School Entry Age Policy and Adolescent Risk–Taking

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March 24, 2025

Abstract

This paper analyzes the impact of school entry age policy on adolescent risk–taking behaviors. In Spain, children must begin primary education in the year they turn six, with a January 1^{st} cutoff date, leading to relative age differences within each academic cohort. Using data from the Spanish School Survey on Drug Use, we analyze a broad range of risky behaviors, including substance use, gambling, gaming, internet use, and sexual activity among students in compulsory education. By comparing students born just before and after the cutoff date, we find that younger students (born below the cutoff) are less likely to engage in risky behaviors compared to older students (born above the cutoff) in the same birth cohort. These results hold across various robustness checks, including using different bandwidths. Further analysis suggests that differences in absolute age—reflecting differences in maturity—and the educational cycle contribute to these findings. When controlling for age differences, young–for–grade appear more likely to smoke marijuana and tobacco, and use internet compulsively. These results are primarily driven by boys. Additional exploration suggests that most behavioral differences fade out by late adolescence in high school. This research broadens our understanding of the non–academic impacts of school entry age policies contributing to the literature on education policy and adolescent development.

Keywords: risky–health behaviors; age disparities; young–for–grade students; old–for–grade students; education policy

JEL codes: I12, I21, J13

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1 Introduction

School systems typically determine a child's school entry based on a cutoff date, which varies across countries but invariably creates age disparities within each academic cohort. Children born before and after the cutoff can differ in age by up to a year, leading to variations in cognitive development and maturity among cohort–mates. Research consistently shows that these age disparities have significant implications for academic performance. Compared to older students in the cohort, younger students have lower test scores (Bedard and Dhuey, 2006; Sprietsma, 2010; Crawford et al., 2014; Peña, 2017; Dhuey et al., 2019) and suffer higher retention rates (Manacorda, 2012; Bedard and Dhuey, 2006; Jerrim et al., 2022). Peña (2020) finds that most of the academic gap is explained by relative age differences at the time of the test. Evidence on longer–term effects on human capital development is mixed (Dhuey and Koebel, 2022). Some studies find that the educational disadvantage of younger students fades out over time, with no impact on college graduation rates (Oosterbeek et al., 2021) or adult labor market outcomes, such as wages (Dobkin and Ferreira, 2010). In contrast, Bedard and Dhuey (2006) show that this academic disadvantage persists and reduces university attendance among younger students.

The age disparities resulting from school entry cutoffs likely have implications beyond academic performance. However, while extensive research has examined their effects on education, studies on non–academic impacts remain relatively scarce. Some studies have focused on attention deficit and hyperactivity disorder (ADHD), finding that younger students systematically receive higher ADHD diagnoses (Elder (2010) and Nicodemo et al. (2024) for UK; Layton et al. (2018) for USA; Schwandt and Wuppermann (2016) for Germany). Other studies have explored the effects on psychological traits, showing that older students have greater confidence in their scholastic competence (Crawford et al., 2014), are more likely to assume leadership roles in high school (Dhuey and Lipscomb, 2008), and exhibit lower levels of neuroticism in adulthood (Barabasch et al., 2024). Among non–academic outcomes, health–risk behaviors——such as substance use——are of particular concern due to their serious long–term consequences.[1] Risky behaviors often initiate in adolescence, raising the likelihood of addiction and substance abuse in adulthood (Schulte and Hser, 2014).[2] Understanding how cutoff–induced age differences influence adolescents' engagement in risky behaviors is therefore crucial. However, this topic remains relatively underexplored. The few existing studies find that younger students are more likely to consume alcohol, tobacco and marijuana, but less likely to engage in sexual activity (Argys and Rees, 2008; Johansen, 2021; Shin, 2023; Fumarco and Principe, 2024). Johansen (2021) also finds that among the youngest in a cohort, women have a higher probability of abortion, childbirth, and cohabitation by their early 20s, while no such effects are observed for men. Similarly, Argys and Rees (2008) report that

[1] Cawley and Ruhm (2011) documents the negative effects of alcohol and drug use on earnings, employment, and educational attainment, as well as their association with increased criminal activity.

[2] In Spain, the percentage of students who have used alcohol, tobacco, e–cigarettes, and cannabis by age 13 is 31%, 16%, 11%, and 3.4%, respectively; the European average is 33%, 18%, 11%, and 2.4%, respectively (ESPAD, 2020). By age 16, prevalence rates rise substantially (see Table A.1 in Appendix A).

younger girls are more prone to substance use than their grade–level peers, while no significant difference is found for boys.

In this study, we analyze whether the Spanish school entry age policy and the age disparities it creates cause differences in risky behavior adoption in adolescence. We consider a broad set of behaviors, including tobacco, alcohol and marijuana use, gambling, gaming, non–prescribed tranquilizers' use, vaping, sexual activity, and internet use. In Spain, the school entry age policy establishes that children must start the first grade of primary education—the first compulsory schooling level—in September of the year they turn 6. The cutoff date is thus January 1^{st}. This means that a student born on December 31^{st} must start school one year earlier than a student born just one day later, on January 1^{st}. The cutoff date, thus, induces variation in the relative age of students who commence school simultaneously. For instance, students born in December have not yet reached 6 years of age when they start school in September, while those born in January are over 6 years old. Hereafter, we designate students born before the cutoff (i.e., in December or earlier) as young–for–grade students, and those born after the cutoff (i.e., in January or later) as old–for–grade students.

Several factors may explain why young–for–grade and old–for–grade may differ in their likelihood of adopting risky behaviors. As discussed above, young–for–grade students tend to have poorer academic performance, which may reduce their learning engagement and increase their propensity to adopt risky behaviors. Additionally, young–for–grade may be more likely to adopt such behaviors if they are more vulnerable to the influence of relatively old students (Argys and Rees, 2008). On the other hand, young–for–grade tend to have a lower cognitive and maturity development when they enter school, which may reduce per se their probability of engaging in risky behaviors. Moreover, old–for–grade may socially outcast young–for–grade due to being less mature. Relatedly to the lower maturity, young–for–grade may be subject to a higher parental control, which reduces their chances of adopting risky behaviors. Finally, differences in the schooling cycle—young–for–grade enter the schooling cycle a year earlier than old–for–grade—may also explain differences in the probability of risk–taking (Johansen, 2021). While exploring the underlying mechanisms is not the primary focus of our study, we estimate various specifications to gain deeper insights into our results and discuss potential factors that may be behind them.

This study expands the existing literature on the effects of school entry age policies on risky behaviors in several ways. First, we analyze outcomes that have not yet been examined, such as gambling, gaming, non–prescribed tranquilizers' use, vaping and internet use. As explained above, previous literature has mainly focused on the use of alcohol, tobacco and marijuana, and sexual activity. Second, we provide additional evidence to the still sparse research on those previously analyzed behaviors. Finally, by analyzing the Spanish policy, we broaden the geographical scope of research on this topic to an unexamined country. Previous studies provide evidence on some risky behaviors for South Korea (Shin, 2023), Denmark (Johansen, 2021), or USA (Argys and Rees, 2008). This study offers novel insights into the interaction of risky behavior adoption and the entry

policy in a social context different from the ones formerly analyzed. Spain is characterized by a strong emphasis on socialization and close family ties, contrasting with the more individualistic cultures of USA and Denmark. While South Korea shares collectivist values with Spain, its focus leans more toward academic pressure and conformity, whereas Spanish culture embraces a more relaxed approach to leisure and social interactions. Additionally, the Spanish education system stands out for its high rates of grade retention, a practice that is relatively uncommon in Denmark and South Korea and less prevalent in the United States. Overall, our study contributes to a more comprehensive understanding of how school entry age policies affect outcomes beyond academic performance, the primary focus of existing studies. This expanded perspective is crucial for policymakers considering adjustments to school entry age regulations or remedial measures aimed at addressing cognitive performance gaps created by the cutoff date.

We use the Spanish School Survey on Drug Use (SSSDU), a nationally representative school–based survey on drug consumption and other risky behaviors conducted by the Ministry of Health. SSSDU surveys adolescents who are enrolled in the last two grades of lower secondary (compulsory) education and in upper secondary (post–compulsory) education. The students' expected age at these grades ranges from 15 to 18 years old.

Our empirical strategy compares the adoption of risky behaviors of students born just before and after the cutoff date. A key feature of the Spanish educational system that ensures the comparability of these two groups is the prohibition to advance or delay a child's school entry. This effectively eliminates the selection bias related to the timing of school entry, which could otherwise arise from parental preferences, adjustment to compensate for a child's developmental differences, or other unobserved factors. In line with this, results from the balance tests do not show significant differences in a wide range of observable characteristics of students born around the cutoff. In our baseline specification, we consider a one–month bandwidth and estimate by OLS the effect of being young–for–grade on the adoption of risky behaviors using the group of students born in January and December. As a robustness check, we use a two– and three–month bandwidth for the estimation of this effect, and, additionally, employ a Regression Discontinuity Design (RDD) estimation with a three–month bandwidth.

In our main analysis, we focus on examining the effect of the school entry cutoff in risk–taking behaviors among students in the final grades of lower secondary education, corresponding to early adolescence (age 15–16). We find that young–for–grade students are less likely to engage in risky behaviors compared to old–for–grade students across most outcomes: gambling, drinking alcohol, smoking tobacco and marijuana, vaping, visiting adult websites and risky sexual activity. These findings are robust to various sensitivity checks, such as dropping socio–demographic controls (indicating randomness around the cutoff date), using different bandwidths, and applying an RDD estimation strategy. In a separate section, we also provide evidence of longer–term effects by extending the analysis to students enrolled in high school (post–compulsory, age 17–18). The reason to focus primarily on compulsory education is that findings from students enrolled in high–

school may be affected by potential positive selection, as early–school leavers are not surveyed. Bearing this issue in mind, results suggest that most significant differences in risky behaviors between young– and old–for grade observed at the early adolescence in compulsory education disappear in high school, at late adolescence.

An important aspect of the Spanish education system relevant to our analysis is the widespread use of grade retention for students with poor academic performance. Spain has one of the highest retention rates among OECD countries, with approximately 30% of students held back during their schooling (OECD, 2020). These retention decisions primarily occur in lower secondary education, causing affected students to no longer progress with their original cohort and, instead, join the cohort that started a year later. Consequently, retention disrupts the one–to–one correspondence between birth cohort and grade level for these students.

To disentangle the extent to which differences in absolute age—i.e., birth cohort—and differences in the schooling cycle—i.e., grade level—may contribute to the overall young–for–grade effect in adolescents' risk–taking in compulsory education, we estimate additional specifications. First, we examine differences in absolute age by estimating the effect separately by the birth cohorts enrolled at the surveyed grades. Second, we consider differences in the schooling cycle by conducting separate estimations for each grade. The evidence suggests that the overall lower propensity of young–for–grade students to engage in risky behaviors is primarily driven by differences in absolute age, which may reflect differences in maturity. To gain more insights on the role of maturity, we use the available survey information on family rules around social behavior and drug consumption. These rules are significantly stricter for young–for–grade students. Adding these variables to the specifications reduces the estimated effect of being young–for–grade on risk–taking, further supporting maturity as a key mechanism for the results.

