.48	2.26	3.12	2.96
	(3.04)	(3.24)	(3.24)	(3.06)	(3.52)	(3.56)
TV/Movie (hours/day)	1.00	1.17	1.00	1.32	0.78	1.12
	(1.15)	(1.34)	(1.23)	(1.51)	(1.12)	(1.50)
Panel E: Educational Outcomes						
Academic Rank in Class	3.53	3.51	3.59	3.57	3.52	3.58
(5=best, 1=worst)	(1.17)	(1.13)	(1.13)	(1.16)	(1.14)	(1.16)
Academic Rank in Grade	3.25	3.25	3.32	3.31	3.16	3.31
(5=best, 1=worst)	(1.13)	(1.09)	(1.10)	(1.11)	(1.05)	(1.08)
Panel F: Health						
Health	3.70	3.75	3.95	3.68	3.94	3.87
(1=Not healthy..5=Very healthy)	(0.90)	(0.85)	(0.84)	(0.92)	(0.86)	(0.87)
Observations	387	323	341	300	407	312

Notes: The sample is restricted to CFPS respondents enrolled in high school. Minors (< 18) versus adults (≥ 18) are defined by age at interview. All dummy variables are indicated as (0/1). Mobile/PC Internet Minutes are measured in minutes per day; Sleep, Study, and TV/Movie are measured in hours per day; Exercise Frequency is measured in times per week. Academic Rank variables are coded from 5 (best) to 1 (worst). Standard deviations are reported in parentheses. After the 2018 wave, the CFPS Internet Module was substantially revised, so several usage-based variables introduced in 2020 have no 2018 equivalents, resulting in omitted statistics. For the full-sample descriptive statistics (including non–high-school respondents), see Table B.10. The minor fluctuation in the male share in 2020 likely reflects small survey-composition variation and does not affect the empirical results.

Table 3: Main Results: Internet and Gaming Outcomes under the 2021 Policy

Panel A: Any Internet Usage	(1)	(2)	(3)	(4)
Minor × Post	-0.006	-0.006	-0.042**	-0.042**
	(0.016)	(0.016)	(0.020)	(0.020)
Minor	-0.042***	-0.042***	-0.005	-0.005
	(0.012)	(0.012)	(0.016)	(0.016)
Post	0.025***		0.051***	
	(0.009)		(0.015)	
Dependent Mean (2020)	0.93	0.93	0.93	0.93
Observations	2,154	2,154	1,360	1,360
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year

Panel B: Total Daily Internet (minutes)	(1)	(2)	(3)	(4)
Minor × Post	-42.4***	-41.9***	-56.4***	-55.6***
	(14.0)	(14.0)	(17.4)	(17.2)
Minor	-40.4***	-40.4***	-13.9	-14.5
	(11.2)	(11.2)	(16.5)	(16.5)
Post	41.6***		47.8**	
	(13.4)		(19.1)	
Dependent Mean (2020)	264	264	259	259
Observations	2,154	2,154	1,360	1,360
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year

Panel C: Any Gaming	(1)	(2)	(3)	(4)
Minor × Post	-0.027	-0.027	-0.107**	-0.106**
	(0.041)	(0.042)	(0.050)	(0.051)
Minor	-0.019	-0.018	0.072**	0.068*
	(0.029)	(0.029)	(0.036)	(0.036)
Post	0.035		0.085**	
	(0.027)		(0.035)	
Dependent Mean (2020)	0.60	0.60	0.58	0.58
Observations	2,154	2,154	1,360	1,360
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year

Panel D: Daily Gaming	(1)	(2)	(3)	(4)
Minor × Post	-0.049	-0.041	-0.084*	-0.086*
	(0.035)	(0.035)	(0.045)	(0.045)
Minor	-0.011	-0.015	0.008	0.015
	(0.024)	(0.023)	(0.033)	(0.032)
Post	-0.001		0.031	
	(0.020)		(0.033)	
Dependent Mean (2020)	0.25	0.25	0.24	0.24
Observations	2,154	2,154	1,360	1,360
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year

Notes: Each column represents a separate regression. Columns (1)–(2) use the full sample of individuals aged 16–19; Columns (3)–(4) restrict the sample to high school students. All specifications include controls for gender, hukou, and both parents' education, with standard errors clustered at the county level. Panels A, C, and D are estimated using OLS (linear probability model); coefficients represent percentage-point changes in probability. Panel B is estimated using Poisson Pseudo–Maximum Likelihood (PPML), with coefficients converted to implied minute changes as $(e^{\beta} - 1) \times$ Dependent Mean. Raw PPML coefficients prior to conversion are reported in Table C.13 (Full Sample) and Table C.12 (High School Sample) under "Robustness Checks: DID Specification Choice." *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table 4: Triple-Difference (DDD) Estimates: Heterogeneity by Internet Coverage Rate

Panel A: Any Internet Usage	(1)	(2)	(3)	(4)
Minor × Post × ICR	-0.020*	-0.021*	0.017	0.016
	(0.011)	(0.012)	(0.012)	(0.012)
Minor × Post	0.017	0.020	-0.026	-0.023
	(0.020)	(0.020)	(0.024)	(0.024)
Dependent Mean (2020)	0.93	0.93	0.93	0.93
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Panel B: Total Daily Internet (minutes)	(1)	(2)	(3)	(4)
Minor × Post × ICR	1.0	-0.6	-0.6	-5.4
	(17.1)	(17.1)	(18.2)	(18.4)
Minor × Post	-45.1**	-43.2**	-55.3**	-49.2**
	(20.7)	(20.3)	(22.5)	(22.9)
Dependent Mean (2020)	263	263	250	250
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Panel C: Any Gaming	(1)	(2)	(3)	(4)
Minor × Post × ICR	0.003	-0.008	-0.015	-0.023
	(0.047)	(0.049)	(0.054)	(0.053)
Minor × Post	-0.028	-0.029	-0.141**	-0.124*
	(0.059)	(0.061)	(0.068)	(0.069)
Dependent Mean (2020)	0.60	0.60	0.59	0.59
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Panel D: Daily Gaming	(1)	(2)	(3)	(4)
Minor × Post × ICR	0.025	0.019	-0.012	-0.022
	(0.040)	(0.039)	(0.063)	(0.065)
Minor × Post	-0.065	-0.054	-0.102	-0.123*
	(0.052)	(0.052)	(0.068)	(0.069)
Dependent Mean (2020)	0.24	0.24	0.23	0.23
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year

Notes: Each column reports coefficients from a triple-difference (DDD) regression of the indicated outcome on the interaction term Minor × Post × ICR and its lower-order components. Columns (1)–(2) use the full sample of individuals aged 16–19, while Columns (3)–(4) restrict the sample to academic high school students. All regressions control for gender, hukou, and both parents' education, with standard errors clustered at the county level. Panels A, C, and D are estimated using OLS (linear probability model), and Panel B uses Poisson Pseudo–Maximum Likelihood (PPML); coefficients in Panel B are converted to implied minute changes as $(e^{\beta} - 1) \times$ Dependent Mean. Only the key interaction Minor × Post × ICR and the baseline DID term Minor × Post are reported here; full regression results including all lower-order terms are presented in Table C.14. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table 5: Effects of the 2021 Policy on Behavioral, Educational, and Health Outcomes of High School Students

Panel A: Digital Substitution				
	Short Video (0/1)	Daily Short Video (0/1)	Online Study (0/1)	Daily Online Study (0/1)
Minor × Post	0.036	0.024	-0.041	-0.028
	(0.045)	(0.065)	(0.055)	(0.048)
Minor	-0.039	-0.126***	0.106**	0.034
	(0.036)	(0.045)	(0.042)	(0.036)
Post	0.055**	0.035	0.035	0.034
	(0.027)	(0.045)	(0.042)	(0.031)
Dependent Mean (2020)	0.82	0.51	0.43	0.17
Estimation Method	LPM	LPM	LPM	LPM
Observations	1,004	1,004	1,004	1,004
Panel B: Time Use and Lifestyle				
	Sleep (hrs/day)	Study (weekday hrs)	Study (weekend hrs)	Exercise (times/week)
Minor × Post	-0.045**	0.023	-0.086	-0.154
	(0.020)	(0.043)	(0.100)	(0.161)
Minor	-0.004	-0.025	-0.114	0.180
	(0.015)	(0.033)	(0.071)	(0.126)
Post	0.007	0.006	-0.017	0.344**
	(0.015)	(0.030)	(0.070)	(0.137)
Dependent Mean (2020)	7.66	10.24	5.70	2.48
Estimation Method	PPML	PPML	PPML	PPML
Observations	1,004	1,004	1,004	1,004
Panel C: Reading, Health, and Academic Performance				
	Reading (books/year)	Health Index	Class Score (z-score)	Grade Score (z-score)
Minor × Post	-0.285	-0.244	-0.016	-0.168
	(0.344)	(0.125)	(0.139)	(0.139)
Minor	0.420***	0.325***	-0.041	-0.002
	(0.159)	(0.086)	(0.115)	(0.102)
Post	0.271	0.228**	0.001	0.025
	(0.199)	(0.099)	(0.103)	(0.092)
Dependent Mean (2020)	5.98	-0.07	-0.00	0.04
Estimation Method	PPML	OLS	OLS	OLS
Observations	1,004	1,004	1,004	1,004

Notes: Each column corresponds to a separate regression restricted to high school students. All specifications include controls for gender, hukou, and both parents' education, with province fixed effects and county-level clustered standard errors (in parentheses). Continuous time-use variables (e.g., hours, exercise frequency, reading) are estimated via PPML to account for skewness and zero values, while binary outcomes use LPM. The *Health Index* and academic scores are standardized (mean = 0, sd = 1) across the pooled sample (2020–2022); higher values indicate better health or stronger academic performance. Adjusted p-values for multiple hypothesis testing are reported in Appendix Table C.18. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table 6: Piecewise Regression Results for Standardized Major Subject Scores

	(1)	(2)	(3)	(4)
β_1: Slope before age 18	-0.050	-0.161***	-0.185	-0.333***
	(0.056)	(0.051)	(0.113)	(0.104)
β_2: Slope for (18, 18.38)	-0.444***	-0.444***	-0.899***	-0.899***
	(0.117)	(0.117)	(0.260)	(0.260)
β_3: Slope after 18.38		0.121		0.378
		(0.138)		(0.354)
γ_1: Jump at Age 18	0.031	0.056*	0.121*	0.153**
	(0.032)	(0.032)	(0.069)	(0.068)
γ_2: Jump at Age 18.38		0.023		-0.163
		(0.054)		(0.134)
β_0: Constant	0.908	2.878***	3.472*	6.117***
	(0.995)	(0.897)	(2.010)	(1.833)
Age Range	[17.38, 18.38]	[17, 19]	[17.38, 18.38]	[17, 19]
Geographical Range	7 Counties	7 Counties	BC County	BC County
Observations	16,401	18,125	3,581	3,896

Notes: This table presents the results of piecewise regression models where the outcome variable is the standardized major subject scores. The analysis includes both BC County and all 7 counties. BC County, with an Internet Coverage Rate (ICR) of 0.755, represents a high Internet coverage region. The remaining 6 counties have an average ICR of 0.320, indicating a lower Internet coverage level. The age range of [17.38, 18.38] is chosen to align with the typical progression through compulsory education in China, assuming no grade-skipping or grade-repetition. Coefficients β_1, β_2, and β_3 represent the slopes for each segment, with β_1 capturing the slope before age 18, β_2 for ages [18, 18.38], and β_3 above age 18.38. Coefficients γ_1 and γ_2 capture the jumps at the thresholds of 18 and 18.38, respectively. Standard errors are in parentheses. Stars denote significance at * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Estimate of Slope Discontinuity (RK) under Different Bandwidth Choices

	Panel A: Polynomial Order ($p = 1$)		
	(1)	**(2)**	**(3)**
Model Description	$[17, 19]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `manual` $h(0.62,\ 0.38)$
Estimate of Slope Discontinuity (RK)			
Point Estimate	-0.208	-0.237	-0.394**
(Std. Error)	(0.375)	(0.895)	(0.129)
Kernel Function	Uniform	Uniform	Uniform
Polynomial Order (p)	1	1	1
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.184/0.204	0.124/0.116	0.62/0.38
Effective Obs (Left)	3,519	2,306	10,062
Effective Obs (Right)	3,243	1,805	6,339
Clustered at School Level	Yes	Yes	Yes
Geographical Coverage	7 Counties	7 Counties	7 Counties
	Panel B: Polynomial Order ($p = 2$)		
	(1)	**(2)**	**(3)**
Model Description	$[17, 19]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `manual` $h(0.62,\ 0.38)$
Estimate of Slope Discontinuity (RK)			
Point Estimate	-0.297	-2.877*	0.551
(Std. Error)	(1.143)	(2.905)	(0.395)
Kernel Function	Uniform	Uniform	Uniform
Polynomial Order (p)	2	2	2
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.223/0.250	0.203/0.117	0.62/0.38
Effective Obs (Left)	4,233	3,847	10,062
Effective Obs (Right)	4,161	1,849	6,339
Clustered at School Level	Yes	Yes	Yes
Geographical Coverage	7 Counties	7 Counties	7 Counties

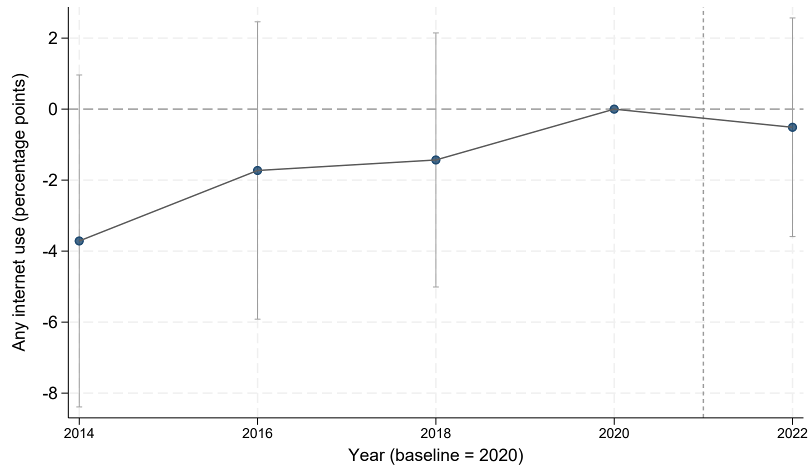
Notes: Panel A employs polynomial order $p = 1$, while Panel B uses $p = 2$. A uniform kernel is used throughout, with cluster-robust standard errors at the school level. Columns (1) and (2) rely on MSE-optimal bandwidth, while column (3) applies a manually specified bandwidth of $h(0.62, 0.38)$. No fixed effects or additional controls are included. In each panel, the first row in the Estimate of Slope Discontinuity block is the point estimate, and the second row (in parentheses) reports the corresponding standard error. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Estimate of Slope Discontinuity (RK) under Different Bandwidth Choices (Bincheng County)

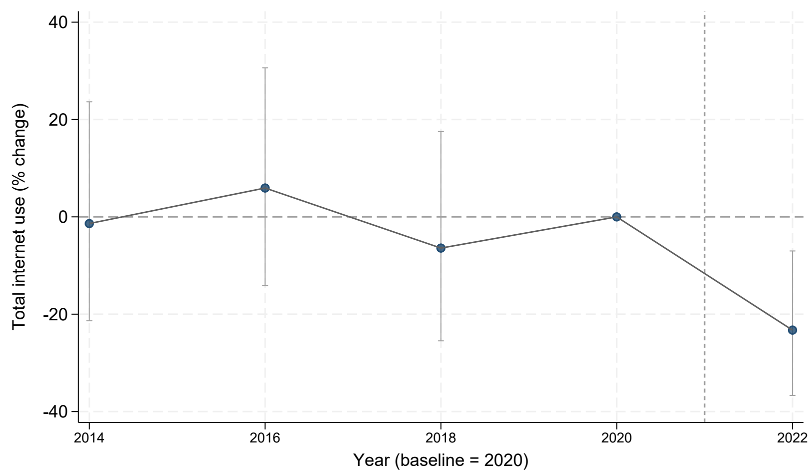
Panel C: Polynomial Order ($p = 1$)			
Model Description	**(1)** [17, 19], `Optimal Bandwidth`	**(2)** [17.38, 18.38], `Optimal Bandwidth`	**(3)** [17.38, 18.38], `manual` $h(0.62, 0.38)$
Estimate of Slope Discontinuity (RK)			
Point Estimate	-0.967	-4.936*	-0.714**
(Std. Error)	(0.997)	(3.182)	(0.278)
Kernel Function	Uniform	Uniform	Uniform
Polynomial Order (p)	1	1	1
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.178/0.187	0.098/0.082	0.620/0.380
Effective Obs (Left)	775	422	2,336
Effective Obs (Right)	582	259	1,245
Clustered at School Level	No	No	No
Geographical Coverage	BC County	BC County	BC County

Panel D: Polynomial Order ($p = 2$)			
Model Description	**(1)** [17, 19], `Optimal Bandwidth`	**(2)** [17.38, 18.38], `Optimal Bandwidth`	**(3)** [17.38, 18.38], `manual` $h(0.62, 0.38)$
Estimate of Slope Discontinuity (RK)			
Point Estimate	-2.758	-9.364*	-0.730
(Std. Error)	(2.743)	(6.392)	(1.084)
Kernel Function	Uniform	Uniform	Uniform
Polynomial Order (p)	2	2	2
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.208/0.292	0.185/0.117	0.620/0.380
Effective Obs (Left)	891	799	2,336
Effective Obs (Right)	984	371	1,245
Clustered at School Level	No	No	No
Geographical Coverage	BC County	BC County	BC County

Notes: Panels C and D both focus on Bincheng (BC) County, which has a high Internet coverage rate. Panel C uses polynomial order $p = 1$, while Panel D uses $p = 2$. A uniform kernel is used throughout, and the estimations are not clustered at the school level. Columns (1) and (2) rely on the MSE-optimal bandwidth, while column (3) applies a manually specified bandwidth of $h(0.62, 0.38)$. No fixed effects or additional controls are included. In each panel, the first row in the Estimate of Slope Discontinuity block is the point estimate, and the second row (in parentheses) reports the corresponding standard error. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.