We additionally estimate a third specification that compares students of roughly the same age who entered school in different years due to the cutoff. Given the survey characteristics, there is only one viable comparison for this approach in compulsory education (students born in December 2003 and in January 2004). Findings show that young–for–grade are more likely to engage in certain behaviors, namely smoking tobacco and marijuana, and using internet compulsively. Note that the overall young–for–grade takes the opposite sign. Therefore, evidence from the adjacent cohort analysis indicates that, if differences in age are controlled for, young–for–grade are more likely to adopt risky behaviors, a finding in line with previous literature. These results additionally support that the overall negative effects are primarily driven by differences in maturity, while also highlighting the role of the schooling cycle in shaping certain risky behaviors.

Finally, we explore heterogeneous effects between girls and boys, and between public and private schools. Findings tend to reproduce the overall patterns but with some differences in the impact of being young–for–grade on risky behaviors by gender and, to a lesser extent, by school type.

The rest of the paper is organized as follows. Section 2 presents the school entry age policy

in Spain and the data used. Section 3 explains the empirical strategy. Section 4 presents the main estimation results, a separate analysis by gender and type of school, a series of robustness checks, and the analysis using the high school sample; it also discusses some possible mechanisms underlying the main findings. Finally, section 5 concludes.

2 Institutional setting and data

2.1 School entry age policy in Spain

In Spain, since 1990, compulsory education begins at age 6 and finishes at the age of 16.[3] The educational system is shown in Figure 1 and is structured as follows: primary education spans six grades (ages 6 to 12), followed by lower secondary education, which comprises four grades (ages 13 to 16). After completing compulsory schooling, students have two options in upper secondary education. One is high school, which is the pathway to university and consists of two grades (ages 17 and 18). Alternatively, a vocational degree, which provides school–based technical education combined with workplace training, lasting one or two grades depending on the specific degree chosen.

The Spanish school entry age policy mandates that children commence the first grade of primary education in the year they turn 6 years old, adhering to a birth year rule. The policy establishes January 1^{st} as the cutoff date. Consequently, students born after this cutoff begin their schooling one year later than those born before it. Figure 2 illustrates the workings of this policy and the age disparities it creates. A child born early in the calendar year, "Child 1" in the figure, is over 6 years old when she/he enters school in mid–September. Conversely, a child born late in the year, "Child 2", is not yet 6 at school entry. Moreover, as the figure illustrates, two children born just one day apart on either side of the cutoff date, "Child 2" and "Child 3", enter school in different years, with Child 3 starting a year later than Child 2. Despite these age disparities, Spanish policy prohibits parents from advancing or delaying a child's school entry (*greenshirting* and *redshirting* practices). This policy is strictly applicable in all Spanish regions, ensuring a consistent implementation of the birth year rule throughout the country.

In Spain, while not compulsory, pre–school education is widely embraced by parents for children aged 3 to 5. This popularity stems from several factors: it is free and publicly provided, most schools offer both pre–school and primary education in the same facility, and enrollment in a school's pre–school often guarantees a spot in its primary program. Consequently, parents frequently enroll their children in pre–school to secure their future primary education placement. In practice, this implies advancing schooling by three years without creating relevant differences in the educational cycle since more than 94% of the 3–year–old children are enrolled at pre–school, as shown by figure E2.1 in the 2023 report of the Spanish Ministry of Education.[4] The massive

[3] Organic Law 1/1990, 3^{rd} October, "Ordenación General del Sistema Educativo" (LOGSE) `https://www.boe.es/eli/es/lo/1990/10/03/1`.

[4] The report "Sistema estatal de indicadores de la educación 2023", Ministerio de Educación

enrollment in pre–school in Spain and the extremely high compliance rate with the birth year rule is also documented in Berniell and Estrada (2020). Importantly, pre–school follows the same starting age policy and academic calendar as primary education. Children begin pre–school in September of the year they turn 3, and like primary education, redshirting and greenshirting are not permitted. The January 1^{st} cutoff remains applicable, preserving the relative age differences among children based on their birth month from the onset of their educational journey.

2.2 Data

We use the Spanish School Survey on Drug Use (SSSDU), a nationally representative school–based survey on drug consumption conducted by the Ministry of Health every two years.[5] The target population of SSSDU is students enrolled in the last two grades of compulsory education (grades 3 and 4 of lower secondary, see Figure 1) and in upper secondary education (high school and vocational education). In the Spanish education system, the students' expected age is 15 and 16 in the last two compulsory grades and 17-18 in upper secondary education.

SSSDU follows a two–stage stratified sampling method where schools are first randomly selected, and, then, complete classes from the targeted schooling levels are randomly sampled. On the day of the survey, all students present in the classroom are surveyed.[6] The questionnaire collects information on a variety of risky behaviors, such as drug use, gambling and sexual activity, along with socio–demographic characteristics. Students fill in the paper–and–pencil questionnaire during a regular class (45–60 minutes) under the only supervision of the survey staff. Students are told that their answers will remain anonymous both to school and parents in order to encourage truth–telling responses and reduce under–reporting. The questionnaire design and the collection method follow other European drug use surveys.

We use the 2018 wave, which sampled students enrolled at the target grades during the 2018–2019 academic year.[7] Students' response rate is 97%. As explained above, the academic year begins in mid–September and extends until the end of June. This period is divided into three terms, with the precise start and end dates determined annually by regional authorities. For the 2018 wave, data collection spanned from February 4^{th} to April 5^{th} 2019. This period fully falls in the second term (mid–school year), capturing data from a stable academic setting where students have adjusted to their classes. Moreover, the narrow time span for data collection reduces potential differences in reported behavior that may arise from differences in the timing of the survey.[8]

y Formación Profesional, is available here: `https://www.libreria.educacion.gob.es/libro/sistema-estatal-de-indicadores-de-la-educacion-2023_182384/`.

[5]The agency responsible for collecting the SSSDU survey ("Encuesta sobre Uso de Drogas en Enseñanzas Secundarias en España") is the Monitoring Center for Drugs and Addictions in the Ministry of Health.

[6]Students with special education needs and recently arrived immigrant students whose mother tongue is not Spanish are also surveyed although their questionnaires are excluded from the SSSDU data set.

[7]We use the latest available wave before the COVID–19 pandemic outbreak. This allows us to employ recent data to estimate the school entry age policy effects while avoiding any bias in reported behavior due to the extraordinary environmental conditions posed by the pandemic, including difficulties to collect the SSSDU data.

[8]Unfortunately, we do not have information on the exact date when the survey was carried out in each class.

The sample contains 38,010 students from 917 schools. We exclude students enrolled in vocational education, as they may be underrepresented due to workplace training commitments, which increase their likelihood of absence on survey days. This only involves dropping 2,928 students (around 7.7% of the initial sample) as the vocational path is not the main choice following compulsory education; in Spain, the majority opts for high school to get access to university.[9] We also exclude a few students with inconsistent responses to birth year and retained status.[10] Inconsistent responses are a minor issue as they involve dropping only 149 students. The final sample of students enrolled in the last two grades of compulsory schooling and in high school contains 34,933 students, around 92% of the initial sample.

In our main analysis, we use the 21,156 students enrolled in compulsory education—60% of the final sample above. The reason is that SSSDU, by design, targets the schooling population and, therefore, does not include students who leave school following compulsory education. In consequence, the high school sample may be a positively selected sample of the total population of 17– and 18–years–old (the expected ages in high school, see Figure 1). Although the figures presented in footnote 9 do not suggest a substantial degree of selection, we focus on the students enrolled in compulsory education. Then, in section 4.5 we extend the analysis to the high school students.

Aside from other socio–demographic characteristics, SSSDU collects the student's birth year and month, which allows us to compute the distance to the cutoff in months. This is our key explanatory variable, which determines the student's relative age with respect to his or her school entry cohort.[11] In our analysis, we also need the student's grade level. In principle, SSSDU only identifies that a student is enrolled in compulsory education, but it does not specify the grade level (third or fourth). However, we can determine this information as follows. We infer all students' grade by first comparing the age they reach in 2019—calculated from the birth year—to the expected age in third and fourth grade—15 and 16, respectively, see Table 1. Note that some students are not in the grade predicted by their age if they have been held back due to poor academic performance. Therefore, we also integrate the data on the birth year and the expected grade with a student's reported retention status. Altogether, this information allows us to build the grade level for every student.[12]

As shown in Table 2 students born in 2004 and 2003 comprise the majority of the compulsory

[9]According to the 2019 wave of the Survey on Education and Labor Market Transition in Spain, around 96% of the students who obtained the compulsory education diploma transitioned to upper secondary education in the next year, while 4% left schooling. Among those who continued their education, around 86% opted for high school and 14% for a vocational degree (source: own calculations using the data retrieved from `https://www.ine.es/jaxi/Tabla.htm?tpx=43581&L=1`).

[10]For example, students claiming to be grade–repeaters and born in 2004 present a logical impossibility in SSSDU. A 2004–born student would be 15 in 2019 —the expected age for third grade (the lowest surveyed grade). Thus, this student could not be both a repeater and a third grader.

[11]Unfortunately, we cannot measure the distance to the cutoff in days, as SSSDU does not collect the student's birthday.

[12]There are no differences in the students' starting age. All students must begin school in the calendar year in which they turn 6.

sample—about 85%, see column (4)— as they turn 15 and 16 in 2019, the expected ages in third and fourth grade. Remaining students, born in 2002 or earlier, are consequently retained students. All students born in 2004 are enrolled in third grade, as shown in column (5). Retained students from this birth cohort are not sampled because they are enrolled in lower, non–surveyed, grades. Regarding the 2003 cohort, 74% of the students are enrolled in the expected grade (column (6)), while 26% are retained students in third grade (column (5)). In this case, only two–year retained students from the 2003 cohort are not observed due to being enrolled in not surveyed grades. Finally, columns (7) and (8) show similar rates of retained students, around 26%, in third and fourth grade. This retention rate is in line with the overall Spanish rate shown in the introduction.

From the SSSDU information, we define the risky behaviors used in our analysis, namely gambling, gaming, non–prescribed tranquilizers' use, alcohol, marijuana and tobacco use, vaping, internet use and sexual activity. Table A.2 reports the exact definition of each outcome. All of them are created as dummy variables equal to one if the student has adopted such behavior and zero otherwise. For alcohol, tobacco and marijuana use, vaping, and non–prescribed tranquilizers' use, we define behaviors for different time spans of consumption (lifetime, last year and last month). In the case of tobacco and tranquilizers, we also create an indicator of a more addictive use (daily in the last month), and for alcohol, we also consider if the student mixed it with energy drinks. For gambling, we create three outcomes that measure whether the student has gambled in the last year (the only time span provided by SSSDU), and whether she/he has gambled online or in person. We measure the propensity for gaming through three outcomes that indicate whether the student has played video games, played e–sports, and watched e–sports in the last year. Following Arenas-Arroyo et al. (2022), we construct a compulsive internet use indicator by combining multiple items providing signs of potential addictive use of internet (see the list of items in Table A.2). Another outcome of the internet use we consider is an indicator for visiting adult websites. Finally, we define three outcomes about the student's sexual activity that indicate whether the student had sex without using condoms, had sex without consent, and had sex but regretted it afterwards. Notice that the minimum legal age in Spain for gambling and for using alcohol, tobacco, marijuana and vaping is 18 years. However, the legal age regulation does not prevent minors from adopting these behaviors, as shown the high prevalence rates in the full sample in Table 3. Moreover, students perceive that alcohol, tobacco and marijuana are quite easy to obtain.[13]

2.3 Descriptive evidence

Figure 3, which shows the birth month distribution of students in compulsory education, suggests no parents' manipulation of their fertility decisions around the cutoff date. The percentage of students born in January and December is similar (8.3% and 8.6%, respectively). Figure 4 cor-

[13]The percentage of 16 year–old students who report that cigarettes, alcohol and cannabis are "fairly easy" or "very easy" to obtain are, respectively, 64%, 84% and 41% in Spain (see Table 3a in ESPAD (2020)). These percentages are higher than the European average reported in the same table (60%, 78%, and 32%, respectively).

roborates this finding. It presents the results from the balance tests using a one–month distance on either side of the cutoff (i.e., comparing students born in December with students born in January) for the grade–retained indicator and the socio–demographic variables (gender, non–Spanish status, parental education and employment status). There are no significant differences in the socio–demographic composition of the students born close to the cutoff date; thus, there is no evidence of cutoff manipulation, such as parents with specific characteristics influencing fertility or timing birth decisions.[14] The lack of significant differences in the students' socio–demographic background around the cutoff is also evidence of the enforcement of the Spanish birth rule that do not permit parents to advance or delay school entry to adjust for disparities in the child's development. The only significant, positive, difference is in the grade–retained variable, indicating a higher proportion of retained students among young–for–grade compared to old–for–grade students. As explained in the introduction, this is a common finding in the literature (Manacorda, 2012; Pedraja-Chaparro et al., 2015; Jerrim et al., 2022). Given this difference and since being a retained student may be associated with the decision to engage in risky behaviors, we control for this variable in our specification, and we also conduct a sensitivity analysis excluding retained students. Findings in Figure 4 stay the same when we use a two— and a three–month bandwidth to obtain the balance tests (results are available upon request).[15]

Figures in section B.1 in Appendix B provide exploratory evidence of the effect of the cutoff date for each outcome. Each dot in the plots represents the average outcome value for students born in a specific month, ordered by normalized month of birth. The normalized month of birth ranges from -6 (July births) to 5 (June births), with 0 denoting January births. The plots show a discrete jump in risky behaviors at the cutoff with January–born students generally showing higher rates of risky behaviors compared to December–born.

Finally, Table 3 compares the average prevalence rates between students born in December (young–for–grade) and in January (old–for–grade). Negative significant differences emerge for gambling, alcohol use, tobacco use, marijuana use, navigating adult websites, and sexual activity, indicating a lower prevalence of these behaviors among young–for–grade compared to the old–for–grade. The evidence provided so far indicates that being young–for–grade is negatively associated with the adoption of risky behaviors in early adolescence (i.e., students enrolled in the final grades of compulsory education).

[14] Moreover, Table A.3 in Appendix A shows that the distribution of the students' characteristics is highly similar across all months of birth.

[15] In the Spanish context, a potential concern regarding parents' manipulation of the cutoff is the *Baby–check* policy, a universal, one–time, child benefit to all new mothers approved in 2007 and canceled in 2010 for children born after December 31. Some parents advanced their date of birth from January 2011 to December 2010 to qualify for the benefit (Borra et al., 2019). However, this manipulation is not an issue for our study since, in the 2018 wave, students were born in 2004 or earlier.

3 Empirical strategy

Our strategy exploits the exogenous variation in the timing of school entry. The entry cutoff determines a perfect compliance with treatment assignment. We define the control group as the old–for–grade students, i.e, the students born above the cutoff and, as a consequence, they are the oldest in the academic cohort. The treated group consists of the young–for–grade students, i.e., the students born below the cutoff date and who, conversely, are the youngest in the academic cohort. In our baseline specification, we use a one–month distance around the cutoff, which implies that the young–for–grade and old–for–grade group consists, respectively, of the students born in December and in January. As a robustness check, we extend our analysis to using a two– and three–month bandwidth.

We specify the following regression to estimate the effect of the age disparities created by the school entry policy on the adoption of risky behaviors:

$$y_{isgc} = \alpha + \beta youngforgrade_i + \gamma X_i + \delta_c + \phi_s + \theta_g + \varepsilon_{isgc} \quad (1)$$

where y_{isgc} is the outcome for student i in school s from birth cohort c and enrolled in grade g; $youngforgrade_i$ is a dummy variable equal to one if student i is born one month below the cutoff (i.e., in December) and 0 if she/he is born one month above the cutoff (i.e., in January); X_i is a vector of control variables that includes the student's socio–demographic characteristics and grade–retained status; δ_c is a vector of birth cohort fixed effects; ϕ_s is a vector of school fixed effects; and θ_g is a vector of grade of enrollment fixed effects.

The vector of school fixed effects ϕ_s accounts for between–school sorting, a potential source of bias in the analysis of risky behaviors in the student population. Between–school sorting refers to the non–random school selection by parents, which leads to the students enrolled in the same school being prone to share a certain socio–economic background. This, in turn, may create differences in the students' propensity to adopt risky behaviors across schools, which may act as a confounder in the analysis. Including school fixed effects in equation (1) controls for this source of selection and implies that the identification of the effect of being young–for–grade on risky behaviors relies on within–school variation. Grade (θ_g) and birth year (δ_c) fixed effects are incorporated to control for potential unobserved time–specific differences in the propensity to engage in risky behaviors that may arise across different birth cohorts and grades (notice that grade and birth cohort are not equivalent because of the retention decisions, as discussed above). Our specification also includes the grade–retained dummy and the set of socio–demographic characteristics to account for potential differences in schooling performance and family background between young– and old–for–grade students that may affect their engagement in risky behaviors.

A potential concern regarding the inclusion of the grade–retained dummy in the regression is whether it can affect estimation results, as being young–for–grade also has an impact on academic performance. To check the sensitivity of the results, we re–estimate all the specifications excluding

the retained dummy, and the results do not change.[16] Moreover, in section 4.2 we discuss to what extent results are sensitive to excluding retained students from the estimation sample. Regarding socio–demographic variables, we should remark that they should not play any role in driving the results, as suggested by the balance tests in Figure 4. Indeed, in section 4.4, we estimate equation (1) without controlling for student's socio–demographic characteristics, and we get the same results. This strongly supports the assumption of randomness around the cutoff date.

Our specification leverages the exogenous variation in risky behaviors induced by the school entry cutoff, as illustrated by the month–of–birth patterns shown in the figures in Appendix B.1. The parameter of interest, β, measures the effect of being born in December—rather than January—on the propensity of engaging in each outcome, conditional on school, grade level, and birth year. We estimate equation (1) separately for each outcome using OLS. In this context, OLS is equivalent to an RDD model with a one–month bandwidth on both sides of the cutoff and a constant function on the running variable (month of birth).

To gain deeper insights into the effect of being young–for–grade on risky behaviors, we also estimate three alternative specifications to equation (1). These specifications are motivated by the high rate of retained students shown in Table 2. Retention decisions break the correspondence between birth year and grade level, as retained students stopped sharing the schooling cycle with their initial cohort–mates and joined the cohort that entered school one year later. These additional specifications explore the extent to which differences in absolute age—i.e., year of birth—and differences in the schooling cycle—i.e., grade level—may contribute to the overall young–for–grade effect estimated in equation (1).

First, to disentangle the role of differences in absolute age, we estimate the effect of being young–for–grade separately for the cohorts born in 2004 and 2003. We use these cohorts because they turn 15 and 16 years old in 2019, respectively, which aligns with the expected ages in the surveyed grades in lower secondary education.[17] The separate regressions by birth year follow the same specification as equation (1), with the exclusion of the birth year fixed effects.[18] In this approach, β estimates the effect of being young–for–grade among students from the same birth cohort—who entered school together—, although not all of them share the same educational cycle at the time of the survey due to retention.

Interpreting the results from this approach requires considering several factors. First, retained students from the 2003 and 2004 cohorts no longer progress with their cohort–mates' educational cycle, meaning they are not observed in the expected grade for their birth cohort (see Table 2). For instance, one–year retained students born in 2003 are in third grade, while their non–retained cohort–mates are in fourth grade (the expected grade). Second, as explained in section 2.2, one–

[16] The table with these results is available upon request.

[17] We do not estimate this specification for the students born in 2002 or earlier because all of them are retained students.

[18] In addition, the regression for the 2004 cohort does not include the grade–retained dummy and grade fixed effects, because all the observed students from the 2004 cohort are non–repeaters enrolled in third grade, the expected grade. As discussed in section 2.2, repeaters from this cohort are enrolled in lower, non–surveyed, grades.

and two–year retained students from the 2004 cohort and two–year retained students from the 2003 cohort are not observed due to the survey design. This could introduce positive selection in the academic composition of the observed students from both cohorts, which may affect, in turn, the estimation of the young–for–grade effect on the adoption of risky behaviors, particularly for the 2004 cohort where no retained students are observed. This issue is less pronounced for the 2003 cohort since it includes one–year retained students, as shown in Table 2. In section 4.2, we discuss in great detail how these factors may impact our findings.

The second approach aims to analyze the extent to which differences in the schooling cycle may influence the overall effect of being young–for–grade. This involves separate estimations for students enrolled in third and fourth grade. We use the same specification as in equation (1), but exclude grade fixed effects. In order to interpret the results from this second approach, it is important to note that students enrolled in third grade are born between 2000 and 2004—see column (7) in Table 2—but only the ones born in 2004 are the expected age; the others are all repeaters. Similarly, students enrolled in fourth grade are born between 2000 and 2003—see column (8) in Table 2—but those born in 2002 or earlier are repeaters. In this approach, β is the effect of being born in December compared to being born in January among the students who share the educational cycle at the time of the survey, although not all of them started school together—retained students entered earlier. Unlike the first approach, which focuses on students from the same birth cohort but may belong to different grade levels, the second approach focuses on students who are in the same grade but may have different entry years. In the absence of retention decisions, both approaches would be equivalent, as the cohort of students who enter school according to their birth year would continue together throughout all grades.

Finally, we consider a third specification that compares students who were born at roughly the same time—i.e., similar absolute age—but who differ in when they entered school due to the administrative cutoff. Specifically, we compare students born in December 2003 and in January 2004. Given the data included in SSSDU 2018, this is the only viable comparison group for this approach. For instance, the comparison between students born in December 2002 and January 2003 would be misleading because all students in compulsory education born in 2002 are retained students. Although born just a few weeks apart, December 2003–born and January 2004–born belong to different educational cycles, with the latter entering school a year later and being the oldest in their cohort, while December–2003 born enter a year earlier and are the youngest in their cohort. We estimate a regression similar to equation (1), where our key variable ($youngforgrade$) is replaced by a dummy variable equal to one if the student is born in December 2003 and zero if she/he is born in January 2004. Consequently, this specification excludes birth year and grade fixed effects. We also exclude the retained students born in December 2003 to avoid this confounding factor in the comparison, as there are no retained students born in January 2004 by the survey design.[19] In this approach, by comparing risky behaviors of students with the same

[19]December–2003 retained students were held back one year and became grade–mates of the 2004–born students

age but enrolled at different grades, we provide additional insight into how the educational cycle may drive the overall young–for–grade effect estimated in equation (1).