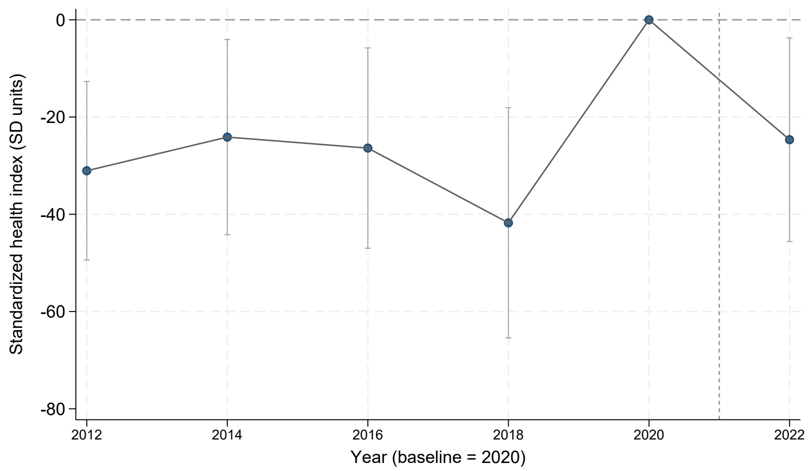
(a) Extensive Margin: Any Internet



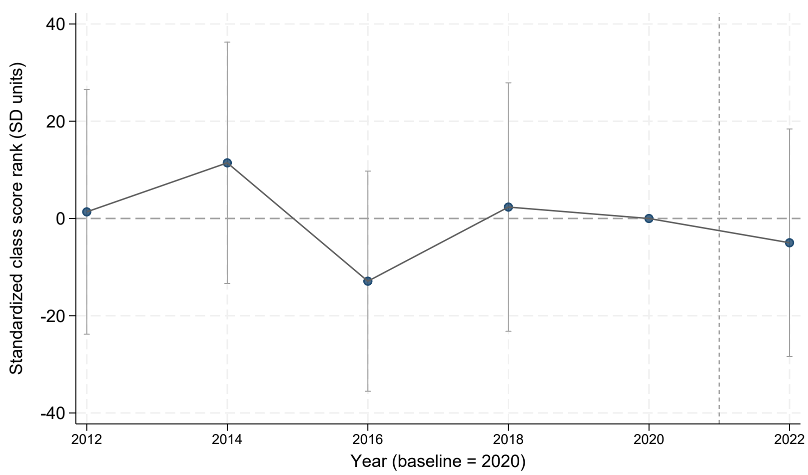
(b) Intensive Margin: Internet Minutes

Figure 1: Event Study: Extensive and Intensive Margins of Internet Usage

Notes: Panel (a) presents LPM estimates for the extensive margin (any internet use), and Panel (b) presents PPML estimates for the intensive margin (total internet minutes). Values are expressed as percentage points (Panel a) and percentage changes (Panel b) relative to 2020. All regressions control for gender, Hukou status, and parental education, and include province and year fixed effects. Standard errors are clustered at the county level. Vertical bars indicate 95% confidence intervals.



(a) Self-reported Health

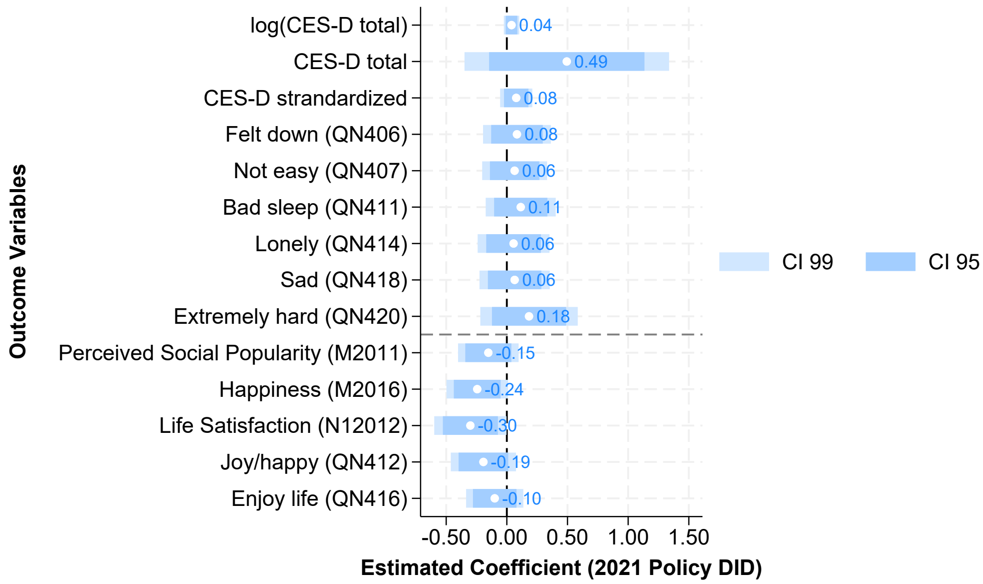


(b) Standardized Score Rank in Class

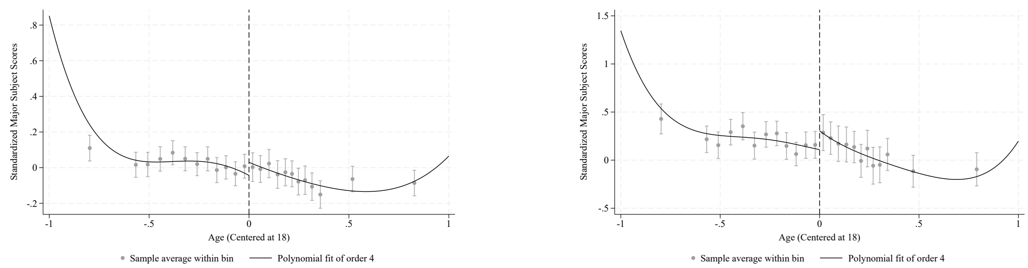
Figure 2: Event Study: Health Status and Educational Outcome

Notes: Panel (a) reports estimates for standardized self-reported health, and Panel (b) reports estimates for standardized class score ranks. All coefficients are expressed in standardized units (SD units) relative to the baseline year 2020. All regressions control for gender, Hukou status, and parental education, and include province and year fixed effects. Standard errors are clustered at the county level. Vertical bars indicate 95% confidence intervals.

Figure 3: Difference-in-Differences Estimates of the 2021 Policy on Mental Health Outcomes



Notes: This figure reports Difference-in-Differences (DID) estimates of the 2021 minor-protection policy on mental health outcomes. The Center for Epidemiologic Studies Depression Scale (CES-D), originally developed by Radloff (1977), is a self-reported measure of depressive symptoms constructed using eight CFPS survey items (QN406, QN407, QN411, QN412, QN414, QN416, QN418, QN420), which assess mood, fatigue, sleep, and life satisfaction. The combined CES-D score ranges from 8 to 32, with higher values indicating more severe depressive symptoms. In the figure, outcomes above the dashed line (e.g., *felt down*, *bad sleep*, *lonely*) are negatively oriented—larger values correspond to worse mental health—while outcomes below the dashed line (e.g., *happiness*, *life satisfaction*, *enjoy life*) are positively oriented—larger values indicate better mental health. All categorical outcomes are estimated using ordered probit models. All regressions control for gender, urban residence, and parental education, and include province and year fixed effects. Standard errors are clustered at the county level, and vertical bars denote 95% and 99% confidence intervals.

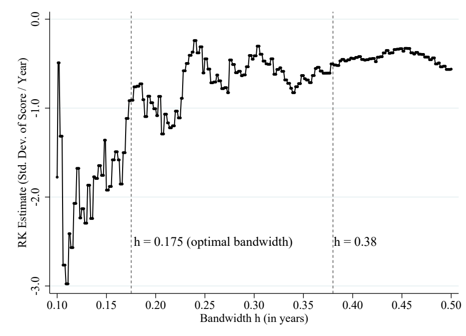


(a) Full Sample

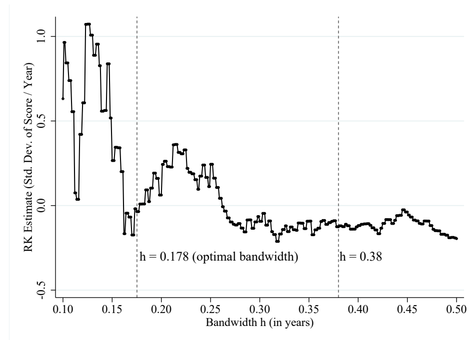
(b) Bincheng County (High Internet Area)

Figure 4: Visual RD/RK Results

Notes: The solid line represents the global polynomial fit based on a fourth-order polynomial regression, fitted separately above and below the cutoff at age 18. The dots represent the local sample means, calculated within disjoint bins of the running variable (age), and plotted against the bin midpoints. The global polynomial provides a smooth approximation to the underlying regression function, while the local sample means illustrate local variability and behavior. The confidence intervals, shown around the local sample means, represent 95% confidence intervals.



(a) High Internet Area (Bincheng County)



(b) Low Internet Area (the Other 6 Counties)

Figure 5: RK estimates with bandwidth from 0.1 year to 0.5 year

Notes: All Regression Kink (RK) estimates in these plots use a uniform kernel and a first-order local polynomial ($p = 1$). The displayed optimal bandwidths also follow the approach of Calonico et al. (2014b) under the same kernel and polynomial order. Bincheng (BC) County is classified as a high-Internet-Coverage-Rate (ICR = 0.76) region, while the other six counties have ICR values of 0.389, 0.384, 0.318, 0.294, 0.272, and 0.265. A full semester is approximately 0.38 years; thus one can interpret $h = 0.38$ as covering an entire semester. On the vertical axis, -1 unit corresponds to a one-standard-deviation reduction in exam scores over one year of unrestricted gaming ($\approx$ 292 additional gaming hours, based on the naive hourly calculation).

Appendix Roadmap

This appendix provides additional materials, robustness analyses, and supplementary evidence supporting the main results.

- **Appendix A.1** documents the measurement framework for digital time-use variables, discusses potential reporting bias, and presents calibrated confidence intervals under classical and non-classical measurement-error assumptions.

- **Appendix A.2** reports extended descriptive statistics and complementary information on sample composition across years and subgroups.

- **Appendix A.3** presents robustness checks for the main difference-in-differences (DID) and triple-difference (DDD) specifications, including alternative estimators, fixed effects, and age windows. This section also reports second-stage estimates on time allocation, academic outcomes, and health, with Romano–Wolf stepdown p-value adjustments for multiple hypothesis testing.

- **Appendix A.4** provides peer-effect and placebo analyses, including age-cohort comparisons and adult-only falsification tests.

- **Appendix A.5** examines heterogeneity by gender, family size, and baseline income, showing consistent patterns across subgroups.

- **Appendix A.6** provides complementary results for the earlier 2019 policy, included for context but not part of the main identification strategy.

- **Appendix A.9** describe the regression-discontinuity (RD) and regression-kink (RK) analyses based on the Binzhou administrative dataset, including validation tests, bandwidth robustness, and slope-jump decompositions.

- Finally, **Appendix A.10** provides supplementary documentation for the Binzhou administrative dataset used in the regression-kink analysis. It reports detailed summary statistics, explains the construction of the exposure variable, and validates the identification design through density and robustness checks.

A.1. Measurement and Calibration

The digital time use variables in this study are based on respondents' retrospective self-reports under a standardized questionnaire framework. These measures capture how respondents recall and describe their past behavior; they are not objective traces from device logs or administrative records. As a result, the DID/FE estimates are technically effects on *self-reported* time use rather than on actual behavior. However, researchers and policymakers are ultimately interested in true usage patterns. The subsequent discussion therefore considers what the regression estimates imply for actual time use, subject to explicit measurement assumptions and external evidence on the reliability of self-reports relative to logs (Parry et al., 2021).

In this design, the digital time-use variable is the *dependent* variable. Under the classical outcome-error model $y_{ipt} = y^*_{ipt} + v_{ipt}$ with $\mathbb{E}[v_{ipt} \mid D_{ipt}, X_{ipt}, \delta_p, \eta_t] = 0$ and $v_{ipt} \perp (D_{ipt}, X_{ipt}, \delta_p, \eta_t, \epsilon_{ipt})$, DID/FE *slopes* (including β_1) are *unbiased/consistent*; the cost is *less precision* (inefficiency).[17]

Several departures illustrate how conclusions change.[18] When such non-classical conditions plausibly arise, coefficients are best read as effects on *self-reported* time use; the likely direction of bias depends on the sign of $b - 1$, δ, or any group$\times$time shift a_1. Diagnostic safeguards include event-study plots for breaks at the policy date, group-specific time trends, and placebo outcomes unaffected by the policy.

For inference, the analysis maintains the classical benchmark and *calibrates precision* (not point estimates) using the meta-analytic reliability between self-reports and logs, $r \simeq 0.38$ (95% CI $[0.33, 0.42]$) from Parry et al.

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[17] **Why unbiased under $y = y^* + v$.** Let $Z_{ipt} \equiv (D_{ipt}, X_{ipt}, \delta_p, \eta_t)$ and residualize: $\tilde{y} = M_Z y$, $\tilde{y}^* = M_Z y^*$, $\tilde{D} = M_Z D$. Then $\hat{\beta}_1 = (\tilde{D}^\top \tilde{D})^{-1} \tilde{D}^\top \tilde{y} = (\tilde{D}^\top \tilde{D})^{-1} \tilde{D}^\top \tilde{y}^* + (\tilde{D}^\top \tilde{D})^{-1} \tilde{D}^\top \tilde{v}$. Since $\mathbb{E}[v \mid Z] = 0 \Rightarrow \mathbb{E}[\tilde{v} \mid Z] = 0$ and $\tilde{D}$ is Z-measurable, $\mathbb{E}[\tilde{D}^\top \tilde{v}] = 0 \Rightarrow \mathbb{E}[\hat{\beta}_1] = \beta_1$.

[18] **Additive and non-classical variants (sketches).** (i) *Additive offset:* $y_{ipt} = a + y^*_{ipt} + v_{ipt}$. The constant a is absorbed by the intercept/FE, so slopes remain unbiased. (ii) *Scale distortion:* $y_{ipt} = a + b\, y^*_{ipt} + v_{ipt}$ with $b \neq 1$ and $v \perp Z$. Then $\mathbb{E}[\hat{\beta}_1] = b\, \beta_1$ (slopes multiplied by b) (Wooldridge, 2021, Sec. 9.4). (iii) *Error correlated with truth:* $v = \delta\, y^* + \nu^*$ with $\mathbb{E}[\nu^* \mid Z] = 0$ gives $y = (1 + \delta)\, y^* + \nu^*$ and proportional slope bias $\mathbb{E}[\hat{\beta}] = (1 + \delta)\beta$ (Bound et al., 2001, p. 3714). (iv) *Treatment$\times$time reporting shift:* $y_{ipt} = a_0 + a_1 D_{ipt} + y^*_{ipt} + v_{ipt}$ with $\mathbb{E}[v \mid Z] = 0$ implies $\mathbb{E}[\hat{\beta}_1] = \beta_1 + a_1$; if minors under-report *more* after the policy ($a_1 < 0$), the DID slope is mechanically more negative than the true effect.