4 Results

4.1 Main estimation results

Figure 5 shows the OLS estimate of the young–for–grade dummy for each outcome and from the different specifications explained above. Notice that the label "Full" in the figure refers to the results obtained by pooling all students enrolled in third and fourth grades, across all birth years (equation (1)). Labels "2003 born" and "2004 born" refer to the separate estimation by birth year, and "3rd grade" and "4th grade" refer to the separate estimation by grade of enrollment. The last label refers to the results from the third approach that compares students born in December 2003 and January 2004. Table A.4 in Appendix A reports all the coefficients and robust standard errors. To interpret the results, it is important to note that outcomes are defined as dummy variables and, thus, the resulting regressions are linear probability models.[20]

First, the results from the estimation of equation (1) show that, when young–for–grade is significant, it always takes a negative value. This indicates that the age disparities created by the cutoff within an academic cohort result in the youngest students (December–born) being less likely to engage in risky behaviors compared to the oldest students (born in January). This lower propensity is observed across nearly all behaviors: gambling (both online and offline), drinking alcohol, smoking tobacco and marijuana, vaping, navigating adult websites, and engaging in sexual activity (both unprotected sex and intercourse later regretted). The effect in the pooled specification ranges from -0.09 for drinking alcohol in lifetime or last year to -0.01 for online gambling; the largest coefficients are found for using alcohol and tobacco, and visiting adult websites. A coefficient of -0.09 in alcohol last year means that the prevalence rate among young–for–grade is on average 9 percentage points lower than among old–for–grade. The magnitude of this coefficient suggests a substantial effect size, as it amounts to 13.2% of the average prevalence rate among old–for–grade (0.68, see Table 3). We do not find significant differences in the use of non–prescribed tranquilizers, gaming and compulsive internet use.

We also analyze alternative specifications to disentangle the extent to which the overall estimated effects are driven both by differences in absolute age and the schooling cycle, as retained decisions break the one–to–one correspondence between birth cohort and grade of enrollment around lower secondary education. To explore the role of differences in absolute age, we estimate the first approach outlined in section 3, which involves a separate estimation for the cohorts born in 2004 and 2003 and who, respectively, turn 15 and 16 in 2019. The results from these separate

who are enrolled in third grade. Results hardly change when we include these retained students, and add the grade–retained dummy as control. They are available upon request.

[20] Estimates from a linear probability model are good proxies for the marginal effects from a probability model, such as a probit model (Wooldridge, 2002).

estimations, shown in Figure 5, reveal that when the coefficients are significant, they are negative, indicating that young–for–grade students are less likely to adopt risky behaviors, in line with overall results. However, findings differ between the old (2003–born) and young (2004–born) cohorts. The significant differences between young–for–grade and old–for–grade students in gambling, the use of alcohol (in the last month), vaping and navigating adult websites found in the young cohort vanish in the old cohort. For tobacco use, in contrast, the differences remain stable across cohorts. New differences emerge in the old cohort for the use of non–prescribed tranquilizers and unprotected sexual activity.

To explore the role of the schooling cycle, we separately estimate the effect of being young–for–grade for third and fourth graders, as explained in section 3. Figure 5 shows that, for some outcomes, results closely align with those from the separate estimation by birth year, while for other outcomes, we observe differences. When comparing the results for the 2004 cohort with those from third graders—the expected grade for 2004–born students—, third graders exhibit smaller differences between young– and old–for–grade students in alcohol use but larger differences in marijuana use. The estimates for tobacco, vaping, adult websites, and unprotected sex remain similar. Fourth graders exhibit more marked and significant differences in gambling and alcohol consumption compared to the 2003 cohort. Differences in marijuana use become non–significant for fourth graders, while differences in tobacco and unprotected sex stay.

Finally, Figure 5 also presents the results from a third approach that compares students born in December 2003 with those born in January 2004. As explained in section 3, these students' absolute ages are close, but they entered different schooling cycles due to the cutoff date. Students born in January 2004 entered school one year later than students born in December 2003. Results show few, but positive, significant differences between these two groups. Students born in December 2003, who are among the youngest in their academic cohort, are more likely to smoke marijuana (in lifetime or last year) and tobacco (lifetime), and to use the internet compulsively than students born in January 2004, the oldest in their academic cohort. The latter are more likely to play video games than the former.

4.2 Discussion of results

Results from the third approach above show that most negative differences in risk–taking between old–for–grade and young–for–grade disappear if we compare students of similar age—i.e., December–2003 and January–2004 born. For the differences that remain significant, December–2003 born—the young–for–grade students in their academic cohort—tend to engage more in risky behaviors than January–2004 born—the old–for–grade students in their academic cohort. Our findings on substance use are in line with previous literature that has analyzed the effect of the school entry age policy on alcohol, tobacco and marijuana use (Argys and Rees, 2008; Shin, 2023; Johansen, 2021). However, unlike previous studies, we do not find that young–for–grade have a higher probability of engaging in sexual intercourse. For other outcomes that have not been

previously studied, we find that young–for–grade are more likely to use internet compulsively and less likely to play video games. We do not find significant differences for gambling, non–prescribed tranquilizers and vaping.

These results provide valuable insights for interpreting the negative significant differences between young–for–grade and old–for–grade found in the pooled estimation and in the separate estimations by grade and birth year. Results from the adjacent cohort analysis indicate that most negative differences can be primarily explained by the relative age disparities that the administrative cutoff creates within each academic cohort. In other words, the nearly one–year difference between January–born and December–born students in the same birth cohort and grade explains the lower probability of risk–taking by December born students. As highlighted earlier, when we control for absolute age differences in the January–2004 and December–2003 comparison, young–for–grade are, indeed, more likely to engage in certain risky behaviors. This suggests that the overall negative differences observed may be mostly driven by differences in development and maturity due to relative age disparities. This is in line with previous literature on ADHD that finds that maturity differences explain differences in diagnosis rates (see, for example, Elder (2010)).

Consistent with the maturity channel, the separate estimation by year of birth shows that most significant differences in behaviors due to the month of birth either disappear or decrease substantially between ages 15 and 16. This suggests that as students grow older, young–for–grade students tend to catch up with old–for–grade students. Table A.6 shows that the prevalence rate in behaviors tends to increase from 15 to 16 years old in both groups of students. For some behaviors such as gambling, alcohol and vaping, the average prevalence among old–for–grade students increases but at a lower rate than among young–for–grade, which results in the latter catching up old–for–grade as they grow up. The above estimation results show that some new differences appear, such as in unprotected sex and using tranquilizers ever in lifetime, which, as shown in Table A.6, are more prevalent behaviors at 16 among old–for–grade students. All this evidence also points to differences in maturity as an important driver of the overall negative effects of being young–for–grade.

To further explore maturity as a potential mechanism, we analyze the data on family rules and test for differences between young–for–grade and old–for–grade students. SSSDU collects information on the extent to which parents set rules at home or outside the home, and whether parents know who the student is with or where, when she or he goes out at night, and students' perceptions of whether their parents would allow them to use tobacco, alcohol, or marijuana. Students also report whether they observe their parents drinking alcohol or any household member smoking daily. Figure 6 shows that young–for–grade students are more likely to report that their parents set the rules in the social environment (outside home) and would not allow them to smoke tobacco or to drink alcohol. However, no difference is found in the students' perceptions about whether their parents would permit them to use marijuana. The reported proportions of parents drinking alcohol and household members smoking are not significantly different between young–

and old–for–grade, suggesting that differences in imitating behaviors observed at home are not driving the results. If parents set rules in line with their children's maturity, then this figure suggests that differences in parents' rules outside the home and permissibility to use alcohol and tobacco reflect differences in the degree of maturity between young– and old–for–grade.

We replicate the analysis for alcohol and tobacco consumption by birth year, grade, and January–2004 vs December–2003 born, adding the family rules' variables that present significant differences as per Figure 6. For both outcomes, we control for whether parents set the rules outside the home. Further, we control for whether parents would permit the specific behavior in the corresponding outcome regression. Table A.5 shows that the inclusion of these variables influences the young–for–grade effects. In the young cohort (2004–born) and third grade, most significant differences disappear, such as alcohol lifetime and last month; for the significant differences, coefficients drop between 13% (tobacco last month) to 50% (alcohol last year). In the 2003 cohort and fourth grade, differences in alcohol remain quite stable, while differences in tobacco become non–significant. Regarding the comparison of the same–age students (last column), the significant difference in alcohol last month vanishes but the difference in tobacco lifetime remains. All this evidence suggests that differences in maturity reflected by differences in family rules drive to some extent the observed differences in behaviors between young– and old–for–grade students.[21]

Nevertheless, differences in maturity are not the full story. We find significant differences in the separate estimation by grade and, more importantly, some significant differences arise from the comparison of similar–age students (born in December 2003 and January 2004). Indeed, as discussed above, results from the latter show a change of sign: the youngest in their academic cohort are more likely to engage in certain behaviors than the oldest in their cohort. This evidence points to the educational cycle also having a role in shaping differences between young–for–grade and old–for–grade in risk–taking. The impact of the educational cycle may stem from differences in the accumulated schooling at the time of the survey and also from the dynamics among classmates, which may relate to social norms and expectations related to a grade (Johansen, 2021). Regarding the latter, retained students (the oldest in the class) may act as role models for the younger—both in absolute and relative terms—students, exerting a negative influence in the adoption of risky behaviors by them. The negative influence of retained students in the use of alcohol and tobacco among all classmates, regardless of the month of birth, has been documented in Lopez-Mayan and Nicodemo (2023). Class dynamics would play a larger role in the results by grade than in findings by birth year, which may explain some of the differences found across the two specifications. Notice that all students born in 2004 are enrolled in third grade, but not all third graders are from this cohort; some are retained students from older cohorts, as shown in Table 2. Similarly,

[21] Fumarco and Principe (2024) investigate the role of parental supervision as a mediating factor for the effect of students' relative age to classmates on risky behaviors. Unlike our study, they do not find that this channel plays an important role. One possible explanation for our different findings is the different definition of family rules. Parental supervision in Fumarco and Principe (2024) is proxied by the weekly frequency of meeting with friends in the evening.

while many 2003–born students are in fourth grade, some are retained in third grade, and the remaining fourth–graders are retained students from older cohorts.

To better understand the role of retained students, we re–estimate all specifications after excluding them from the sample. The results are presented in Table A.7 in Appendix A. Compared to the findings in Figure 5 and Table A.4, which include retained students, the most noticeable differences arise in the separate estimations by grade. This suggests that class dynamics play a more significant role in that specification than in estimations by birth year. Among third and fourth graders, excluding retained students results in a larger negative gap in alcohol outcomes, whereas the previously significant negative gaps in gambling and marijuana use observed in the full sample disappear. For other behaviors, such as tobacco use, the results remain largely unchanged. The separate estimations by birth year reveal differences only in alcohol and tobacco outcomes. Specifically, when retained students from the 2003 cohort are excluded, the negative effects on alcohol and tobacco become more pronounced. These effects now resemble those estimated for the 2004 cohort, which originally comprises only non–retained students. For other outcomes, the exclusion of retained students has little impact on the results from the 2003 cohort.

The fact that results for alcohol and tobacco among non–retained students in the 2003 cohort align more closely with those of the 2004 cohort suggests that the unbalanced composition of retained students across cohorts may partly explain the differences in those outcomes between the two cohorts, as shown in Figure 5. As discussed in section 2.2, students born in 2004 who were retained due to poor academic performance are enrolled in lower, non–surveyed grades. This issue is less pronounced in the 2003 cohort, where one–year retained students are still surveyed in third grade, and only those retained twice are not surveyed.[22]

The findings in Table A.7 further indicate that excluding retained students leads to a strong alignment between the estimations by cohort and by grade. This is because we focus on non–retained students who entered school together—belonging to the same cohort—, and remain in the same schooling cycle at the time of the survey. For these students, the correspondence between grade level and year of birth has not been disrupted. This evidence suggests that, in the absence of retention, we would not observe differences in the results by grade and by birth year in the full sample. The fact that Figure 5 exhibits some differences between both approaches suggests that the presence of retained students in class influences the adoption of risky behaviors.