(2021). This tightens CIs while leaving $\hat{\beta}$ unchanged; because logs are not perfectly measured, using $r = 0.38$ is *conservative* for precision.[19]

Table A.9: Calibrated Confidence Intervals for Main DID Estimates (Minor×Post)

	(1)	(2)	(3)	(4)
Panel A: Any Internet Usage (LPM)				
Observed 95% CI	[–0.037, 0.025]	[–0.037, 0.025]	[–0.081, –0.003]	[–0.081, –0.003]
Calibrated CI ($r = 0.38$)	[–0.018, 0.006]	[–0.018, 0.006]	[–0.057, –0.027]	[–0.057, –0.027]
Panel B: Total Daily Internet (minutes, PPML implied)				
Observed 95% CI	[–69.8, –15.0]	[–69.3, –14.5]	[–90.5, –22.3]	[–89.3, –21.9]
Calibrated CI ($r = 0.38$)	[–52.8, –32.0]	[–52.3, –31.5]	[–69.4, –43.4]	[–68.4, –42.8]
Panel C: Any Gaming (LPM)				
Observed 95% CI	[–0.107, 0.053]	[–0.109, 0.055]	[–0.205, –0.009]	[–0.206, –0.006]
Calibrated CI ($r = 0.38$)	[–0.058, 0.004]	[–0.058, 0.004]	[–0.144, –0.070]	[–0.144, –0.068]
Panel D: Daily Gaming (LPM)				
Observed 95% CI	[–0.118, 0.020]	[–0.110, 0.028]	[–0.172, 0.004]	[–0.174, 0.002]
Calibrated CI ($r = 0.38$)	[–0.075, –0.023]	[–0.067, –0.015]	[–0.118, –0.051]	[–0.120, –0.052]

Notes: Each cell reports the 95% confidence interval for the coefficient on **Minor×Post** in Table 3. "Observed" intervals use reported standard errors; "Calibrated" intervals adjust precision using the reliability of self-reports relative to device logs ($r = 0.38$) from Parry et al. (2021). Under the classical outcome-error model, calibration narrows CIs while leaving point estimates unchanged. Results show that intensive-margin effects (Panels B and D) remain negative and statistically significant after calibration, while extensive-margin outcomes (Panels A and C) remain near zero.

[19]**CI calibration.** With $y = y^* + v$ and $v \perp y^*$, $r^2 = \mathrm{Var}(y^*)/\mathrm{Var}(y)$. For the DID/FE slope, SE inflates by $\sqrt{\mathrm{VIF}} = \sqrt{(1/r^2 - \kappa)/(1-\kappa)}$ where $\kappa \equiv R^2_{\text{true, within}}$. When $\kappa \approx 0$, $\mathrm{SE}_{\text{true}} \approx r\,\mathrm{SE}_{\text{obs}}$ so $\mathrm{CI}_{\text{true}}:\ \hat{\beta} \pm 1.96 \cdot r\,\mathrm{SE}_{\text{obs}}$. Sensitivity is reported for $r \in [0.33, 0.42]$ (Parry et al., 2021).

A.2. Descriptive Statistics and Data Background

A.2.1. Alternative Descriptive Statistics

Table B.10: Descriptive Statistics: CFPS Full Sample Aged 16–19

	2018		2020		2022	
Variable	**<18**	**≥18**	**<18**	**≥18**	**<18**	**≥18**
Panel A: Demographics						
Age	16.47	18.49	16.46	18.49	16.49	18.44
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Male (0/1)	0.56	0.49	0.49	0.57	0.54	0.52
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Urban (0/1)	0.42	0.54	0.44	0.46	0.42	0.45
	(0.49)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)
Panel B: Internet Usage						
Mobile Internet (0/1)	0.85	0.93	0.91	0.95	0.93	0.98
	(0.36)	(0.25)	(0.29)	(0.22)	(0.26)	(0.14)
Mobile Internet (minutes/day)			214.21	248.65	218.24	293.56
			(197.90)	(207.10)	(201.88)	(210.99)
PC Internet (0/1)	0.40	0.49	0.28	0.43	0.25	0.46
	(0.49)	(0.50)	(0.45)	(0.49)	(0.43)	(0.50)
PC Internet (minutes/day)			29.90	47.88	20.76	58.78
			(92.86)	(105.12)	(59.72)	(115.03)
Panel C: Gaming						
Online Gaming (0/1)			0.58	0.63	0.61	0.65
			(0.49)	(0.48)	(0.49)	(0.48)
Gaming Daily (0/1)			0.23	0.27	0.21	0.27
			(0.42)	(0.44)	(0.41)	(0.45)
Panel D: Time Use						
Sleep (hours/day)	7.93	7.91	7.84	7.86	7.55	7.78
	(1.21)	(1.19)	(1.24)	(1.27)	(1.27)	(1.17)
Study (hours/day)	8.23	7.95	8.23	7.94	8.21	7.78
	(2.84)	(3.09)	(2.81)	(3.30)	(3.09)	(3.21)
Exercise (times/week)	2.76	2.81	2.44	2.04	3.07	2.69
	(2.93)	(2.99)	(3.39)	(2.84)	(3.59)	(3.35)
TV/Movie (hours/day)	1.07	1.31	1.06	1.36	0.84	1.19
	(1.15)	(1.29)	(1.23)	(1.51)	(1.12)	(1.57)
Panel E: Educational Outcomes						
Academic Rank in Class	3.48	3.50	3.58	3.57	3.53	3.60
(5=best, 1=worst)	(1.18)	(1.14)	(1.17)	(1.16)	(1.15)	(1.17)
Academic Rank in Grade	3.23	3.25	3.27	3.31	3.16	3.33
(5=best, 1=worst)	(1.16)	(1.10)	(1.15)	(1.11)	(1.07)	(1.09)
Panel F: Health						
Health	3.72	3.78	3.96	3.71	3.96	3.84
(1=Not healthy..5=Very healthy)	(0.94)	(0.94)	(0.90)	(0.94)	(0.87)	(0.86)
Observations	655	671	558	513	551	532

Notes: Minors (< 18) versus adults (≥ 18) are based on respondents' ages at the time of each CFPS survey. Standard deviations are reported in parentheses.

A.3. Robustness Checks

A.3.1. DID Specification Choices

Table C.11: Robustness Checks: DID Specification Choice for Full Sample

Panel A: OLS (minutes)	(1)	(2)	(3)	(4)
Minor × Post	-59.14***	-59.23***	-60.36***	-59.11***
	(21.13)	(19.77)	(19.75)	(19.72)
Minor	-49.60***	-46.54***	-48.22***	-48.11***
	(15.20)	(14.66)	(14.61)	(14.71)
Post	55.56***	52.50***	51.52***	
	(15.45)	(15.09)	(14.98)	
Dependent Mean	277.6	277.6	277.6	277.6
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	2,154	2,154	2,154	2,154
Panel B: Log($y+1$)	(1)	(2)	(3)	(4)
Minor × Post	-0.243*	-0.241*	-0.258*	-0.245*
	(0.149)	(0.141)	(0.141)	(0.143)
Minor	-0.337***	-0.322***	-0.326***	-0.325***
	(0.111)	(0.108)	(0.108)	(0.110)
Post	0.388***	0.361***	0.361***	
	(0.101)	(0.100)	(0.100)	
Dependent Mean	4.86	4.86	4.86	4.86
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	2,154	2,154	2,154	2,154
Panel C: PPML	(1)	(2)	(3)	(4)
Minor × Post	-0.190**	-0.191***	-0.194***	-0.191***
	(0.077)	(0.071)	(0.071)	(0.070)
Minor	-0.187***	-0.177***	-0.184***	-0.184***
	(0.058)	(0.056)	(0.056)	(0.056)
Post	0.175***	0.164***	0.159***	
	(0.050)	(0.048)	(0.048)	
Dependent Mean	277.6	277.6	277.6	277.6
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	2,154	2,154	2,154	2,154

Notes: Robust standard errors clustered at the county level in parentheses. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Panel A reports OLS in levels. Panel B reports results with log(y+1) transformation. Panel C reports Poisson Pseudo–Maximum Likelihood (PPML) estimates. Dependent Mean is calculated as the sample mean of the dependent variable in each specification. All regressions use individuals aged 16–19 from the 2020 and 2022 CFPS waves.

Table C.12: Robustness Checks: DID Specification Choice for High School Sample

Panel A: OLS (minutes)	(1)	(2)	(3)	(4)
Minor × Post	-72.42***	-69.25***	-70.72***	-69.71***
	(25.14)	(24.35)	(24.44)	(24.29)
Minor	-13.23	-12.79	-14.07	-14.55
	(18.43)	(17.79)	(18.02)	(18.11)
Post	54.93***	52.42***	51.46***	
	(18.88)	(18.58)	(18.61)	
Dependent Mean	265.7	265.7	265.7	265.7
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	1,360	1,360	1,360	1,360
Panel B: Log($y+1$)	(1)	(2)	(3)	(4)
Minor × Post	-0.414**	-0.352*	-0.375**	-0.385**
	(0.187)	(0.184)	(0.185)	(0.184)
Minor	-0.054	-0.075	-0.076	-0.067
	(0.138)	(0.134)	(0.136)	(0.139)
Post	0.441***	0.397***	0.401***	
	(0.141)	(0.139)	(0.141)	
Dependent Mean	4.83	4.83	4.83	4.83
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	1,360	1,360	1,360	1,360
Panel C: PPML	(1)	(2)	(3)	(4)
Minor × Post	-0.260***	-0.249***	-0.253***	-0.249***
	(0.093)	(0.089)	(0.089)	(0.088)
Minor	-0.051	-0.050	-0.057	-0.059
	(0.071)	(0.069)	(0.069)	(0.069)
Post	0.188***	0.179***	0.173***	
	(0.066)	(0.064)	(0.064)	
Dependent Mean	265.7	265.7	265.7	265.7
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	1,360	1,360	1,360	1,360

Notes: Robust standard errors clustered at the county level in parentheses. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Panel A reports OLS in levels. Panel B reports results with log(y+1) transformation. Panel C reports Poisson Pseudo–Maximum Likelihood (PPML) estimates. Dependent Mean is calculated as the sample mean of the dependent variable in each specification. All regressions restrict the sample to high school students.

A.3.2. Triple-Differences: Specification Choices

Table C.13: Robustness Checks: DDD Specification Choices

	(1)	(2)	(3)	(4)
Panel A: OLS (minutes)				
Minor × Post ×z-ICR	-10.830	0.384	4.154	-2.699
	(22.486)	(20.000)	(21.153)	(21.335)
Minor × Post	-57.924*	-55.006**	-58.994**	-56.776**
	(30.237)	(27.200)	(26.645)	(26.361)
Minor ×z-ICR	11.639	6.676	7.159	10.272
	(20.922)	(17.839)	(17.867)	(17.208)
Post ×z-ICR	-3.752	-1.045	-1.777	-1.941
	(28.910)	(27.084)	(28.169)	(30.532)
Minor	-57.446***	-53.983***	-52.506***	-51.766***
	(20.937)	(19.408)	(18.837)	(18.683)
Post	53.497**	56.558**	58.593**	
	(24.707)	(23.007)	(22.572)	
z-ICR	25.334	3.629	2.922	3.270
	(27.611)	(20.835)	(21.130)	(22.460)
Dependent Mean	274.7	274.7	274.7	274.7
Observations	1,074	1,074	1,074	1,074
Panel B: Log($y+1$)				
Minor × Post ×z-ICR	-0.279**	-0.219**	-0.226*	-0.249**
	(0.114)	(0.109)	(0.115)	(0.116)
Minor × Post	-0.251	-0.217	-0.241	-0.205
	(0.182)	(0.163)	(0.173)	(0.174)
Minor ×z-ICR	0.276***	0.251***	0.250***	0.269***
	(0.092)	(0.085)	(0.085)	(0.094)
Post ×z-ICR	0.037	0.052	0.045	0.050
	(0.137)	(0.130)	(0.138)	(0.128)
Minor	-0.263**	-0.259*	-0.251*	-0.264*
	(0.132)	(0.131)	(0.131)	(0.138)
Post	0.358***	0.353***	0.364***	
	(0.136)	(0.130)	(0.136)	
z-ICR	0.112	0.000	0.001	-0.000
	(0.128)	(0.128)	(0.131)	(0.135)
Dependent Mean	4.83	4.83	4.83	4.83
Observations	1,074	1,074	1,074	1,074
Panel C: PPML				
Minor × Post ×z-ICR	-0.033	0.004	0.001	-0.006
	(0.060)	(0.059)	(0.064)	(0.064)
Minor × Post	-0.179*	-0.171*	-0.185**	-0.179**
	(0.106)	(0.095)	(0.093)	(0.090)
Minor ×z-ICR	0.059	0.045	0.044	0.052
	(0.054)	(0.052)	(0.053)	(0.051)
Post ×z-ICR	-0.019	-0.014	-0.019	-0.012
	(0.077)	(0.080)	(0.084)	(0.092)
Minor	-0.223***	-0.210***	-0.203***	-0.204***
	(0.074)	(0.069)	(0.067)	(0.066)
Post	0.168**	0.177***	0.184***	
	(0.073)	(0.067)	(0.065)	
z-ICR	0.077	0.007	0.007	0.005
	(0.076)	(0.063)	(0.063)	(0.068)
Dependent Mean	274.7	274.7	274.7	274.7
Observations	1,074	1,074	1,074	1,074

Notes: Robust standard errors clustered at the county level in parentheses. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Panel A reports OLS in levels. Panel B reports results with $\log(y+1)$ transformation. Panel C reports Poisson Pseudo Maximum Likelihood (PPML) estimates. Dependent Mean is calculated as the sample mean of the dependent variable in each specification. Post main effect is omitted when Province×Year FE are included.

A.3.3. Triple-Differences: Alternative Fixed Effects

Table C.14: Triple-Difference (DDD) Estimates: Heterogeneity by Internet Access

Panel A: Any Internet Usage	(1)	(2)	(3)	(4)
Minor × Post × ICR	-0.020*	-0.021*	0.017	0.016
	(0.011)	(0.012)	(0.012)	(0.012)
Minor × Post	0.017	0.020	-0.026	-0.024
	(0.020)	(0.020)	(0.024)	(0.024)
Minor × ICR	0.027***	0.027***	0.001	-0.001
	(0.010)	(0.010)	(0.010)	(0.010)
Post × ICR	-0.010*	-0.009	-0.015	-0.009
	(0.006)	(0.011)	(0.009)	(0.011)
ICR (z-score)	0.004	0.003	0.008	0.005
	(0.009)	(0.011)	(0.012)	(0.013)
Minor	-0.051***	-0.053***	-0.004	-0.007
	(0.016)	(0.016)	(0.020)	(0.020)
Post	0.021*	—	0.050**	—
	(0.012)		(0.016)	
Dependent Mean	0.94	0.94	0.93	0.93
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Fixed Effects	Province	Province×Year	Province	Province×Year
Panel B: Total Daily Internet (minutes)	(1)	(2)	(3)	(4)
Minor × Post × ICR	1.000	-0.600	-0.606	-5.467
	(17.100)	(17.100)	(18.202)	(18.350)
Minor × Post	-45.097**	-43.157**	-55.315**	-49.119**
	(20.673)	(20.300)	(22.460)	(22.936)
Minor × ICR	11.305	13.350	18.957	24.457**
	(14.777)	(14.337)	(13.381)	(12.928)
Post × ICR	-6.483	-3.697	-9.830	-9.580
	(21.907)	(24.253)	(21.261)	(22.088)
ICR (z-score)	2.383	1.189	-2.058	-2.467
	(17.085)	(18.017)	(12.180)	(12.143)
Minor	-49.297***	-49.410***	-26.167	-30.409
	(14.518)	(14.329)	(18.586)	(18.216)
Post	52.174**	—	64.346**	—
	(21.016)		(27.610)	
Dependent Mean (2020)	263	263	250	250
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Fixed Effects	Province	Province×Year	Province	Province×Year
Panel C: Any Gaming	(1)	(2)	(3)	(4)
Minor × Post × ICR	0.003	-0.008	-0.015	-0.023
	(0.047)	(0.049)	(0.054)	(0.053)
Minor × Post	-0.028	-0.029	-0.141**	-0.124*
	(0.059)	(0.061)	(0.068)	(0.069)
Minor × ICR	-0.001	0.011	0.004	0.018
	(0.033)	(0.036)	(0.034)	(0.035)
Post × ICR	-0.003	-0.010	-0.004	-0.021
	(0.023)	(0.027)	(0.033)	(0.035)
ICR (z-score)	0.025	0.025	0.037	0.041
	(0.016)	(0.018)	(0.021)	(0.022)
Minor	-0.046	-0.045	0.085	0.076
	(0.043)	(0.044)	(0.047)	(0.047)
Post	0.044	—	0.096**	—
	(0.037)		(0.046)	
Dependent Mean	0.60	0.60	0.53	0.53
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Fixed Effects	Province	Province×Year	Province	Province×Year
Panel D: Daily Gaming	(1)	(2)	(3)	(4)
Minor × Post × ICR	0.025	0.019	-0.012	-0.022
	(0.040)	(0.039)	(0.063)	(0.065)
Minor × Post	-0.066	-0.054	-0.102	-0.123*
	(0.052)	(0.052)	(0.068)	(0.069)
Minor × ICR	-0.014	-0.010	0.002	0.010
	(0.025)	(0.025)	(0.056)	(0.058)
Post × ICR	-0.013	0.002	-0.027	-0.018
	(0.026)	(0.028)	(0.038)	(0.041)
ICR (z-score)	0.001	-0.008	0.017	0.012
	(0.023)	(0.025)	(0.027)	(0.029)
Minor	-0.036	-0.042	-0.029	-0.010
	(0.036)	(0.034)	(0.054)	(0.054)
Post	0.052	—	0.075	—
	(0.037)		(0.047)	
Dependent Mean	0.25	0.25	0.23	0.23
Observations	1,071	1,071	698	698
Sample	Full	Full	High School	High School
Fixed Effects	Province	Province×Year	Province	Province×Year

Notes: Each column reports estimates from a triple-difference regression including the interaction Minor × Post × ICR and all lower-order terms. Columns (1)–(2) use the full sample of individuals aged 16–19; Columns (3)–(4) restrict the sample to high school students. All specifications include controls for gender, hukou, and both parents' education, with standard errors clustered at the county level. Panels A, C, and D are estimated using OLS (linear probability model), and Panel B uses Poisson Pseudo–Maximum Likelihood (PPML), with coefficients converted to implied minute changes as $(e^{\beta} - 1) \times$ Dependent Mean. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

A.3.4. Alternative Age Samples

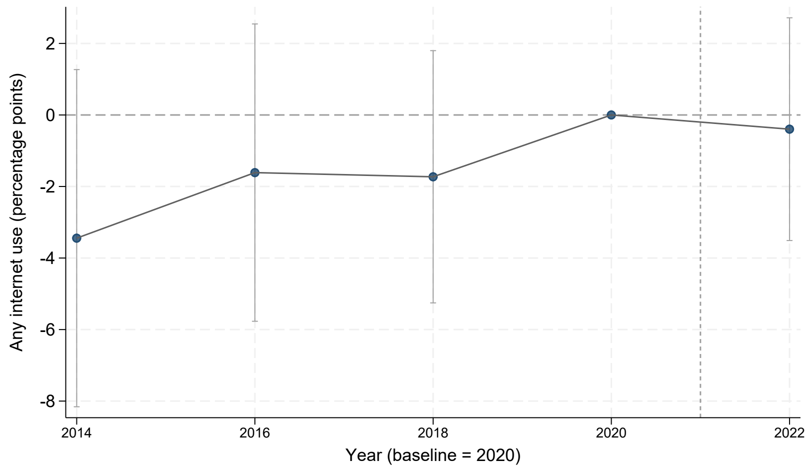
Table C.15: Robustness Checks: Alternative Age Samples

Panel A: Any Internet Usage	(1)	(2)	(3)	(4)
Minor × Post	-0.020	-0.006	-0.004	-0.003
	(0.022)	(0.016)	(0.015)	(0.043)
Minor	-0.010	-0.042***	-0.049***	0.009
	(0.018)	(0.012)	(0.012)	(0.029)
Post	0.032**	0.025***	0.022***	0.066***
	(0.015)	(0.009)	(0.007)	(0.023)
Dependent Mean	0.952	0.945	0.942	0.942
Age Range	17–18	16–19	15–20	15–20
Fixed Effects	Province	Province	Province	Individual
Controls	Yes	Yes	Yes	No
Observations	1,085	2,154	2,728	1,620
Panel B: Total Daily Internet (minutes)	(1)	(2)	(3)	(4)
Minor × Post	-0.211**	-0.194***	-0.162***	-0.214**
	(0.096)	(0.071)	(0.063)	(0.093)
Implied Effect (minutes)	[-55]	[-52]	[-45]	[-56]
Minor	-0.065	-0.184***	-0.243***	0.006
	(0.074)	(0.056)	(0.051)	(0.080)
Post	0.175***	0.159***	0.130***	0.402***
	(0.068)	(0.048)	(0.035)	(0.062)
Dependent Mean	279	277	279	279
Age Range	17–18	16–19	15–20	15–20
Fixed Effects	Province	Province	Province	Individual
Controls	Yes	Yes	Yes	No
Observations	1,085	2,154	2,728	1,616
Panel C: Any Gaming	(1)	(2)	(3)	(4)
Minor × Post	-0.038	-0.027	-0.017	0.014
	(0.057)	(0.041)	(0.037)	(0.076)
Minor	0.004	-0.019	-0.009	-0.060
	(0.039)	(0.029)	(0.027)	(0.057)
Post	0.026	0.035	0.028	-0.014
	(0.038)	(0.027)	(0.021)	(0.047)
Dependent Mean	0.609	0.618	0.609	0.609
Age Range	17–18	16–19	15–20	15–20
Fixed Effects	Province	Province	Province	Individual
Controls	Yes	Yes	Yes	No
Observations	1,085	2,154	2,728	1,620
Panel D: Daily Gaming	(1)	(2)	(3)	(4)
Minor × Post	-0.060	-0.049	-0.031	0.014
	(0.051)	(0.035)	(0.031)	(0.064)
Minor	-0.003	-0.011	-0.018	-0.042
	(0.037)	(0.024)	(0.022)	(0.060)
Post	-0.018	0.016	-0.001	-0.014
	(0.038)	(0.025)	(0.020)	(0.046)
Dependent Mean	0.243	0.248	0.239	0.239
Age Range	17–18	16–19	15–20	15–20
Fixed Effects	Province	Province	Province	Individual
Controls	Yes	Yes	Yes	No
Observations	1,085	2,154	2,728	1,620

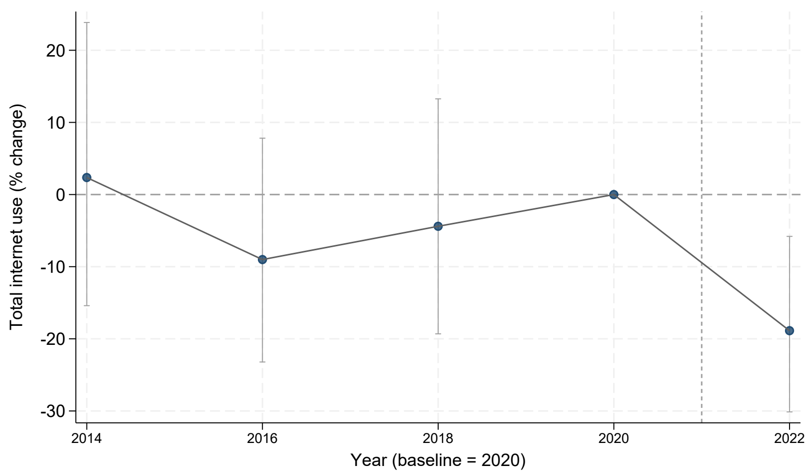
Notes: *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Standard errors clustered at the county level are reported in parentheses. Panels A, C, and D are estimated using OLS (linear probability model); Panel B uses PPML with implied effects in minutes reported as $(e^{\beta} - 1) \times$ Dependent Mean. Specifications vary by age range, fixed effects, and inclusion of controls as indicated.

A.3.5. Event–Study and Parallel–Trend Validation Figures

This subsection presents event–study plots that validate the parallel-trend assumption underlying the DID identification strategy. Each figure reports pre-policy and post-policy coefficients normalized to the baseline year 2020. All regressions include the same controls and fixed effects as the main specification.



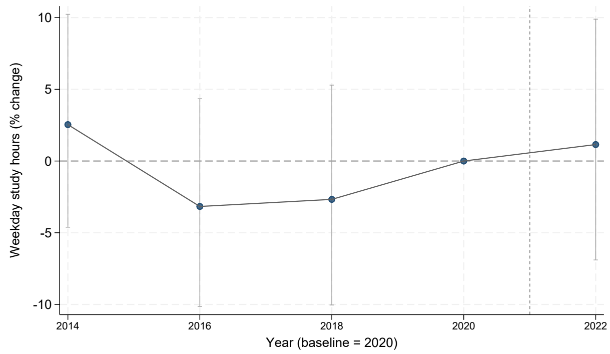
(a) Extensive Margin: Any Internet Use



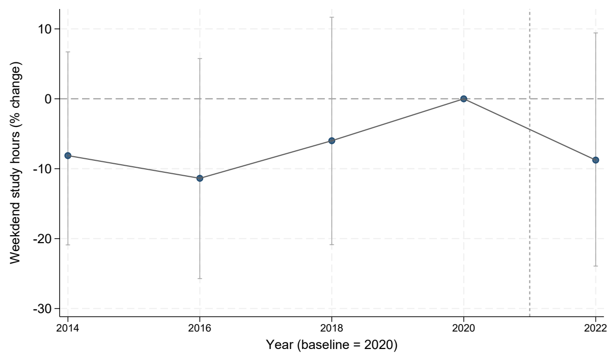
(b) Intensive Margin: Total Internet Minutes

Figure C.6: Event Study: Internet Usage (Full Sample, Ages 16–19)

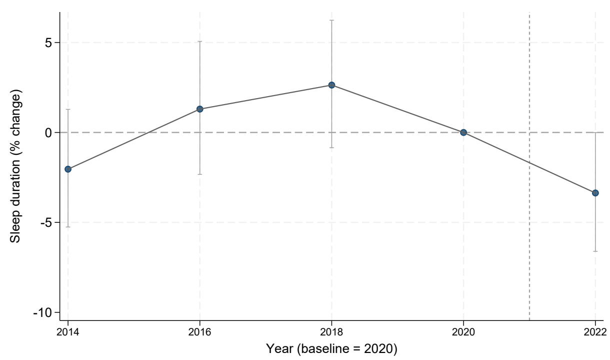
Notes: Panels (a)–(b) present event–study estimates using the **full sample of individuals aged 16–19**, including both high school and non–high school respondents. Panel (a) reports LPM estimates for the extensive margin (any internet use), and Panel (b) reports PPML estimates for the intensive margin (total internet minutes). Coefficients are expressed as percentage points (Panel a) and percentage changes (Panel b) relative to the baseline year 2020. All regressions control for gender, Hukou status, and parental education, and include province and year fixed effects. Standard errors are clustered at the county level. Vertical bars indicate 95% confidence intervals.



(a) Weekday Study Hours



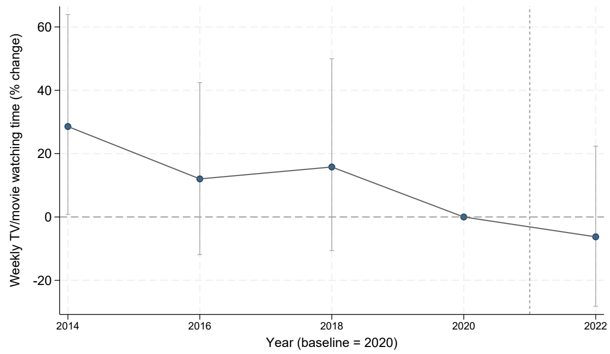
(b) Weekend Study Hours



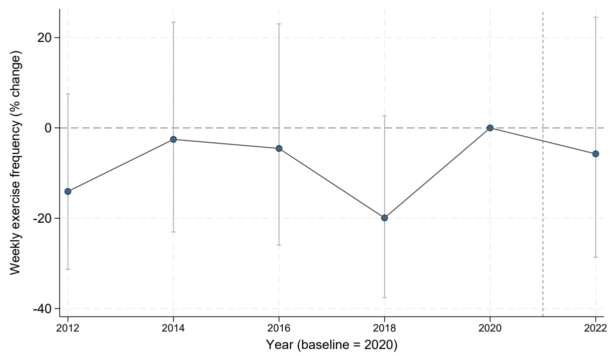
(c) Sleep Duration

Figure C.7: Event Study: Intensive Margins of Study and Sleep Time

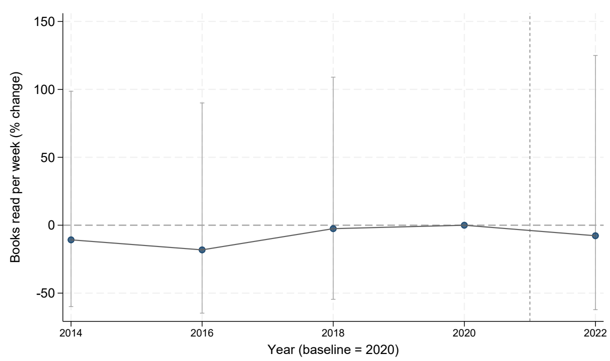
Notes: Each panel reports PPML estimates for a continuous time-use outcome (weekday study hours, weekend study hours, and sleep duration). Coefficients are transformed and expressed as percentage changes relative to the baseline year 2020. All regressions control for gender, Hukou status, and parental education, and include province and year fixed effects. Standard errors are clustered at the county level. Vertical bars denote 95% confidence intervals.



(a) Screen Entertainment (TV/Movie Time)



(b) Exercise Frequency



(c) Book Reading Amount

Figure C.8: Event Study: Intensive Margins of Leisure and Physical Activities

Notes: Panels (a)–(c) report PPML estimates for continuous outcomes: weekly screen entertainment time (TV/movie watching), weekly exercise frequency, and weekly book reading. Coefficients are expressed as percentage changes relative to 2020. All regressions control for gender, Hukou status, and parental education, and include province and year fixed effects. Standard errors are clustered at the county level. Vertical bars denote 95% confidence intervals.

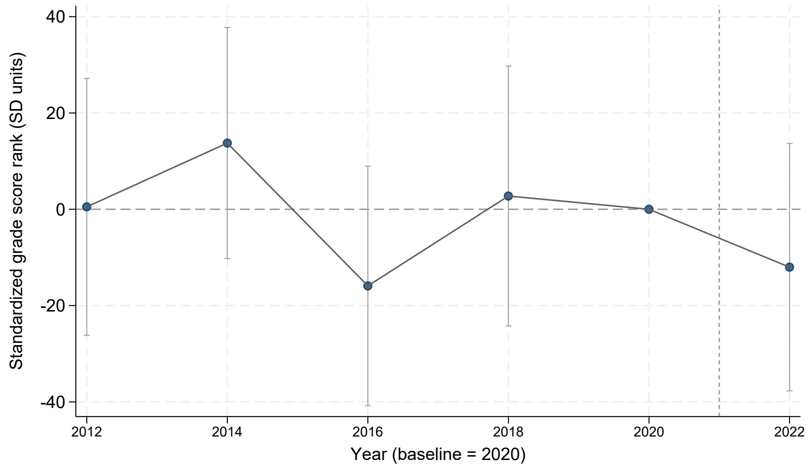


Figure C.9: Event Study: Standardized Grade Score Rank

Notes: OLS estimates for standardized grade score ranks (mean = 0, SD = 1). Coefficients are expressed in standard-deviation units relative to the baseline year 2020. Regressions control for gender, urban residence, and parental education, and include province and year fixed effects. Standard errors are clustered at the county level, and vertical bars denote 95% confidence intervals.

A.3.6. HonestDiD Sensitivity for Internet Time

This appendix quantifies how our Internet-time results depend on the parallel-trends assumption using the HonestDiD approach of Rambachan and Roth (2023b). I treat 2020 as the baseline year, the pre-period as $\{2014, 2016, 2018\}$, and the post-period as 2022 (see Figure C.10).

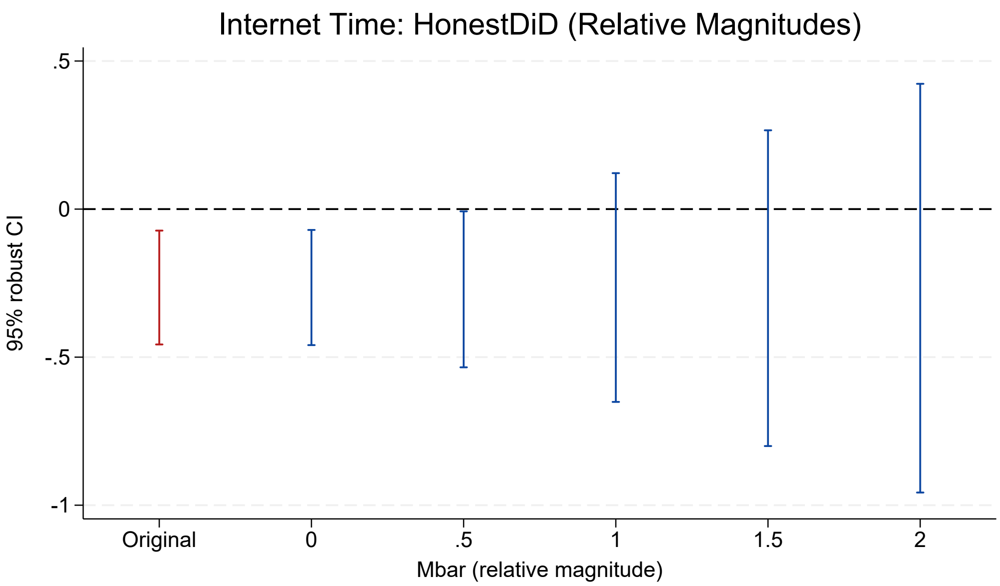


Figure C.10: Sensitivity of Internet Time Estimates to Parallel-Trend Deviations (HonestDiD)

Notes: Pre-period is $\{2014, 2016, 2018\}$ with 2020 as baseline; post-period is 2022. The horizontal axis shows $\bar{M}$, the maximum allowed post-treatment trend deviation as a multiple of the largest absolute pre-trend difference.[20] Confidence intervals are 95% robust confidence sets following Rambachan and Roth (2023b). Effects are estimated on the log scale via PPML; percentage changes can be recovered as $100 \cdot (\exp(\beta) - 1)$.Under the relative-magnitude bound (Δ^{RM}), setting $\bar{M} = 0$ effectively imposes exact parallel trends in the post-treatment period, so the robust CI essentially matches the conventional DID CI.