4.3 Heterogeneity analysis: gender and type of school

We first examine whether the effect of being young–for–grade on risky behaviors differs between boys and girls. Figure 7 shows the results by gender, and Table A.8 in Appendix A reports the coefficients and robust standard errors. Similar to the full sample results in Figure 5, most

[22]The absence of two–year retained students in both cohorts is a minor concern as the decision to retain a student more than once is infrequent in compulsory education. The overall two–year retention rate in the compulsory education sample is below 5%.

significant effects of being young–for–grade diminish or disappear as boys and girls age or progress through the educational cycle.

For offline gambling, the overall negative effect of being young–for–grade is driven by boys, with no significant differences among girls, consistent with higher gambling prevalence among boys (Calado et al., 2017). In the adjacent cohort analysis, boys show no significant differences in this outcome, indicating that the lower propensity of young–for–grade boys in the full specification is due to absolute age differences. Both boys and girls born in December 2003 (young–for–grade) are less likely to play video games than those born in January 2004 (old–for–grade), with a stronger effect for girls. This suggests that the schooling cycle, rather than age, drives this outcome.

Young–for–grade girls and boys are significantly less likely to drink alcohol, with a larger effect for girls, especially in the 2004 cohort. These differences disappear in the 2003 cohort, pointing to maturity as the primary driver. The adjacent cohort analysis supports this, showing no significant differences by gender. These results contrast with previous literature that finds gender differences in the effect of being young–for–grade (Shin, 2023; Argys and Rees, 2008; Johansen, 2021). For marijuana and tobacco use, young–for–grade boys and girls are less likely to engage in these behaviors, with stronger effects for boys in third grade. The adjacent cohort analysis reveals that, when controlling for maturity, young–for–grade boys are more likely to use tobacco and marijuana than old–for–grade boys, while no differences arise for girls. This contrasts with Argys and Rees (2008), which find that girls are more susceptible to peer pressure. In our case, results suggest that young–for–grade boys may be more vulnerable to social pressure.

For internet use, young–for–grade boys are less likely to visit adult websites, but this difference disappears in the adjacent cohort analysis. However, December 2003–born boys (young–for–grade) are more likely to use the internet compulsively than January 2004–born boys (old–for–grade), indicating that the schooling cycle, rather than age differences, drives this outcome. No gender differences are found for sex–related behaviors, non–prescribed tranquilizers, or vaping.

Next, we explore differences by school type. Around 63% of students attend public schools, aligning with national statistics.[23] Remaining students attend private schools, category that in SSSDU also includes semiprivate schools—*colegios concertados*—, private schools receiving public funding. The entry–age rule applies to all types of schools.

Table A.9 in Appendix A reports the results. The negative effect of being young–for–grade is present in both school types but diminishes when age differences are controlled for. For gambling, the effect is concentrated in public schools and explained by maturity differences. In the adjacent cohort analysis, young–for–grade students in public schools are less likely to play video games, with effects twice as strong as in private schools. For alcohol, the negative effect is slightly stronger in private schools, but maturity differences largely explain this, as most effects become non–significant in the adjacent cohort analysis. For marijuana, young–for–grade students in private

[23]The total percentage of students in public schools in 2018/2019 was 66% (see `https://estadisticas.educacion.gob.es/EducaDynPx/educabase/index.htm?type=pcaxis&path=/no-universitaria/alumnado/matriculado/2018-2019-rd/rg-todas&file=pcaxis&l=s0`).

schools are more likely to use it when maturity is controlled for, while no differences are found in public schools. No significant differences by school type are observed for tobacco, vaping, non–prescribed tranquilizers, internet use, or sexual activity.

4.4 Robustness

We test the sensitivity of results to a battery of robustness checks. First, we re–estimate the specification (1) after excluding the socio–demographic variables. The results stay the same as in the equivalent specification ("full" label) in Figure 5.[24] The lack of influence of socio–demographic characteristics on the results is a good indication of randomness around the cutoff date.

Second, we re–estimate equation (1) using different bandwidths to define the treatment and control groups around the cutoff date. We consider two– and three–month bandwidths, which implies using the students born between November and February, and between October and March, respectively. As explained in section 2.3, the balance tests for the socio–demographic variables also hold when using these alternative bandwidths. Figure B.3.1 in Appendix B shows that the results mirror our main findings from the full specification in Figure 5.

Finally, we assess the robustness of our results to employing an alternative estimation strategy, a sharp regression discontinuity approach. Month of birth is the running variable and fully determines the treatment status (being young–for–grade). We consider a three–month bandwidth on either side of the cutoff with linear functions of the running variable. The RDD results are plotted in Figure B.4.1 in Appendix B, and they hardly change compared to results from the full specification in Figure 5.

4.5 High school sample

Here, we explore whether our findings in compulsory education remain in high school. Figure B.2.1 in Appendix B shows that the distribution of the month of birth of high school students exhibits more differences, but not sizable, than the distribution in compulsory education in Figure 3. In particular, the percentage of high school students born in January and December is still quite close (7.3% and 7.9%, respectively). The slight differences in the distribution between compulsory school and high school may be the result of the potential positive selection in the high school sample. As discussed in section 2.2, SSSDU does not survey students who have left education following compulsory schooling. With the available information, we cannot deal with this selection and, therefore, findings on age disparities in risk–taking in the high school context should be considered with caution.

We estimate the same specifications outlined in section 3 using the high school students.[25] Notice that in the high school sample, the students from the birth years 2002 and 2001 are the expected age in 2019—17 and 18 respectively—for first and second high school grade. For the

[24] For the sake of brevity, we do not report these results, but they are available upon request.

[25] Similarly, for the separate estimation by birth year, we do not consider the birth cohorts enrolled in high school but solely composed by retained students (born in 2000 or earlier).

adjacent cohort analysis in high school, the only viable comparison is between students born in December 2001 and in January 2002. These two groups of students are roughly the same age, but they entered different schooling cycles. Students born in December 2001 are the young–for–grade in their academic cohort and entered school one year earlier than students born in January 2002, who are the old–for–grade in their academic cohort.

Figure 8 presents the young-for-grade effects, while Table A.11 in Appendix A reports the corresponding coefficients and robust standard errors. Some significant negative differences in behavior observed in compulsory education between young–for–grade and old–for–grade students fade out in high school and become non–significant, as shown in Figure 8. This is the case for smoking tobacco, vaping, and visiting adult websites.

However, for behaviors such as gambling, alcohol, marijuana, and unprotected sex, negative and significant differences persist, indicating that young–for–grade students remain less likely to engage in these behaviors than old–for–grade. Nevertheless, most of these differences disappear in separate analysis by birth year and grade, except for gambling and alcohol use in the last month. The effect on gambling remains stable across birth cohorts and grades at approximately 6 pp. For alcohol, the effect is significant in the old cohort (2001–born) but not in the young cohort (2002–born). Additionally, results by grade show that the gap in alcohol consumption is larger among second graders than first graders.

The adjacent cohort analysis further highlights the absence of significant gaps in most outcomes. Notably, the positive gaps observed in compulsory education for alcohol use in the last month, marijuana, lifetime tobacco use, and compulsive internet use are no longer significant in high school. In contrast, for some behaviors—such as tobacco use in the last month and vaping in the last year—we observe a negative gap between students born in December 2001 (young–for–grade) and those born in January 2002 (old–for–grade). Since no significant effects were found for these behaviors in compulsory education, this may suggest that the schooling cycle—including class dynamics and expected behavior within a grade—function differently or even in opposing ways across educational stages.

While multiple factors may be at play—and considering the issue of self–selection into high school—results from the high school sample suggest that most significant differences in risky behaviors between young–for–grade and old–for–grade students at ages 15 and 16 tend to disappear by ages 17 and 18. Similar to what we observe in compulsory education, young–for–grade high school students appear to catch up to old–for–grade in many behaviors (see Table A.10 in Appendix A). The fading of these differences over time is consistent with maturity as a potential mechanism. As students grow older, young–for–grade students will gradually reach higher levels of maturity, which may explain the disappearance of most behavioral gaps with old–for–grade students. Another plausible explanation, complementary to the maturity channel, is the role of social influence. The imitation process, in which old–for–grade students may serve as role models for young–for–grade students, may intensify throughout adolescence, further contributing to the

convergence in behaviors.

5 Conclusions

This study estimates the effect of the school entry age policy in Spain on the adolescents' adoption of risky behaviors. The cutoff created by the Spanish policy—January 1st—induces relative age differences in the cohort of students that enter school together. We exploit this exogenous cutoff to estimate the causal effect of being born before the cutoff (young–for–grade) compared to being born after (old–for–grade) on risky behaviors. Our main analysis focuses on students enrolled in the final grades of compulsory education, corresponding with early adolescence (15–16 years).

We find that being young–for–grade reduces the likelihood of engaging in various risky behaviors, including gambling, alcohol and tobacco use, and sexual activity among students from the same birth year and grade level. However, when we compare students of roughly the same age but who entered school at different years due to the administrative cutoff, few significant differences between old–for–grade and young–for–grade students remain. Moreover, those differences that stay significant present a positive sign, indicating that young–for–grade are more likely to engage in those behaviors—namely smoking tobacco and marijuana, and using internet compulsively—a finding opposite to the overall effect. These results are primarily explained by boys, as no significant differences are found among girls. This suggests that overall differences in risk–taking between old–for–grade and young–for–grade are mainly driven by differences in relative age within the academic cohort, which in turn may reflect differences in maturity. The maturity mechanism is also supported by the results obtained after controlling for differences in family rules. Results from the high school sample additionally suggest that the gaps in risky behaviors between old–for–grade and young–for–grade tend to disappear in late adolescence, although this evidence should be considered with caution due to potential selection into high school.

Our results also suggest that maturity is not the only mechanism. The educational cycle also contributes to shaping relative–age disparities in risk–taking, as suggested by the positive significant differences found among same–age students. These differences may reflect the impact of differences in accumulated schooling, and classroom dynamics, which involve interactions between young–for–grade, old–for–grade, and retained students. The evidence from the analysis without the retained students also suggests that old–for–grade and young–for–grade may be differently vulnerable to retained students' influence. Further research on this channel is needed.

Findings underscore the importance of studying the wide–ranging, non–academic impacts of the age disparities created by the school entry age policy. Developing and implementing age–appropriate educational programs on risky behaviors, taking into account the relative age differences within grades, could help address the varying levels of maturity and exposure to risk factors among students in the same grade. While young–for–grade students show lower engagement in risky behaviors, likely due to a lower maturity development, they may face other challenges re-

lated to being the youngest in their cohort. Moreover, in the absence of specific interventions, they eventually tend to catch up with old–for–grade students in the adoption of risky behaviors. On the other hand, schools may also implement targeted prevention programs for old–for–grade and retained students, who appear more prone to engaging in risky behaviors. These programs could focus on building resilience, decision–making skills, and awareness of the risks associated with behaviors like gambling, substance use, and unsafe sexual practices. Schools should provide academic and social–emotional support to help all students navigate potential difficulties resulting from differences in their degree of development.