A.3.7. Second-Stage Results: Alternative Sample Definitions

Table C.16: Second-Stage Estimates Using the Maximum Available Sample (2020–2022 CFPS High School Students)

Panel A: Digital Substitution				
	Short Video (0/1)	Daily Short Video (0/1)	Online Study (0/1)	Daily Online Study (0/1)
Minor × Post	0.002	0.034	-0.076*	-0.050
	(0.030)	(0.046)	(0.043)	(0.033)
Adjusted *p*-value	1.000	0.980	0.545	0.733
Minor	-0.030	-0.138***	0.056**	0.018
Post	0.054***	0.050*	0.074**	0.044*
Observations	2,034	2,034	2,034	2,034
R-squared	0.065	0.058	0.042	0.039
Panel B: Time Use and Lifestyle				
	Sleep (hrs/day)	Study (weekday hrs)	Study (weekend hrs)	Exercise (times/week)
Minor × Post	-0.030**	0.027	-0.122	-0.000
	(0.013)	(0.040)	(0.079)	(0.078)
Adjusted *p*-value	0.257	0.980	0.733	1.000
Minor	-0.001	0.042	0.082	0.100**
Post	-0.006	-0.024	-0.013	0.194***
Observations	2,149	1,900	1,899	2,150
R-squared	0.041	0.055	0.060	0.080
Panel C: Health and Academic Performance				
	Reading (books/year)	Health Index	Class Score (z-score)	Grade Score (z-score)
Minor × Post	-0.186	-0.168*	-0.058	-0.102
	(0.332)	(0.091)	(0.111)	(0.118)
Adjusted *p*-value	0.980	0.455	0.980	0.980
Minor	0.394***	0.293***	-0.009	-0.044
Post	0.083	0.154**	0.026	0.002
Observations	2,149	2,163	1,496	1,306
R-squared	0.108	0.058	0.034	0.031

Notes: The sample includes all CFPS respondents (ages 16–19) observed in 2020 and 2022. **Minor** equals 1 for individuals under 18, and **Post** equals 1 for 2022. Robust standard errors clustered at the county level are reported in parentheses for **Minor × Post**. Stars on coefficients denote significance based on unadjusted p-values (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). All regressions include province fixed effects and control for gender, hukou, and both parents' education. Continuous time-use outcomes (study hours, sleep, exercise, and reading) are estimated via PPML to address skewness and zeros. The *Health Index* and academic scores are standardized (mean = 0, sd = 1); higher values indicate better health or stronger academic performance.

A.3.8. Second-Stage Results: Alternative Fixed Effects Specifications

Table C.17: Effects of the 2021 Policy on Behavioral, Educational, and Health Outcomes of Academic High School Students (Province FE, Common Sample)

Panel A: Digital Substitution				
	Short Video (0/1)	Daily Short Video (0/1)	Online Study (0/1)	Daily Online Study (0/1)
Minor × Post	0.040	0.035	-0.035	-0.025
	(0.047)	(0.065)	(0.055)	(0.047)
Adjusted p-value	0.976	0.984	0.984	0.984
Minor	-0.045	-0.135***	0.102**	0.031
Post	0 (omitted)	0 (omitted)	0 (omitted)	0 (omitted)
Observations	1,002	1,002	1,002	1,002
R-squared	0.101	0.110	0.091	0.084
Panel B: Time Use and Lifestyle				
	Sleep (hrs/day)	Study (weekday hrs)	Study (weekend hrs)	Exercise (times/week)
Minor × Post	-0.046**	0.026	-0.096	-0.141
	(0.021)	(0.045)	(0.103)	(0.168)
Adjusted p-value	0.371	0.984	0.958	0.984
Minor	-0.003	-0.028	-0.102	0.184
Post	0 (omitted)	0 (omitted)	0 (omitted)	0 (omitted)
Observations	1,002	1,002	1,002	1,002
R-squared	0.097	0.093	0.109	0.115
Panel C: Reading, Health, and Academic Performance				
	Reading (books/year)	Health Index	Class Score (z-score)	Grade Score (z-score)
Minor × Post	-0.193	-0.262**	-0.012	-0.177
	(0.324)	(0.127)	(0.141)	(0.142)
Adjusted p-value	0.984	0.371	0.984	0.894
Minor	0.421***	0.326***	-0.042	0.003
Post	0 (omitted)	0 (omitted)	0 (omitted)	0 (omitted)
Observations	1,002	1,002	1,002	1,002
R-squared	0.252	0.097	0.064	0.048

Notes: The sample is restricted to high school students with non-missing values across all second-stage outcomes (N = 1,002). **Minor** is an indicator for being under 18, and **Post** equals one for 2022. Robust standard errors clustered at the county level are shown in parentheses for the interaction term. Stars on coefficients indicate significance from unadjusted p-values (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). All specifications include province-by-year fixed effects and controls for gender, hukou, and both parents' education. Continuous time-use outcomes (study hours, sleep, exercise, and books) are estimated via PPML to handle skewness and zero values. The *Health Index* and academic scores are standardized (mean = 0, sd = 1), with higher values indicating better health or stronger academic performance. Adjusted p-values for multiple hypothesis testing are reported in Appendix Table C.18.

A.3.9. Multiple Hypothesis Testing

Table C.18: Romano–Wolf Stepdown Adjusted p-values for Second-Stage Outcomes (High School Sample)

	Province FE				**Province × Year FE**			
	Model	Resample	RW	Holm	Model	Resample	RW	Holm
Short Video (any)	0.430	0.411	0.984	1.000	0.400	0.387	0.976	1.000
Short Video (daily)	0.712	0.693	0.985	1.000	0.591	0.605	0.984	1.000
Online Study (any)	0.452	0.454	0.984	1.000	0.528	0.538	0.984	1.000
Online Study (daily)	0.563	0.559	0.984	1.000	0.587	0.588	0.984	1.000
Sleep (log hours)	0.045	0.052	0.404	0.623	0.042	0.045	0.371	0.495
Weekday Study (log hours)	0.839	0.844	0.985	1.000	0.811	0.813	0.984	1.000
Weekend Study (log hours)	0.376	0.378	0.975	1.000	0.333	0.331	0.958	1.000
Exercise Frequency (log)	0.404	0.385	0.984	1.000	0.472	0.469	0.984	1.000
Book Reading (log)	0.724	0.712	0.985	1.000	0.598	0.598	0.984	1.000
Health Index (std.)	0.053	0.052	0.417	0.571	0.041	0.036	0.371	0.432
Class Score (std.)	0.911	0.908	0.985	0.908	0.935	0.936	0.984	0.936
Grade Score (std.)	0.230	0.249	0.891	1.000	0.216	0.238	0.894	1.000
Rejections (5% level)	0				0			

Notes: This table reports Romano–Wolf stepdown and Holm–Bonferroni adjusted p-values for the estimated effects of the 2021 policy on multiple behavioral, educational, and health outcomes among high school students. Each outcome is tested separately using the coefficient on **Minor** × **Post**. All regressions include gender, hukou, and parents' education as controls. Column blocks compare two specifications: (i) province fixed effects and (ii) province-by-year fixed effects. Standard errors are clustered at the county level. No null hypotheses are rejected at the 5% level after multiple-testing adjustment.

A.3.10. *First-Stage Estimates by Internet Channel and Step-Down p-Value Adjustments*

Table C.19: Detailed First-Stage Estimates: Effects of the 2021 Policy on Internet and Gaming Behavior (Non-missing Sample)

	(1) Gaming	(2) Daily Gaming	(3) Mobile	(4) Mobile Minutes	(5) PC	(6) PC Minutes
Panel A: Estimated Coefficients						
Minor × Post	-0.1085**	-0.0856*	-0.0322	-58.2101**	-0.0449	-17.2532
	(0.0502)	(0.0446)	(0.0218)	(22.4689)	(0.0429)	(10.8946)
Post (2020)	0.0855**	0.0340	0.0521***	49.3506***	0.0015	5.1593
	(0.0343)	(0.0327)	(0.0160)	(17.2863)	(0.0299)	(7.3640)
Minor (<18)	0.0732**	0.0068	-0.0152	-11.8254	-0.0770**	-3.3317
	(0.0357)	(0.0329)	(0.0176)	(15.5564)	(0.0313)	(8.2601)
Observations	1,358	1,358	1,358	1,358	1,358	1,358
R-squared	0.194	0.129	0.051	0.088	0.135	0.104
Panel B: Multiple Testing Adjusted *p*-values (Minor × Post)						
Model *p*-value	0.032	0.056	0.141	0.010	0.296	0.115
Romano–Wolf *p*-value	0.150	0.211	0.322	0.072	0.322	0.322
Holm *p*-value	0.150	0.224	0.288	0.096	0.310	0.402

Notes: Each column reports a separate OLS regression with robust standard errors clustered at the county level (in parentheses). All regressions include province and year fixed effects and control for gender, hukou status, and parental education (mother's and father's schooling). **Minor** equals one if the respondent was under age 18, and **Post** equals one for the 2020 wave. The interaction term **Minor × Post** captures the causal effect of the 2019 gaming-restriction policy for minors. **Gaming** and **Daily Gaming** indicate any and daily video-game use, respectively. **Mobile** and **PC** indicate whether the respondent used a mobile device or a personal computer to access the internet. **Mobile Minutes** and **PC Minutes** measure total daily usage time (minutes) on each device. Romano–Wolf and Holm *p*-values are step-down multiple-testing adjustments across all six outcomes. All regressions are based on the non-missing subsample to ensure consistent sample size across outcomes. Conceptually, these first-stage behavioral variables are direct policy targets rather than independent hypotheses, so multiple-testing correction is reported for completeness but not interpreted as part of the main inference.

A.4. Peer Effects and Placebo Tests

A.4.1. Sample Distribution by Age Cohort and Survey Year

Table D.20: Sample Distribution by Age and Survey Year (Ages 16–19)

	Age 16	**Age 17**	**Age 18**	**Age 19**	**Total**
Panel A: All Respondent					
2020	301	257	261	252	1,071
2022	282	269	299	234	1,084
Total	583	526	560	486	2,155
Panel B: High School Students					
2020	148	194	201	99	642
2022	173	235	229	84	721
Total	321	429	430	183	1,363
Panel C: Boarding High School Students					
2020	110	150	141	75	476
2022	130	177	165	64	536
Total	240	327	306	139	1,012

Notes: This table reports the number of observations by age (16–19) and survey year (2020, 2022) for three samples. Panel A includes all students aged 16–19. Panel B restricts to students currently enrolled in high school. Panel C further restricts to boarding high school students.

A.4.2. Peer Effect Robustness: Internet Outcomes by Age Cohort

Table D.21: Peer Effect Estimates: Any Internet Use by Age Cohort

	16 vs 19	17 vs 19	18 vs 19
Panel A: All Students			
Post × Cohort	0.008	-0.001	0.019
	(0.024)	(0.024)	(0.021)
Observations	1,069	1,012	1,044
Dependent Mean (2020)	0.94	0.95	0.96
Panel B: High School Students			
Post × Cohort	-0.049	-0.043	-0.021
	(0.035)	(0.036)	(0.035)
Observations	501	611	611
Dependent Mean (2020)	0.97	0.93	0.93
Panel C: Boarding High Schools			
Post × Cohort	-0.046	-0.036	-0.031
	(0.050)	(0.045)	(0.044)
Observations	373	466	441
Dependent Mean (2020)	0.92	0.91	0.88
Controls	Yes	Yes	Yes
Fixed Effects	Province FE	Province FE	Province FE
Clustering	County	County	County

Notes: Dependent variable is an indicator for any Internet use. Each column compares the indicated age cohort (16-, 17-, or 18-year-olds) with 19-year-olds in a difference-in-differences design. Coefficients correspond to the interaction term **Post × Cohort** (2022). All regressions include controls for gender, hukou, and both parents' education, with province fixed effects and county-clustered standard errors (in parentheses). Dependent Mean is the share using Internet in 2020 for each subsample. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table D.22: Peer Effect Estimates: Total Internet Usage by Age Cohort

	16 vs 19	17 vs 19	18 vs 19
Panel A: All Students			
Post × **Cohort**	-0.197**	-0.175*	0.011
	(0.089)	(0.098)	(0.091)
Observations	1,069	1,012	1,044
Dependent Mean (2020)	267.4	278.9	290.3
Panel B: High Schools			
Post × **Cohort**	-0.214	-0.221*	-0.039
	(0.140)	(0.130)	(0.136)
Observations	501	611	611
Dependent Mean (2020)	243.6	253.1	251.5
Panel C: Boarding High Schools			
Post × **Cohort**	-0.162	-0.270*	-0.031
	(0.155)	(0.140)	(0.147)
Observations	373	466	441
Dependent Mean (2020)	260.0	272.6	247.4
Controls	Yes	Yes	Yes
Fixed Effects	Province FE	Province FE	Province FE
Clustering	County	County	County

Notes: Dependent variable is total daily internet minutes. Coefficients correspond to the interaction term **Post** × **Cohort** (2022), estimated using Poisson pseudo–maximum likelihood (PPML). Each column compares the indicated cohort (ages 16, 17, or 18) to 19-year-olds as the baseline group. All regressions include gender, hukou, and parents' education as controls and province fixed effects. Standard errors (in parentheses) are clustered at the county level. Dependent Mean reports the average internet minutes per day in 2020 for each corresponding subsample. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

A.4.3. Peer Effect Robustness: Gaming Outcomes by Age Cohort

Table D.23: Peer Effect Estimates: Any Gaming Behavior by Age Cohort

	16 vs 19	17 vs 19	18 vs 19
Panel A: All Students			
Post × Cohort	-0.009	-0.043	-0.003
	(0.054)	(0.057)	(0.050)
Observations	1,069	1,012	1,044
Dependent Mean (2020)	0.49	0.48	0.48
Panel B: High Schools			
Post × Cohort	-0.118	-0.139*	-0.033
	(0.072)	(0.078)	(0.072)
Observations	501	611	611
Dependent Mean (2020)	0.46	0.45	0.45
Panel C: Boarding High Schools			
Post × Cohort	-0.199**	-0.186*	-0.070
	(0.087)	(0.096)	(0.083)
Observations	373	466	441
Dependent Mean (2020)	0.47	0.45	0.44
Controls	Yes	Yes	Yes
Fixed Effects	Province FE	Province FE	Province FE
Clustering	County	County	County

Notes: Dependent variable is an indicator for any gaming in the past week. Coefficients correspond to the interaction term **Post × Cohort** (2022), estimated using linear probability models (OLS). Each column compares the indicated cohort (ages 16, 17, or 18) to 19-year-olds as the baseline group. All regressions include controls for gender, hukou, and parents' education, with province fixed effects. Standard errors (in parentheses) are clustered at the county level. Dependent Mean reports the mean gaming participation rate in 2020 for each corresponding subsample. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table D.24: Peer Effect Estimates: Daily Gaming Behavior by Age Cohort

	16 vs 19	17 vs 19	18 vs 19
Panel A: All Students			
Post × **Cohort**	-0.062	-0.125**	-0.078
	(0.048)	(0.053)	(0.054)
Observations	1,069	1,012	1,044
Dependent Mean (2020)	0.18	0.19	0.20
Panel B:High Schools			
Post × **Cohort**	-0.124*	-0.215***	-0.143**
	(0.072)	(0.077)	(0.071)
Observations	501	611	611
Dependent Mean (2020)	0.17	0.18	0.18
Panel C: Boarding High Schools			
Post × **Cohort**	-0.199**	-0.263***	-0.178**
	(0.084)	(0.088)	(0.076)
Observations	373	466	441
Dependent Mean (2020)	0.17	0.18	0.17
Controls	Yes	Yes	Yes
Fixed Effects	Province FE	Province FE	Province FE
Clustering	County	County	County

Notes: Dependent variable is an indicator for daily gaming in the past week. Coefficients correspond to the interaction term **Post** × **Cohort** (2022), estimated using linear probability models (OLS). Each column compares the indicated cohort (ages 16, 17, or 18) to 19-year-olds as the baseline group. All regressions include controls for gender, hukou, and parents' education, with province fixed effects. Standard errors (in parentheses) are clustered at the county level. Dependent Mean reports the average daily-gaming participation rate in 2020 for the corresponding subsample. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