Acknowledgments

Lopez-Mayan acknowledges financial support from the Spanish Ministry of Economy and Competitiveness grant PID2023-149412NB-I00. Montresor acknowledges funding from the University of Verona P.I.A. 2023 grant. Nicodemo acknowledges funding from the National Institute for Health Research Applied Research Collaboration Oxford and Thames Valley at Oxford Health NHS Foundation Trust and the Economic and Social Research Council, United Kingdom (grant number ES/T008415/1). Authors acknowledge insightful comments from participants at the XVI Labour Economics Meeting in Barcelona, the XVIII Winter School on Inequality and Social Welfare Theory in Canazei, and the III Italian Workshop on the Health Economics of Risky Health Behavior in Bologna.

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Tables

Table 1: Expected age by schooling level and grade

A. Compulsory education			
Grade	Expected age	Birth cohort	Repeaters' age
3rd	15	2004	>15
4th	16	2003	>16

B. High school			
Grade	Expected age	Birth cohort	Repeaters' age
1st	17	2002	>17
2nd	18	2001	>18

Table 2: Distribution of birth years and enrollment in the compulsory education sample

	Age in					Distrib. by grade	
Birth year	2019	N	%	In 3^{rd} g (%)	In 4^{th} g (%)	3^{rd} g	4^{th} g
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2004	15	8,500	40.2	**100**	–	**73.2**	–
2003	16	9,518	45.0	*25.8*	**74.2**	*21.1*	**74.1**
2002	17	2,395	11.3	*23.2*	*76.8*	*4.8*	*19.3*
2001	18	692	3.3	*14.0*	*86.0*	*0.8*	*6.2*
2000	19	51	0.2	*17.6*	*82.4*	*0.1*	*0.4*
Total	–	21,156	100	45.1	54.9	100 [11,612]	100 [9,544]

Expected age in 3^{rd} and 4^{th} grade is, respectively, 15 and 16 years old. In brackets, number of students. Bold text indicates students enrolled in the expected grade according to their birth cohort. Italics indicate retained students.

Table 3: Descriptive statistics of outcome variables in the compulsory education sample

	Full sample		Young–for–grade		Old–for–grade		Mean
Variables	N	Mean	N	Mean	N	Mean	diff.
Gambling							
Online	19163	0.046	1656	0.042	1583	0.058	-0.016**
							(0.008)
Offline	18257	0.108	1606	0.096	1505	0.128	-0.032***
							(0.011)
Online/Offline	19873	0.123	1722	0.112	1650	0.150	-0.038***
							(0.012)
Gaming							
Videogames	20501	0.832	1752	0.829	1712	0.836	-0.007
							(0.013)
Play e-sports	20695	0.486	1773	0.473	1725	0.494	-0.021
							(0.017)
Watch e-sports	20553	0.345	1755	0.326	1712	0.338	-0.012
							(0.016)
Non-prescribed tranquilizers							
Lifetime	20926	0.071	1803	0.063	1738	0.071	-0.008
							(0.008)
Last year	20929	0.050	1804	0.042	1739	0.053	-0.011
							(0.007)
Last month	20925	0.025	1804	0.024	1735	0.025	-0.002
							(0.005)
Last month, daily	20923	0.009	1804	0.008	1734	0.007	0.000
							(0.003)
Alcohol							
Lifetime	21137	0.690	1816	0.647	1756	0.729	-0.082***
							(0.015)
Last year	19483	0.641	1668	0.595	1622	0.681	-0.087***
							(0.017)
Last month	19032	0.419	1625	0.386	1585	0.459	-0.073***
							(0.017)
Last month, w/ ED	21069	0.133	1809	0.128	1752	0.146	-0.018
							(0.012)
Marijuana							
Lifetime	20495	0.214	1770	0.205	1708	0.228	-0.024*
							(0.014)
Last year	20447	0.178	1750	0.168	1702	0.187	-0.019
							(0.013)
Last month	20206	0.112	1727	0.101	1690	0.120	-0.018*
							(0.011)
Tobacco							
Lifetime	21111	0.331	1810	0.311	1752	0.349	-0.038**
							(0.016)
Last year	21101	0.280	1809	0.266	1750	0.299	-0.033**
							(0.015)
Last month	20722	0.194	1771	0.176	1718	0.212	-0.037***
							(0.013)
Last month, daily	20722	0.058	1771	0.050	1718	0.059	-0.010
							(0.008)
Vaping							
Lifetime	20991	0.451	1799	0.444	1748	0.461	-0.017
							(0.017)
Last year	19916	0.358	1707	0.350	1656	0.376	-0.026
							(0.017)
Last month	19845	0.143	1698	0.146	1652	0.143	0.003
							(0.012)
Internet use							
CIUS	20606	0.539	1773	0.561	1719	0.544	0.017
							(0.017)
Adult websites	20720	0.432	1774	0.414	1731	0.451	-0.037**
							(0.017)
Sexual activity							
No condom	20740	0.096	1789	0.080	1732	0.115	-0.035***
							(0.010)
No consensual	20724	0.014	1791	0.016	1729	0.014	0.002
							(0.004)
Regretted	20741	0.051	1788	0.049	1731	0.065	-0.016**
							(0.008)

21,156 students enrolled in compulsory education. Young–for–grade: students born in December. Old–for–grade: students born in January. Last column shows tests of differences in mean between young–for-grade and old–for–grade. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figures

Figure 1: Spanish schooling system

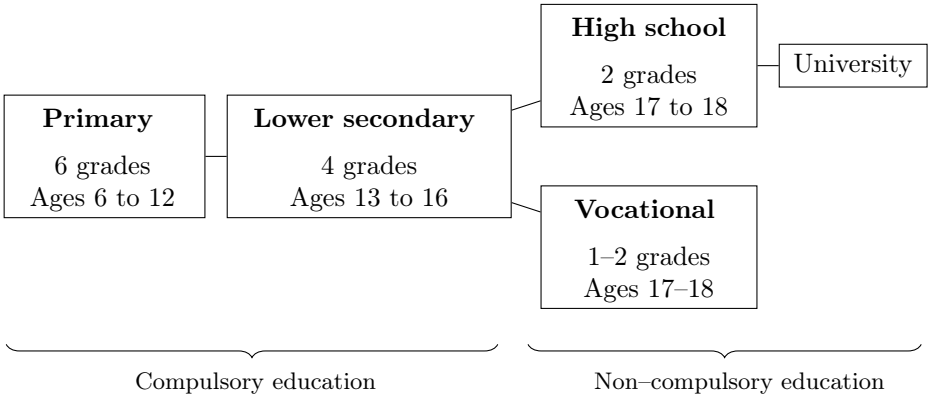


Figure 2: Spanish school entry policy

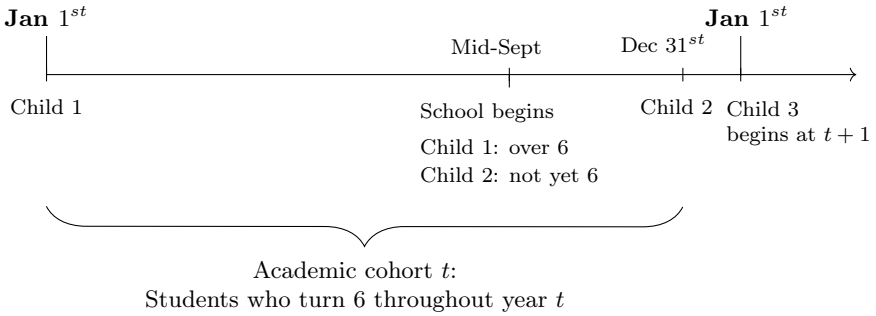
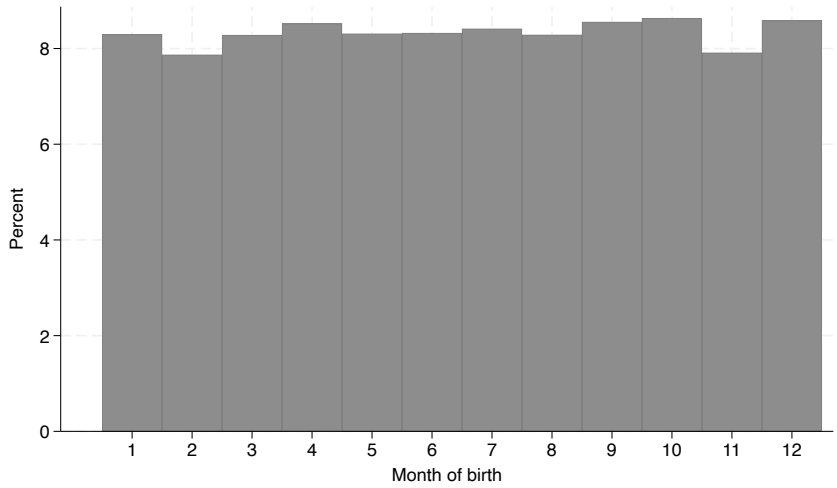
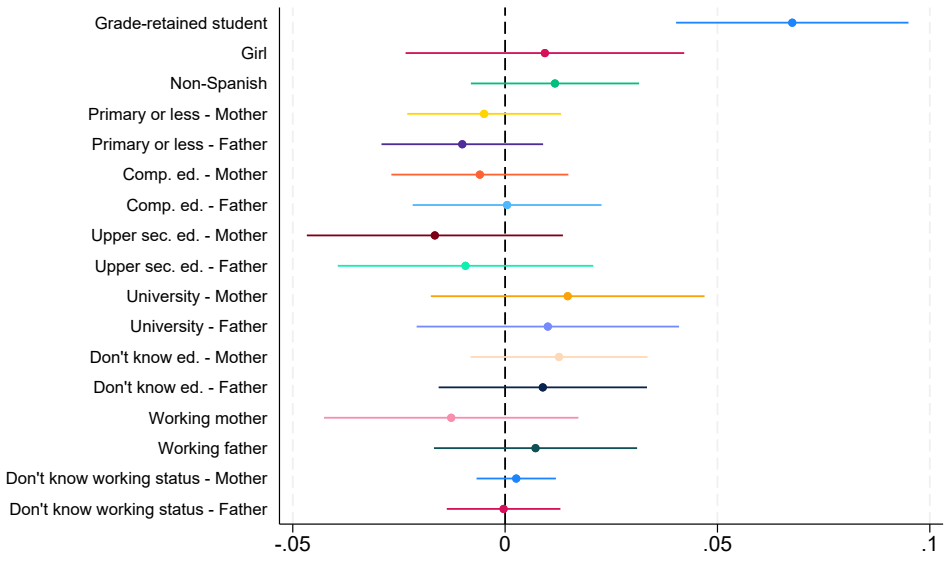