A.4.4. Placebo and Minors-Only ICR Analyses

Table D.25: Placebo Test: Internet and Gaming Outcomes among 18–19-Year-Olds

Panel A: Any Internet Usage (LPM)	(1)	(2)	(3)	(4)
***z*-ICR × Post**	-0.015	-0.006	-0.019	-0.012
	(0.008)	(0.013)	(0.011)	(0.015)
z-ICR	0.007	0.005	0.007	0.006
	(0.010)	(0.014)	(0.010)	(0.013)
Post	0.047**		0.047**	
	(0.016)		(0.018)	
Dependent Mean	0.96	0.96	0.96	0.96
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	305	305	305	305
Panel B: Total Daily Internet (minutes, PPML)	(5)	(6)	(7)	(8)
***z*-ICR × Post**	-0.052	-0.079	-0.083	-0.081
	(0.090)	(0.106)	(0.095)	(0.107)
z-ICR	0.014	0.026	0.027	0.023
	(0.052)	(0.058)	(0.057)	(0.063)
Post	0.232**		0.232**	
	(0.097)		(0.097)	
Dependent Mean (2020)	301	301	301	301
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	305	305	305	305
Panel C: Any Gaming (LPM)	(9)	(10)	(11)	(12)
***z*-ICR × Post**	0.039	0.027	0.005	-0.003
	(0.038)	(0.039)	(0.033)	(0.040)
z-ICR	0.030	0.032	0.047**	0.045*
	(0.018)	(0.020)	(0.019)	(0.024)
Post	0.126**		0.126**	
	(0.048)		(0.048)	
Dependent Mean	0.61	0.61	0.61	0.61
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	305	305	305	305
Panel D: Daily Gaming (LPM)	(13)	(14)	(15)	(16)
***z*-ICR × Post**	-0.021	0.004	-0.030	0.006
	(0.035)	(0.030)	(0.037)	(0.035)
z-ICR	0.045**	0.033*	0.029	0.006
	(0.021)	(0.018)	(0.026)	(0.023)
Post	0.072		0.072	
	(0.049)		(0.049)	
Dependent Mean	0.30	0.30	0.30	0.30
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	305	305	305	305

Notes: *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Standard errors in parentheses. This table reports placebo tests using the same difference-in-differences specifications as Table D.26, but restricting the sample to respondents aged 18–19 (adults not subject to the 2021 gaming restriction). Panels A, C, and D are estimated by Ordinary Least Squares (linear probability model), while Panel B uses Poisson Pseudo–Maximum Likelihood (PPML). ***z*-ICR × Post** captures the interaction between standardized county-level internet coverage and the post-policy year. "Demographics" includes gender, hukou, and parents' education. In columns with fixed effects, the *Post* indicator is omitted by construction. None of the interaction coefficients are statistically significant, suggesting no spurious policy effects among adults.

Table D.26: Policy Impact on Internet and Gaming Behaviors among Minors: Difference-in-Differences Estimates (2021 Restriction)

Panel A: Any Internet Usage (LPM)	(1)	(2)	(3)	(4)
z-ICR × Post	-0.003	0.004	0.001	0.008
	(0.008)	(0.010)	(0.010)	(0.011)
z-ICR	0.015	0.009	0.011	0.005
	(0.014)	(0.016)	(0.015)	(0.017)
Post	0.024		0.023	
	(0.018)		(0.018)	
Dependent Mean	0.93	0.93	0.93	0.93
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	384	384	384	384
Panel B: Total Daily Internet (PPML)	(5)	(6)	(7)	(8)
z-ICR × Post	-0.029	0.005	-0.029	0.019
	(0.055)	(0.060)	(0.059)	(0.066)
z-ICR	0.046	0.031	0.032	0.007
	(0.042)	(0.045)	(0.047)	(0.051)
Post	-0.047		-0.053	
	(0.090)		(0.091)	
Dependent Mean (2020)	184	184	184	184
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	384	384	384	384
Panel C: Any Gaming (LPM)	(9)	(10)	(11)	(12)
z-ICR × Post	-0.010	-0.029	-0.019	-0.040
	(0.043)	(0.040)	(0.040)	(0.046)
z-ICR	0.017	0.031	0.026	0.042
	(0.038)	(0.034)	(0.033)	(0.034)
Post	-0.047		-0.070	
	(0.051)		(0.049)	
Dependent Mean	0.49	0.49	0.49	0.49
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	384	384	384	384
Panel D: Daily Gaming (LPM)	(13)	(14)	(15)	(16)
z-ICR × Post	-0.045	-0.048	-0.038	-0.040
	(0.042)	(0.053)	(0.041)	(0.050)
z-ICR	0.015	0.021	0.008	0.015
	(0.047)	(0.055)	(0.045)	(0.051)
Post	-0.031		-0.042	
	(0.044)		(0.042)	
Dependent Mean	0.19	0.19	0.19	0.19
Controls	No	No	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Clustering	County	County	County	County
Observations	384	384	384	384

Notes: *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Standard errors in parentheses. All models use a difference-in-differences design with data from the 2020 and 2022 waves of the CFPS. The estimation sample consists of respondents aged 16–17 in high schools with non-missing county-level internet coverage. Panels A, C, and D are estimated by Ordinary Least Squares (linear probability model), while Panel B is estimated by Poisson Pseudo–Maximum Likelihood. Coefficients in Panel B can be interpreted as semi-elasticities; percentage effects are $100 \times (e^{\beta} - 1)$. **$z$-ICR × Post** captures the interaction between standardized internet coverage and the post-policy year. "Demographics" includes gender, hukou, and parents' education. In columns with fixed effects, the *Post* indicator is omitted by construction.

A.5. Heterogeneity Analyses

A.5.1. Gender Differences in Policy Impacts

Table E.27 examines heterogeneity in policy effects by gender among high school students. Both female and male students experienced similar absolute declines in total internet use after the 2021 restriction—around 60–85 minutes per day. However, because female students had a lower pre-policy baseline, the reduction represents a larger percentage drop for them, indicating a more pronounced proportional contraction in online activity. Gaming outcomes, by contrast, show no significant gender differences, as both groups exhibit small and statistically insignificant changes in participation and daily gaming. Complete regression estimates are reported in Table E.27.

Table E.27: Heterogeneity by Gender: Internet and Gaming Outcomes among High School Students (Ages 16–19)

	(1) Any Internet LPM	(2) Total Internet LPM	(3) Total Internet PPML	(4) Any Gaming LPM	(5) Daily Gaming LPM
Panel A: Female					
Minor × Post	-0.012	-84.620**	-0.295**	-0.114	-0.082
	(0.034)	(34.358)	(0.133)	(0.070)	(0.056)
Minor	-0.009	-0.103	-0.003	0.122**	0.048
	(0.029)	(24.456)	(0.104)	(0.050)	(0.037)
Post	0.064**	93.734***	0.339***	0.079	0.050
	(0.027)	(27.677)	(0.102)	(0.054)	(0.038)
Dependent Mean	0.915	227	227	0.341	0.093
Fixed Effects	Province	Province	Province	Province	Province
Observations	661	661	661	661	661
Panel B: Male					
Minor × Post	-0.075**	-59.261*	-0.228*	-0.085	-0.081
	(0.031)	(35.096)	(0.122)	(0.069)	(0.072)
Minor	0.005	-24.981	-0.089	0.026	-0.032
	(0.024)	(27.581)	(0.094)	(0.052)	(0.052)
Post	0.042**	15.000	0.047	0.094**	0.015
	(0.019)	(23.802)	(0.077)	(0.046)	(0.053)
Dependent Mean	0.942	293	293	0.743	0.374
Fixed Effects	Province	Province	Province	Province	Province
Observations	698	698	698	698	698

Notes: Each column corresponds to a separate regression for the specified outcome, restricted to high school students. Panel A reports results for female students and Panel B for male students. Columns (1), (2), (4), and (5) are estimated using OLS (linear probability model for binary outcomes, and standard OLS for continuous outcomes). Column (3) uses Poisson Pseudo–Maximum Likelihood (PPML), with coefficients in logs; implied minute changes are approximated by $(e^{\beta} - 1) \times$ Dependent Mean. All regressions include controls for hukou and both parents' education, with province fixed effects and county-level clustered standard errors (in parentheses). Dependent means are computed for pre-policy adults (non-minors) within each gender. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

A.5.2. Family Size Differences in Policy Impacts

Table E.28: Heterogeneity by Family Size: Internet and Gaming Outcomes among High School Students (Ages 16–19)

	(1) Any Internet LPM	(2) Total Internet OLS	(3) Total Internet PPML	(4) Any Gaming LPM	(5) Daily Gaming LPM
Panel A: Family Size $\leq$ 4					
Minor × Post	-0.042	-88.523***	-0.311***	-0.106	-0.136*
	(0.032)	(33.225)	(0.112)	(0.066)	(0.076)
Minor	-0.011	-10.672	-0.038	0.065	0.008
	(0.026)	(24.785)	(0.086)	(0.045)	(0.053)
Post	0.049**	53.288**	0.174**	0.093*	0.053
	(0.022)	(26.715)	(0.085)	(0.049)	(0.053)
Dependent Mean	0.932	274	274	0.608	0.267
Fixed Effects	Province	Province	Province	Province	Province
Observations	688	692	692	688	688
Panel B: Family Size > 4					
Minor × Post	-0.042	-33.734	-0.107	-0.083	-0.008
	(0.031)	(35.345)	(0.138)	(0.081)	(0.062)
Minor	0.004	-28.783	-0.128	0.063	-0.003
	(0.026)	(26.379)	(0.110)	(0.058)	(0.044)
Post	0.057**	44.102*	0.153	0.064	-0.009
	(0.025)	(26.016)	(0.094)	(0.058)	(0.048)
Dependent Mean	0.929	219	219	0.563	0.209
Fixed Effects	Province	Province	Province	Province	Province
Observations	672	672	672	672	672

Notes: Each column corresponds to a separate regression for the specified outcome, restricted to high school students aged 16–19. Panel A reports results for students from smaller families (four or fewer members) and Panel B for larger families (more than four). Columns (1), (2), (4), and (5) are estimated using OLS (linear probability model for binary outcomes and standard OLS for continuous outcomes), while column (3) uses Poisson Pseudo–Maximum Likelihood (PPML). All regressions control for gender, hukou, and both parents' education, and include province fixed effects with county-level clustered standard errors in parentheses. Dependent means are computed for *pre-policy adults (non-minors) in 2020* within each family-size group. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

A.5.3. Heterogeneity by Pre-Policy (2020) Family Income

Table E.29: Triple-Difference (DDD) Estimates: Heterogeneity by Baseline Family Income (Standardized 2020)

	(1) Province FE	(2) Province×Year FE
Panel A: Any Internet Usage (LPM)		
Minor × Post × Income$^{std}_{2020}$	0.011	0.005
	(0.018)	(0.019)
Minor × Post	-0.035*	-0.037*
	(0.020)	(0.020)
Dependent Mean (2020)	0.93	0.93
Observations	1,175	1,175
Panel B: Total Daily Internet (minutes, PPML)		
Minor × Post × Income$^{std}_{2020}$	0.048	0.034
	(0.081)	(0.087)
Minor × Post	-77.6***	-77.3***
	(25.5)	(25.6)
Dependent Mean (2020)	248	248
Observations	1,175	1,175
Panel C: Any Gaming (LPM)		
Minor × Post × Income$^{std}_{2020}$	0.073	0.088
	(0.063)	(0.072)
Minor × Post	-0.069	-0.064
	(0.057)	(0.059)
Dependent Mean (2020)	0.59	0.59
Observations	1,175	1,175
Panel D: Daily Gaming (LPM)		
Minor × Post × Income$^{std}_{2020}$	0.087	0.087
	(0.069)	(0.069)
Minor × Post	-0.073	-0.073
	(0.051)	(0.051)
Dependent Mean (2020)	0.24	0.24
Observations	1,175	1,175

Notes: Each column reports coefficients from a triple-difference (DDD) regression of the indicated outcome on Minor×Post×Income$^{std}_{2020}$ and its lower-order terms. Family income is standardized (mean = 0, sd = 1) based on 2020 values within the analytic sample. All regressions control for gender, hukou, and both parents' education; standard errors are clustered at the county level. Panels A, C, and D use OLS (linear probability model), and Panel B employs Poisson pseudo–maximum likelihood (PPML), with coefficients expressed as implied minute changes. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

A.6. Evidence on the 2019 Policy

A.6.1. Internet Outcomes

Table F.30: Main Results: Internet and Gaming Outcomes (2019 Policy)

Panel A: Any Internet Usage	(1)	(2)	(3)	(4)
Minor × Post	-0.006	-0.006	-0.044	-0.045
	(0.016)	(0.016)	(0.030)	(0.031)
Minor	-0.042***	-0.042***	-0.005	-0.006
	(0.012)	(0.012)	(0.024)	(0.025)
Post	0.025***		0.064***	
	(0.009)		(0.022)	
Dependent Mean (2020)	0.93	0.93	0.91	0.91
Observations	2,154	2,154	930	927
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year
Panel B: Total Daily Internet (minutes)	(1)	(2)	(3)	(4)
Minor × Post	-42.4***	-41.9***	-66.1***	-66.9***
	(14.0)	(14.0)	(19.2)	(18.9)
Minor	-40.4***	-40.4***	-15.1	-15.4
	(11.2)	(11.2)	(19.5)	(19.6)
Post	41.6***		50.9**	
	(13.4)		(22.7)	
Dependent Mean (2020)	264	264	242	242
Observations	2,154	2,154	930	927
Sample	Full	Full	High School	High School
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Province	Province×Year	Province	Province×Year

Notes: *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Standard errors clustered at the county level are reported in parentheses. Panel A estimated by OLS (linear probability model); Panel B estimated by Poisson Pseudo–Maximum Likelihood (PPML). For Panel B, both raw PPML coefficients and implied effects in minutes are reported, computed as $(e^{\beta} - 1) \times$ Dependent Mean. "Controls" include gender, hukou, and parents' education. Columns with province×year FE omit the *Post* indicator by construction. Importantly, the 2018 CFPS asked about "weekly leisure internet use" (hours per week), whereas the 2020 CFPS asked about "daily total internet use" from mobile and PC devices (minutes per day). These measures are not strictly comparable; results using 2018 data should therefore be interpreted with caution and are provided for completeness only.

Table F.31: Main Results: Internet and Gaming Outcomes (2019 Policy)

Panel A: Any Internet Usage	(1)	(2)	(3)	(4)
Minor × Post	0.027	0.028	0.027	0.025
	(0.018)	(0.018)	(0.018)	(0.018)
Minor	-0.064***	-0.061***	-0.064***	-0.066***
	(0.014)	(0.013)	(0.014)	(0.014)
Post	0.014	0.013	0.006	
	(0.014)	(0.013)	(0.014)	
Dependent Mean	0.92	0.92	0.92	0.92
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	2,521	2,521	2,521	2,520
Panel B: Total Daily Internet (minutes)	(1)	(2)	(3)	(4)
Minor × Post	0.061	0.054	0.067	0.106
	(0.084)	(0.081)	(0.082)	(0.083)
Implied Effect (minutes)	[11]	[10]	[12]	[20]
Minor	-0.252***	-0.234***	-0.246***	-0.252***
	(0.058)	(0.058)	(0.058)	(0.059)
Post	0.692***	0.705***	0.673***	
	(0.052)	(0.052)	(0.053)	
Dependent Mean	187	187	187	187
Controls	No	No	Yes	Yes
Fixed Effects	None	Province	Province	Province×Year
Observations	2,521	2,521	2,521	2,508

Notes: *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively. Standard errors clustered at the county level are reported in parentheses. Panel A estimated by OLS (linear probability model); Panel B estimated by Poisson Pseudo–Maximum Likelihood (PPML). For Panel B, both raw PPML coefficients and implied effects in minutes are reported, computed as $(e^{\beta} - 1) \times$ Dependent Mean. "Controls" include gender, hukou, and parents' education. Columns with province×year FE omit the *Post* indicator by construction. Importantly, the 2018 CFPS asked about "weekly leisure internet use" (hours per week), whereas the 2020 CFPS asked about "daily total internet use" from mobile and PC devices (minutes per day). These measures are not strictly comparable; results using 2018 data should therefore be interpreted with caution and are provided for completeness only.

A.6.2. Behavioral, Educational, and Health Outcomes

Table F.32: Estimated Effects of the 2019 Policy on Behavioral, Educational, and Health Outcomes of Individuals Aged 16–19 (Province and Year FE)

	(1) Mobile Internet	(2) PC Internet	(3) Internet Minutes	(4) Daily Sleep	(5) Weekday Study	(6) Weekend Study
Minor × Post	0.034*	-0.075*	-25.958	-0.129	-0.009	0.212
	(0.019)	(0.038)	(17.691)	(0.103)	(0.338)	(0.349)
Adjusted p-value	0.515	0.416	0.713	0.792	1.000	0.970
Minor	-0.073***	-0.066**	-28.600***	0.061	0.384*	-0.086
	(0.015)	(0.027)	(7.550)	(0.069)	(0.210)	(0.247)
Post	0.022	-0.050*	153.490***	-0.003	-0.175	0.278
	(0.014)	(0.028)	(13.472)	(0.076)	(0.237)	(0.268)
Observations	2,544	2,544	2,544	2,535	2,083	2,082
R-squared	0.059	0.110	0.165	0.022	0.036	0.023
	(7) Exercise	(8) Extra Books	(9) Total Books	(10) Health	(11) Class Rank	(12) Grade Rank
Minor × Post	0.480**	0.053	1.976	0.290***	0.046	-0.008
	(0.239)	(0.040)	(1.775)	(0.087)	(0.101)	(0.117)
Adjusted p-value	0.386	0.762	0.842	0.020	0.970	1.000
Minor	-0.056	0.047**	0.691	-0.064	-0.033	-0.011
	(0.166)	(0.023)	(0.812)	(0.053)	(0.071)	(0.078)
Post	-0.731***	-0.020	-1.584	-0.064	0.038	0.030
	(0.182)	(0.028)	(1.219)	(0.056)	(0.079)	(0.089)
Observations	2,537	2,538	2,544	2,538	1,615	1,420
R-squared	0.018	0.020	0.041	0.035	0.029	0.037

Notes: The sample includes individuals aged 16 to 19 from the 2018 and 2020 CFPS waves. Minor is an indicator for being under 18. Post is an indicator for the year 2020. Standard errors (in parentheses) are clustered by county. All regressions include province fixed effects. Mobile Internet and PC Internet are indicators for device-based internet use; Internet Minutes is total daily usage. Daily Sleep, Weekday Study, and Weekend Study (hours) measure *daily* time allocation. Exercise is weekly frequency of physical activity. Extra Books is an indicator for reading non-textbook materials, and Total Books is the annual count of such books. Health is a standardized index (mean = 0, sd = 1) where higher values indicate better health. Class Rank and Grade Rank are standardized ordinal measures, with higher values indicating stronger academic performance. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$ based on unadjusted model p-values. Romano-Wolf adjusted p-values are reported separately for **Minor × Post**.

Table F.33: Estimated Effects of the 2019 Policy on Behavioral, Educational, and Health Outcomes of Individuals Aged 16–19 (Province-by-Year FE)

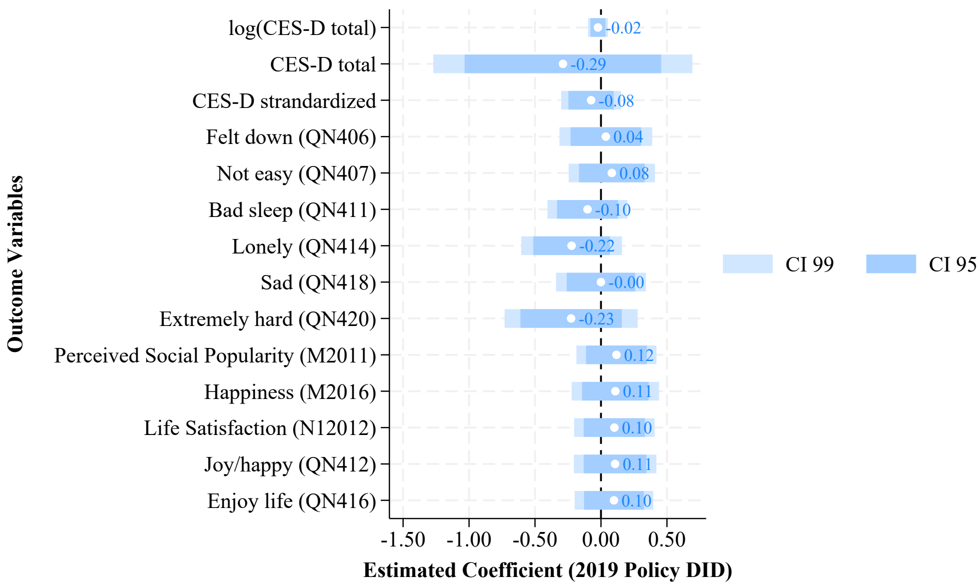
	(1) Mobile Internet	(2) PC Internet	(3) Internet Minutes	(4) Daily Sleep	(5) Weekday Study	(6) Weekend Study
Minor × Post	0.033*	-0.077**	-22.987	-0.149	-0.033	0.158
	(0.019)	(0.038)	(17.732)	(0.105)	(0.345)	(0.354)
Adjusted p-value	0.535	0.416	0.802	0.713	0.990	0.990
Minor	-0.074***	-0.066**	-31.093***	0.073	0.376*	-0.074
	(0.015)	(0.027)	(7.436)	(0.071)	(0.212)	(0.252)
Observations	2,544	2,544	2,544	2,535	2,083	2,082
R-squared	0.069	0.121	0.183	0.034	0.046	0.035
	(7) Exercise	(8) Extra Books	(9) Total Books	(10) Health	(11) Class Rank	(12) Grade Rank
Minor × Post	0.514**	0.055	1.920	0.310***	0.032	-0.016
	(0.246)	(0.040)	(1.638)	(0.087)	(0.103)	(0.119)
Adjusted p-value	0.317	0.782	0.802	0.020	0.990	0.990
Minor	-0.056	0.047**	0.686	-0.065	-0.040	-0.014
	(0.166)	(0.024)	(0.813)	(0.053)	(0.071)	(0.078)
Observations	2,537	2,538	2,544	2,538	1,615	1,420
R-squared	0.025	0.029	0.065	0.048	0.046	0.054

Notes: The sample includes individuals aged 16 to 19 from the 2018 and 2020 CFPS waves. Minor is an indicator for being under 18. Standard errors (in parentheses) are clustered by county. All regressions include province-by-year fixed effects.Mobile Internet and PC Internet are indicators for device-based internet use; Internet Minutes is total daily usage. Daily Sleep, Weekday Study, and Weekend Study (hours) measure *daily* time allocation. Exercise is weekly frequency of physical activity. Extra Books is an indicator for reading non-textbook materials, and Total Books is the annual count of such books. Health is a standardized index (mean = 0, sd = 1) where higher values indicate better health. Class Rank and Grade Rank are standardized ordinal measures, with higher values indicating stronger academic performance. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$ based on unadjusted model p-values. Romano-Wolf adjusted p-values are reported separately for **Minor × Post**.

A.6.3. Mental Health Effects of the 2019 Policy

Figure F.11 plots the DID coefficients for a range of self-reported mental health outcomes, including both negative dimensions (e.g., depressive symptoms, loneliness) and positive dimensions (e.g., perceived social popularity, happiness). For the depressive-symptom variables—such as CES-D total score and CES-D standardized—the estimates are slightly negative, suggesting that the 2019 policy may have reduced (or at least not worsened) minors' depressive-symptom severity. Some negative mental states (e.g., felt down, not easy) show small positive coefficients, whereas others (e.g., bad sleep, lonely, extremely hard) exhibit negative estimates; none, however, appear strongly significant, indicating that random noise and sample size constraints may partly obscure the true effects. Meanwhile, positive attitudes (e.g., perceived social popularity, happiness, life satisfaction, joy/happy, enjoy life) carry uniformly positive point estimates, again hinting that minors might have experienced a modest improvement in overall well-being. Nonetheless, most confidence intervals overlap zero, implying the changes are statistically fragile. In other words, the 2019 policy did not induce large, robust mental health shifts, but it also shows no evidence of harming adolescents' emotional states. Combined, these results suggest at most a mild beneficial effect on well-being, though the small sample and wide confidence intervals limit any firm conclusions.

Figure F.11: Difference-in-Differences Estimates of the 2019 Policy on Mental Health Outcomes



Notes: This figure reports Difference-in-Differences (DID) estimates of the 2019 minor-protection policy on mental health measures. The Center for Epidemiologic Studies Depression Scale (CES-D), originally developed by Radloff (1977), is a widely used self-report measure for depressive symptoms. In this study, I construct the CES-D score using eight survey items from the CFPS (QN406, QN407, QN411, QN412, QN414, QN416, QN418, QN420), which assess key aspects of mood, fatigue, sleep, and life satisfaction. The combined score ranges from 8 to 32, with higher values indicating greater depressive symptoms. To ensure consistency with CFPS coding, I report question numbers as they appear in the original survey. All categorical outcome variables are estimated using ordered probit models, while all regressions include province and year fixed effects, with standard errors clustered at the county level.

A.7. Effects of Daily Internet Minutes on Score Rank

Table G.34 summarizes the estimated effects of daily internet use on academic rank (where a larger rank value indicates better performance). Columns (1) through (4) show OLS estimates under different controls and fixed-effects setups. From a naive perspective, more daily internet time appears associated with lower academic rank, even after accounting for a richer set of controls. However, I do not interpret this correlation as strictly causal, particularly given potential endogeneity concerns (e.g., unobserved student traits or reverse causality). A final observation from Column (5) is that, once I address endogeneity using *minor_policy* as an instrument, the negative OLS association between daily internet minutes and academic rank effectively vanishes. Despite the point estimate flipping sign (from negative to positive), the large standard error leaves the coefficient statistically indistinguishable from zero, suggesting no strong evidence of a causal link in either direction. The first-stage F-statistic of 9.74, while not alarmingly low, indicates only moderate instrument strength, which constrains how definitively I can interpret the 2SLS findings. In short, these results imply that once I isolate the exogenous component of daily internet use—as driven by the minor-specific policy—the data do not support a sizable impact on academic rank, reinforcing that the strong negative correlation in OLS is unlikely to reflect a robust causal mechanism, as it may instead be driven by omitted variables such as intrinsic motivation, parental supervision, or students' underlying academic abilities that simultaneously influence internet use and academic performance.

Table G.34: Effects of Daily Internet Minutes on Score Rank (Larger = Better)

	OLS Estimates				IV (2SLS)
	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	*Score Rank (Larger = Better)*				
Coefficient of total internet minutes	−0.0004293	−0.0003955	−0.0003854	−0.0003046	+0.0013955
(Std. Error)	(0.0001384)	(0.0001623)	(0.0001625)	(0.0001545)	(0.0015703)
p-value	[0.002]	[0.016]	[0.019]	[0.050]	[0.374]
Observations	1,158	1,156	1,145	1,145	1,081
Fixed Effects	None	Prov + Year	Prov + Year	Prov + Year	–
Clustering	None	County	County	County	County
Additional Controls	None	None	Simple Set	Full Set	–
First-Stage F-Stat	–	–	–	–	9.74

Notes: The dependent variable is Score Rank (Larger = Better). Standard errors appear in parentheses, and p-values in brackets. Columns (1)–(4) are OLS; column (5) is 2SLS, instrumenting daily internet minutes with minor policy. Columns (2)–(4) include province and year fixed effects. County-level clustering applies in columns (3)–(5). "Simple Set" controls include minor, gender, and hukou; "Full Set" adds family size, parents' education, and household income. The reported first-stage F-statistic in column (5) assesses instrument relevance. Negative coefficients imply more internet use correlates with worse rank.

A.8. Comparison with the Binzhou RK Design: CFPS Senior Subsample

To facilitate comparison with the regression kink (RK) design using the Binzhou administrative data, Appendix Table H.35 restricts the CFPS sample to senior high school students (i.e., those preparing for the college entrance examination) observed in 2020 and 2022. This ensures that both datasets focus on the same educational stage and policy exposure period. Consistent with the Binzhou results, the difference-in-differences estimates show that the 2021 restriction substantially reduced gaming participation among senior high school students, with no statistically significant changes in overall Internet use. These findings suggest that the decline in gaming activity documented in the CFPS survey data aligns closely with the behavioral adjustments observed in the administrative exam-based sample.

Table H.35: DID Estimates among Senior High School Students (CFPS 2020–2022), for Comparison with the Binzhou RK Design

	(1) Province FE	(2) Province×Year FE
Panel A: Any Internet Usage		
Minor × Post	-0.054	-0.032
	(0.075)	(0.077)
Minor	-0.033	-0.038
	(0.053)	(0.055)
Post	0.047**	
	(0.024)	
Dependent Mean (2020)	0.98	0.98
Observations	447	447
R-squared	0.099	0.152
Panel B: Total Daily Internet (minutes)		
Minor × Post	-0.162	-0.124
	(0.234)	(0.223)
Minor	-0.303	-0.305
	(0.193)	(0.175)
Post	0.172**	
	(0.070)	
Dependent Mean (2020)	547	548
Observations	447	447
Pseudo R^2	0.157	0.187
Panel C: Any Gaming		
Minor × Post	-0.327**	-0.288**
	(0.137)	(0.141)
Minor	0.150	0.127
	(0.105)	(0.112)
Post	0.103**	
	(0.045)	
Dependent Mean (2020)	0.52	0.49
Observations	447	447
R-squared	0.251	0.274
Panel D: Daily Gaming		
Minor × Post	-0.217**	-0.225**
	(0.105)	(0.105)
Minor	0.072	0.089
	(0.080)	(0.082)
Post	0.083**	
	(0.042)	
Dependent Mean (2020)	0.16	0.14
Observations	447	447
R-squared	0.216	0.244

Notes: Each column represents a separate regression restricted to Grade-12 high school students (ages 16–19) observed in 2020 and 2022. All regressions control for gender, hukou, and both parents' education, with standard errors clustered at the county level. Panels A, C, and D are estimated using OLS (linear probability model). Panel B is estimated using Poisson Pseudo–Maximum Likelihood (PPML), with coefficients interpreted as semi-elasticities relative to the dependent mean. Column (1) includes province fixed effects; Column (2) includes province-by-year fixed effects. *, **, and *** denote $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

A.9. Regression Discontinuity and Kink Analyses

A.9.1. Regression Discontinuity Results

Table I.36 reports the RD estimates for the entire sample of seven counties, comparing first-order (Panel A) and second-order (Panel B) local polynomials. In each panel, Columns (1) and (2) employ the MSE-optimal bandwidth selection procedure, while Column (3) implements a manually specified bandwidth. In general, a first-order polynomial is recommended in canonical RD applications, and here the corresponding point estimates are small and statistically insignificant, suggesting that any immediate effect of additional gaming near age 18 is negligible. Although the discrete nature of the running variable (age) can pose challenges, the modest but sufficient counts of unique observations (Unique Obs) help to ensure that a local polynomial RD analysis remains feasible for these data.

Table I.37 presents analogous RD estimates for Bincheng County, which has a higher Internet coverage rate and thus might exhibit different effects. Panel C employs a first-order local polynomial ($p = 1$), while Panel D uses a second-order polynomial ($p = 2$). As in the full-sample analysis, Columns (1) and (2) rely on MSE-optimal bandwidth selection, whereas Column (3) specifies a manual bandwidth. Although first-order estimates yield somewhat larger point estimates, none of the discontinuities is robustly significant. Note also that standard errors are not clustered at the school level because the number of schools in Bincheng County is relatively small. Nonetheless, the modest but sufficient number of unique birthdates still supports a local polynomial RD approach in this narrower setting.