Figure 3: Distribution of month of birth - Students in compulsory education



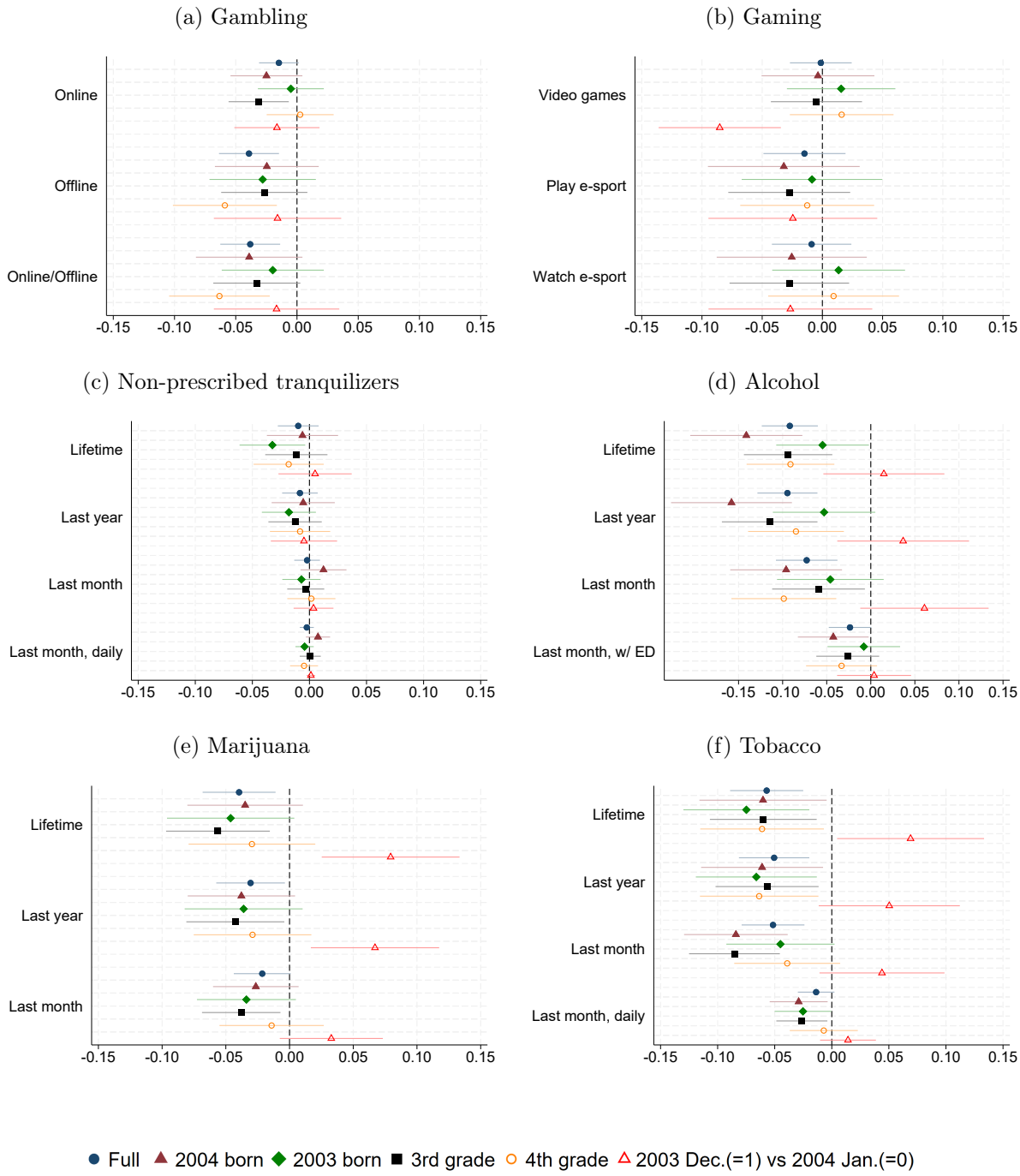
Note: SSSDU 2018 wave, N = 21,156 students.

Figure 4: Covariate balance test: Young–for–grade vs Old–for–grade students



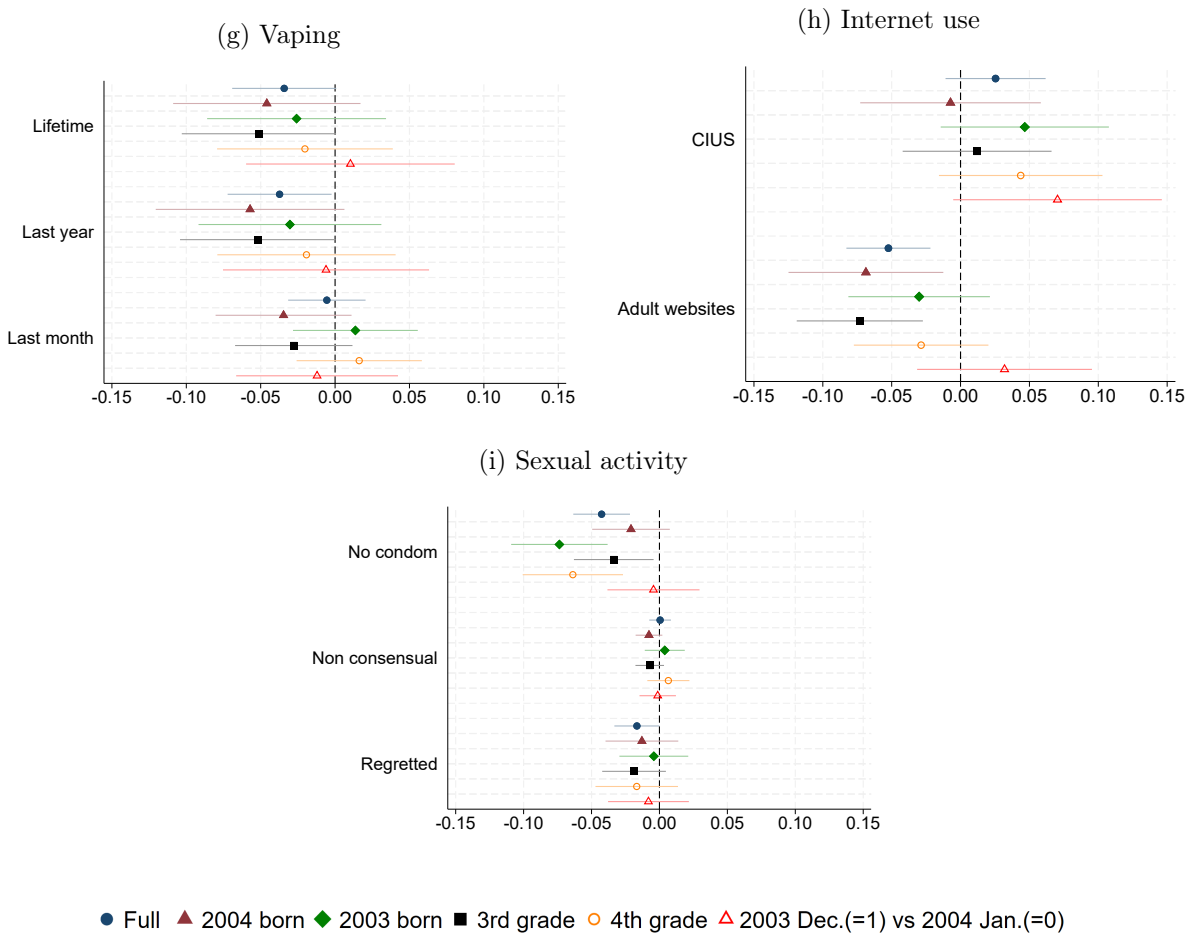
Note: Compulsory education sample. 1,756 old–for–grade students (born in January); 1,817 young–for–grade students (born in December).

Figure 5: Young–for–grade results — All specifications



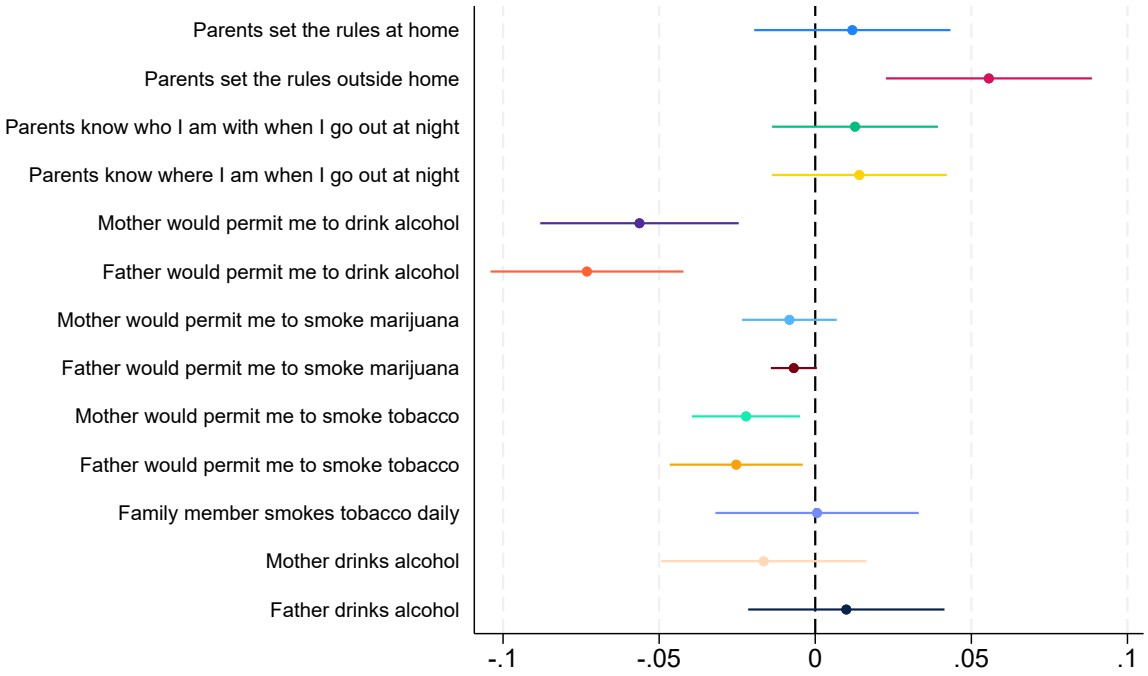
Note: Compulsory education sample. Number of observations in the sample used to estimate each specification: 3,573 students in the full specification; 1,411 and 1,596 born, respectively in 2004 and 2003; 1,933 and 1,640 enrolled, respectively, in 3rd and 4th grade; 1,309 born in December 2003 and January 2004. OLS estimates of the young–for–grade dummy. One separate estimation for each outcome in each specification.

Figure 5: Young–for–grade results — All specifications (continued)