Table I.36: Estimate of Discontinuity (RD) under Different Bandwidth Choices

Panel A: Polynomial Order ($p = 1$)			
	(1)	**(2)**	**(3)**
Model Description	$[17, 19]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `manual` $h(0.62, 0.38)$
Estimate of Discontinuity (RD)			
Point Estimate	0.048	0.040	0.042
(Std. Dev.)	(0.154)	(0.154)	(0.150)
Kernel Function	Triangular	Triangular	Triangular
Polynomial Order (p)	1	1	1
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.292/0.383	0.306/0.143	0.620/0.380
Effective Obs (Left)	5,292	5,525	10,062
Effective Obs (Right)	6,350	2,238	6,339
Unique Obs (Left)	336	222	222
Unique Obs (Right)	357	137	137
Clustered at School Level	Yes	Yes	Yes
Geographical Coverage	7 Counties	7 Counties	7 Counties

Panel B: Polynomial Order ($p = 2$)			
	(1)	**(2)**	**(3)**
Model Description	$[17, 19]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `manual` $h(0.62, 0.38)$
Estimate of Discontinuity (RD)			
Point Estimate	0.028	0.067	0.047
(Std. Dev.)	(0.160)	(0.169)	(0.155)
Kernel Function	Triangular	Triangular	Triangular
Polynomial Order (p)	2	2	2
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.399/0.356	0.357/0.152	0.620/0.380
Effective Obs (Left)	6,803	6,229	10,062
Effective Obs (Right)	6,033	2,339	6,339
Unique Obs (Left)	336	222	222
Unique Obs (Right)	357	137	137
Clustered at School Level	Yes	Yes	Yes
Geographical Coverage	7 Counties	7 Counties	7 Counties

Notes: All columns report sharp RD estimates with a triangular kernel and cluster-robust standard errors at the school level. Panel A employs a first-order local polynomial ($p = 1$), whereas Panel B uses a second-order local polynomial ($p = 2$). Columns (1) and (2) use the MSE-optimal bandwidth , while Column (3) relies on a manually chosen bandwidth $\left[h(0.62, 0.38)\right]$. The first row in each Estimate of Discontinuity block is the conventional point estimate, and the second row (in parentheses) is the conventional standard error.Unique Obs (Left/Right) refers to the count of unique mass points below/above the cutoff. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table I.37: Estimate of Discontinuity (RD) under Different Bandwidth Choices (Bincheng County)

Panel C: Polynomial Order ($p = 1$)			
	(1)	**(2)**	**(3)**
Model Description	$[17, 19]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `manual` $h(0.62, 0.38)$
Estimate of Discontinuity (RD)			
Point Estimate	0.092	0.140	0.164**
(Std. Error)	(0.111)	(0.146)	(0.074)
Kernel Function	Triangular	Triangular	Triangular
Polynomial Order (p)	1	1	1
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.161/0.292	0.151/0.103	0.620/0.380
Effective Obs (Left)	707	667	2,336
Effective Obs (Right)	984	320	1,245
Unique Obs (Left)	285	222	222
Unique Obs (Right)	270	137	137
Clustered at School Level	No	No	No
Geographical Coverage	BC County	BC County	BC County

Panel D: Polynomial Order ($p = 2$)			
	(1)	**(2)**	**(3)**
Model Description	$[17, 19]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `Optimal Bandwidth`	$[17.38, 18.38]$, `manual` $h(0.62, 0.38)$
Estimate of Discontinuity (RD)			
Point Estimate	0.060	0.147	0.189*
(Std. Error)	(0.137)	(0.171)	(0.108)
Kernel Function	Triangular	Triangular	Triangular
Polynomial Order (p)	2	2	2
Bandwidth Choice	MSE	MSE	Manual
BW est. (h)	0.251/0.387	0.265/0.144	0.620/0.380
Effective Obs (Left)	1,067	1,116	2,336
Effective Obs (Right)	1,249	452	1,245
Unique Obs (Left)	285	222	222
Unique Obs (Right)	270	137	137
Clustered at School Level	No	No	No
Geographical Coverage	BC County	BC County	BC County

Notes: All columns report Regression Discontinuity (RD) estimates on Bincheng (BC) County only. Panel C uses polynomial order $p = 1$, while Panel D uses $p = 2$. A triangular kernel is used throughout. Columns (1) and (2) apply the MSE-optimal bandwidth, and column (3) adopts a manually specified bandwidth of $h(0.62, 0.38)$. The first row in each Estimate of Discontinuity block is the point estimate, followed in parentheses by the conventional standard error. Unique Obs (Left/Right) denotes the number of unique mass points for individuals below/above the cutoff. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.9.2. Regression Kink Robustness Checks

Table I.38: Robustness of Piecewise Regression Results (7 Counties)

	(1)	(2)	(3)	(4)
β_1: Slope before age 18	-0.161***	-0.162***	-0.145***	-0.141***
	(0.051)	(0.051)	(0.036)	(0.037)
β_2: Slope for (18,18.38)	-0.444***	-0.449***	-0.342***	-0.355***
	(0.117)	(0.117)	(0.086)	(0.086)
β_3: Slope after 18.38	0.121	0.128	0.072	0.048
	(0.138)	(0.138)	(0.131)	(0.120)
γ_1: Jump at Age 18	0.056*	0.057*	0.039	0.025
	(0.032)	(0.032)	(0.024)	(0.024)
γ_2: Jump at Age 18.38	0.023	0.025	0.042	0.051
	(0.054)	(0.054)	(0.036)	(0.036)
Female indicator		0.041***	0.001	-0.003
		(0.015)	(0.018)	(0.018)
Constant	2.878***	2.871***	2.584***	1.021
	(0.897)	(0.897)	(0.630)	(0.813)
School FE	No	No	Yes	Yes
Birth-County FE	No	No	No	Yes
Observations	18,125	18,125	18,125	18,125
R^2	0.0031	0.0035	0.323	0.338

Notes: The dependent variable is standardized major subject scores. Columns (1)–(2) use robust standard errors. Columns (3)–(4) include school fixed effects and cluster at the school level; column (4) further adds birth-county fixed effects (based on students' place of birth). β_1–β_3 denote slopes in different age ranges; γ_1 and γ_2 denote jumps at ages 18 and 18.38, respectively. Stars denote significance at * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table I.39: Robustness of Piecewise Regression Results (BC County)

	(1)	(2)	(3)	(4)
β_1: Slope before age 18	-0.333***	-0.338***	-0.133***	-0.117***
	(0.104)	(0.104)	(0.037)	(0.026)
β_2: Slope for (18,18.38)	-0.899***	-0.908***	-0.458***	-0.433***
	(0.260)	(0.260)	(0.079)	(0.058)
β_3: Slope after 18.38	0.378	0.382	0.890**	0.588**
	(0.354)	(0.353)	(0.345)	(0.252)
γ_1: Jump at Age 18	0.153**	0.155**	0.062***	0.062**
	(0.068)	(0.068)	(0.018)	(0.022)
γ_2: Jump at Age 18.38	-0.163	-0.155	-0.156	-0.091
	(0.134)	(0.133)	(0.095)	(0.105)
Female indicator		0.051	-0.015	-0.016
		(0.032)	(0.037)	(0.032)
Constant	6.117***	6.173***	2.551***	0.797
	(1.833)	(1.833)	(0.660)	(0.522)
School FE	No	No	Yes	Yes
Birth-County FE	No	No	No	Yes
Observations	3,896	3,896	3,896	3,896
R^2	0.014	0.014	0.498	0.525

Notes: The dependent variable is standardized major subject scores. Columns (1)–(2) use robust standard errors. Columns (3)–(4) include school fixed effects and cluster at the school level; column (4) further adds birth-county fixed effects (based on students' place of birth). β_1–β_3 denote slopes in different age ranges; γ_1 and γ_2 denote jumps at ages 18 and 18.38, respectively. Stars denote significance at * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.10. Supplementary Material for Binzhou Dataset

A.10.1. Supplementary Summary Statistics: Mock Exam Data

Table J.40: Summary Statistics: Mock University Entrance Exam (Binzhou City, 2021)

VARIABLES	N	Mean	SD	Min	Max
Scores					
Total score (C+M+E+3 electives)	19,203	364.3	81.5	2	656
Main score (C+M+E)	19,203	228.6	50.9	2	376
Compulsory subjects					
Chinese	19,203	96.5	11.5	2	129
Math	19,203	70.9	25.7	4	150
English	19,203	62.0	20.4	3	114
Elective subjects					
Physics	6,913	52.0	17.0	3	97
Chemistry	8,147	49.4	16.0	2	98
Biology	13,323	41.2	12.8	6	94
Politics	7,545	50.6	10.6	9	78
History	8,929	45.2	9.7	4	79
Geography	12,438	41.3	9.0	6	74
Running variable					
Age	19,203	18.0	0.46	15.2	20.9
Adult (=1 if age $\geq$ 18)	19,203	0.44	0.50	0	1
Individual control					
Male	19,203	0.54	0.50	0	1
County-level statistics					
Internet account holders	7	231,898	101,372	105,751	392,367
Population	7	554,319	115,651	399,482	746,309
Internet coverage rate	7	0.42	0.18	0.26	0.76
Disposable income per capita	7	29,334	3,085	24,827	33,459
Number of schools	7	5.39	1.34	3	7
Number of teachers	7	931.6	253.2	544	1,282

Note: The number of observations for each subject differs because some students were absent and elective course choices varied. County-level statistics are aggregated at the county-year level (N=7 counties).
Source: Binzhou City Education Bureau.

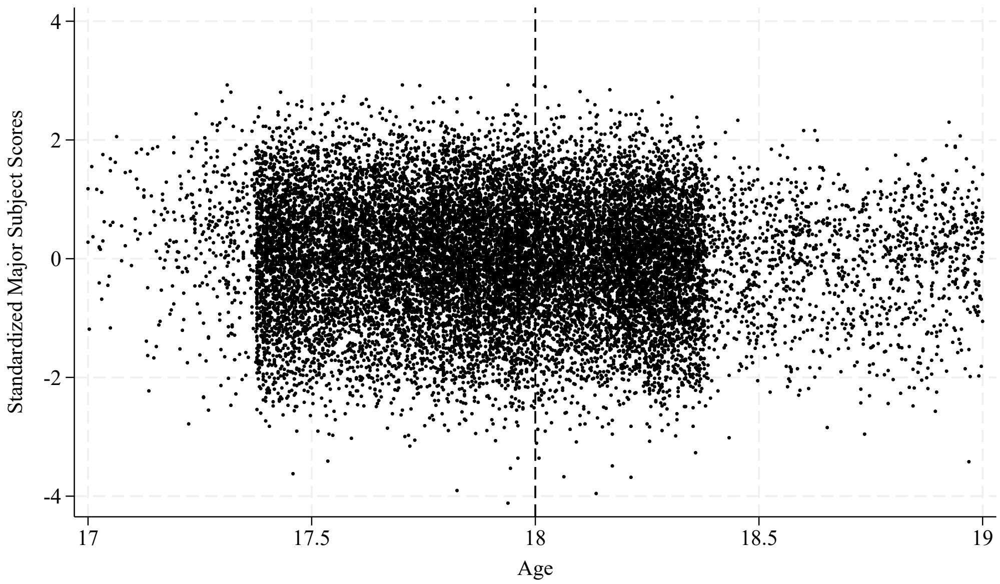


Figure J.12: Scatterplot of Standardized Major Subject Scores by Age

Notes: This figure presents the raw relationship between age (x-axis, 17 to 19 years) and standardized major subject scores (y-axis, mean zero and unit standard deviation) for the mock exam sample. Each dot represents an individual student's score. The vertical dashed line at age 18 marks the policy cutoff for adulthood. The scatterplot suggests no immediate visual jump in scores at the cutoff, supporting the need for a formal regression discontinuity (RD) design analysis to uncover any causal impact.

A.10.2. Illustration of Duration Calculation

Table J.41: Illustration of Duration Calculation (Policy from 2021-09-01 to 2022-01-17)

ID Number	**Birth Date**	**Age**	**No Restriction Days** *(since 09/01/2021)*	**Expected Gaming Hours** *(since 09/01/2021)*
20050117	2005-01-17	17.0000	0	59.77
...	...	...	...	...
20040118	2004-01-18	17.9973	0	59.77
20040117	2004-01-17	18.0000	1	60.60
20040116	2004-01-16	18.0027	2	61.37
...	...	...	...	...
20030901	2003-09-01	18.3800	139	170.98
...	...	...	...	...
20030117	2003-01-17	19.0000	139	170.98

Notes: *Age* is the exact age (in decimal years) as of 2022-01-17; for example, 17.9973 implies about 17 years, 364 days. *No Restriction Days* is the number of days each individual qualifies for unrestricted gaming hours (1.23 hours/day) after turning 18, up to 2022-01-17. Those turning 18 *after* 2022-01-17 have zero *No Restriction Days*, while turning 18 on 2022-01-17 yields one day, etc. *Expected Gaming Hours* is a naive calculation based on combining the average time from the pre-2021 regime (1.23 hours/day) with the new policy (0.43 hours/day).

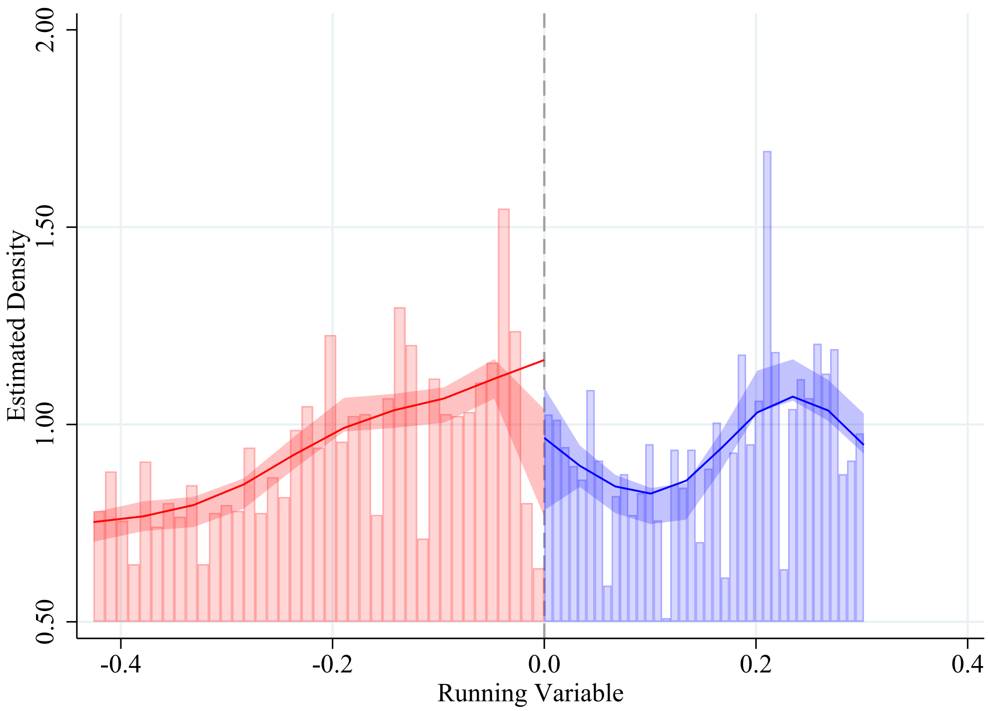
A.10.3. Data Processing and Validity Discussion

In this subsection, I provide a detailed explanation of the data processing procedures applied to the Mock College Entrance Exam dataset. This elaboration encompasses the absence of manipulation from multiple perspectives, including data collection, data processing, and the true data-generating process. This data set covers the entire population of high school seniors within a city of prefecture level, constituting a population-level representation rather than a mere sample. Furthermore, the database contains precise birth date information for all individuals derived from individual resident identity cards rather than self-reported birthdays. By the "Resident Identity Card Act" of China, citizens who have reached the age of 16 are required to apply for a resident identity card. Children under the age of 16 years may apply voluntarily. In practice, every high school senior student possesses resident identity cards to take the University Entrance Exam. Within the main body of the text, my data preprocessing efforts encompass solely two steps: 1. Exclusion of individuals who did not participate in the exam; 2. removal of individuals aged over 20 or under 16. To my knowledge, no data manipulation has been undertaken during the data processing phase or in the data collection process. Moreover, to my understanding, no mechanisms for manipulation have been identified from the real data-generating process perspective. For instance, one hypothetical mechanism is as follows: among regular high school seniors, due to an increase in age, students might bear more familial responsibilities, which could lead to increased dropout rates or absenteeism from work. The next paragraph will present evidence to contradict the mechanism. In addition, legally defined age restrictions on prohibited behaviors are closely associated with 18-year-olds. China does not have a prescribed legal drinking age. The legal age for marriage is set at a minimum of 22 years for men and 20 years for women. Additionally, the minimum legal working age is 16 years.

Although the primary identification in a Regression Kink (RK) design hinges on changes in slopes rather than jumps in levels, it remains prudent to examine whether there is any bunching or discontinuity in the distribution of the running variable at the cutoff point. The McCrary test, introduced by McCrary (2008), is well known to diagnose such potential manipulation in canonical Regression Discontinuity (RD) settings by evaluating whether the density of the running variable is continuous at the threshold. Here, I adapt a related local-polynomial density approach following Cattaneo et al. (2020, 2024b), which does not require pre-binning and tends to exhibit favorable size and power properties. The null hypothesis is that the density of the running variable, considered separately for observations above and below the cutoff, is continuous at that point.

In this application, the test yields a robust local-polynomial statistic of approximately -0.1302 (p-value ≈ 0.8964), indicating that there are no statistically significant differences in density on either side of the cutoff. Figure J.13 visualizes these findings, showing that the estimated density reveals no meaningful jump at `age_run` $= 0$. Consequently, there is little evidence of strategic manipulation, such as systematically reporting birthdates just above or below the cut-off, which bolsters the continuity assumption central to both Regression Discontinuity (RD) and Regression Kink (RK) analyses. In essence, the data do not show a salient violation of the "no manipulation" condition, helping ensure that subsequent causal inferences regarding policy effects remain credible.

Figure J.13: Histogram and Estimated Density of the Running Variable



Notes: This figure plots the histogram and a local-polynomial density estimate of the running variable age_run centered at zero (i.e., $age - 18$). I employ the `rddensity` command Cattaneo et al. (2020) with its default specification: a triangular kernel, local-polynomial order $p = 2$, and a jackknife variance estimator. The vertical dashed line at 0 highlights the cutoff (age 18). The resulting test statistic (not shown) does not suggest a significant discontinuity at the cutoff, reinforcing the continuity assumption for the running variable. The background histogram helps visualize how observations are distributed, with no strong signs of bunching around the cutoff.

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