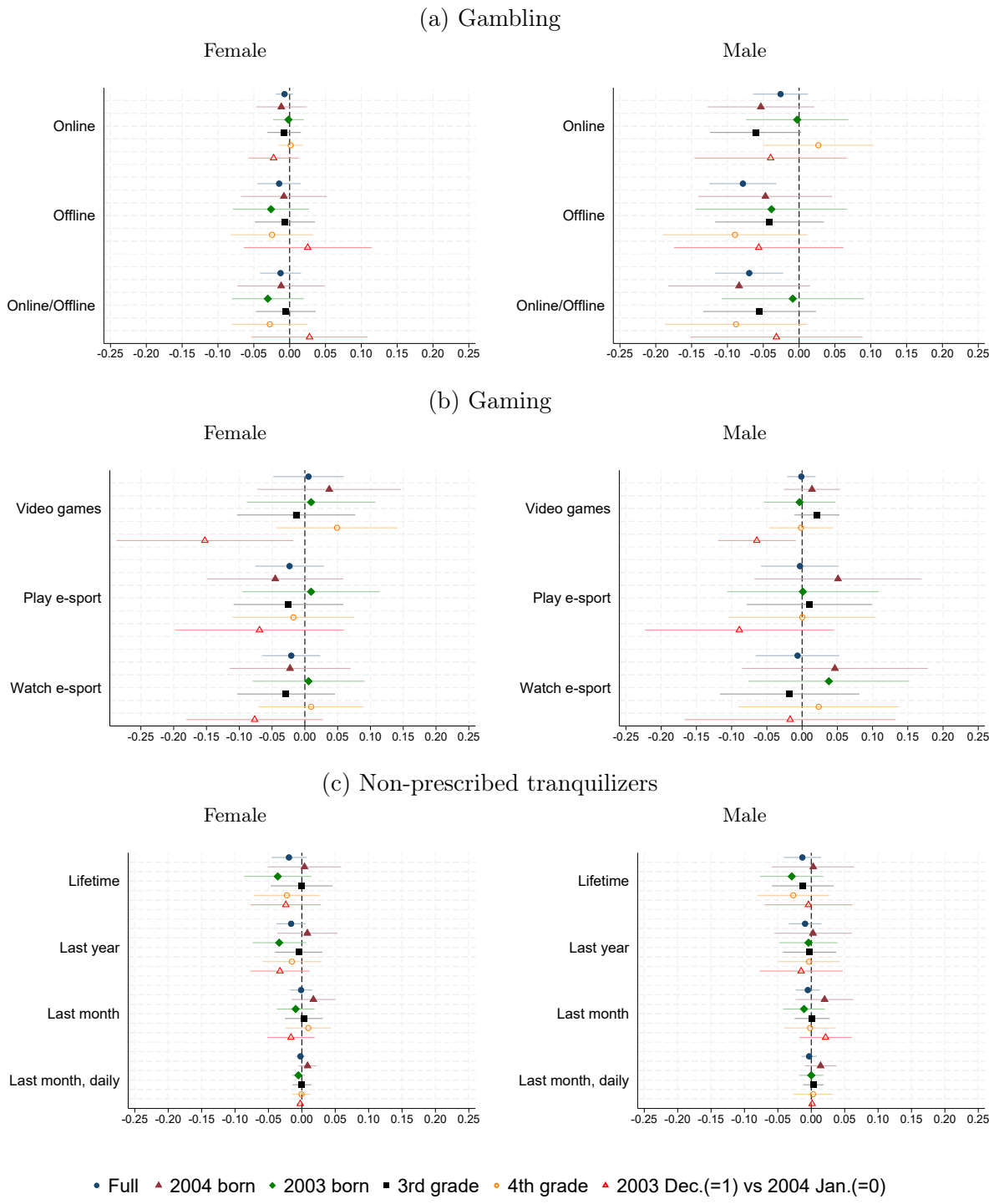
Note: Compulsory education sample. Number of observations in the sample used to estimate each specification: 3,573 students in the full specification; 1,411 and 1,596 born, respectively in 2004 and 2003; 1,933 and 1,640 enrolled, respectively, in 3rd and 4th grade; 1,309 born in December 2003 and January 2004. OLS estimates of the young–for–grade dummy. One separate estimation for each outcome in each specification.

Figure 6: Balance test for family rules: Young–for–grade vs Old–for–grade students



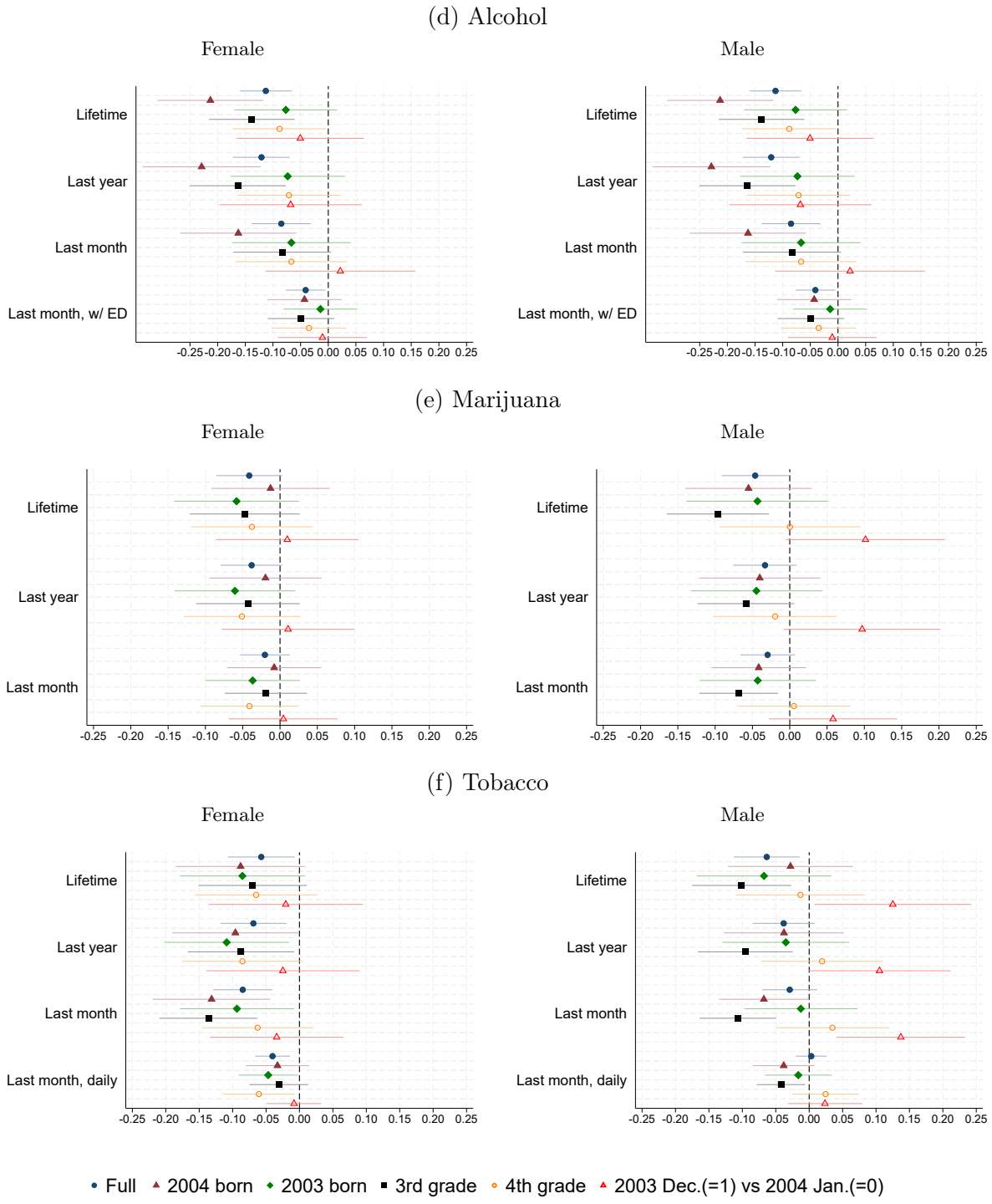
Note: Compulsory education sample. 1,756 old–for–grade students (born in January); 1,817 young–for–grade students (born in December). Family rules are defined as dummy variables. The first four variables are equal to 1 if the student reports that their parents always or almost always, respectively, set the rules at home, outside home, know who the student is with, or where she/he is. The next six variables are equal to 1 if the student reports that the mother or father would permit (or actually permits) him/her to use the respective substance. Next variable is equal to 1 if the student reports that a household member smokes daily. The last two questions are equal to 1 if the student reports that the mother or father drinks alcohol (occasionally, only on weekends, moderate daily drinking, or heavy daily drinking) and 0 if the father or mother never drinks.

Figure 7: Young–for–grade effects, by gender — All specifications



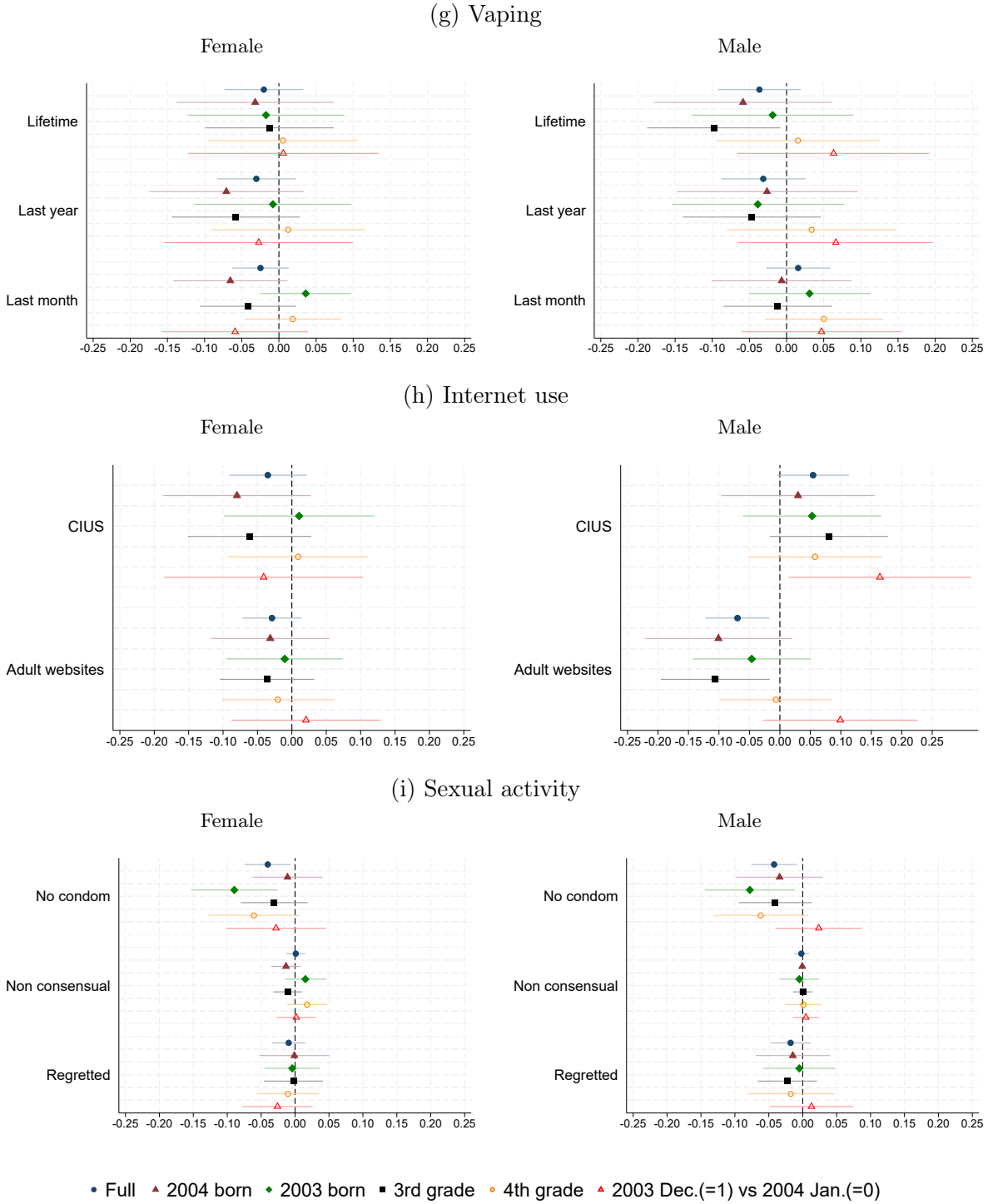
Note: Compulsory education sample. Number of observations in the sample used to estimate each specification: 1,832 girls and 1,741 boys, respectively, full specifications; 760 girls and 651 boys born in 2004, and 807 girls and 789 boys born in 2003; 988 girls and 945 boys enrolled in 3rd grad, and 844 girls and 796 boys enrolled in 4h grade; 676 girls and 629 boys born in December 2003 and January 2004. OLS estimates of the young–for–grade dummy. One separate estimation for each outcome in each specification.

Figure 7: Young–for–grade effects, by gender — All specifications (continued)



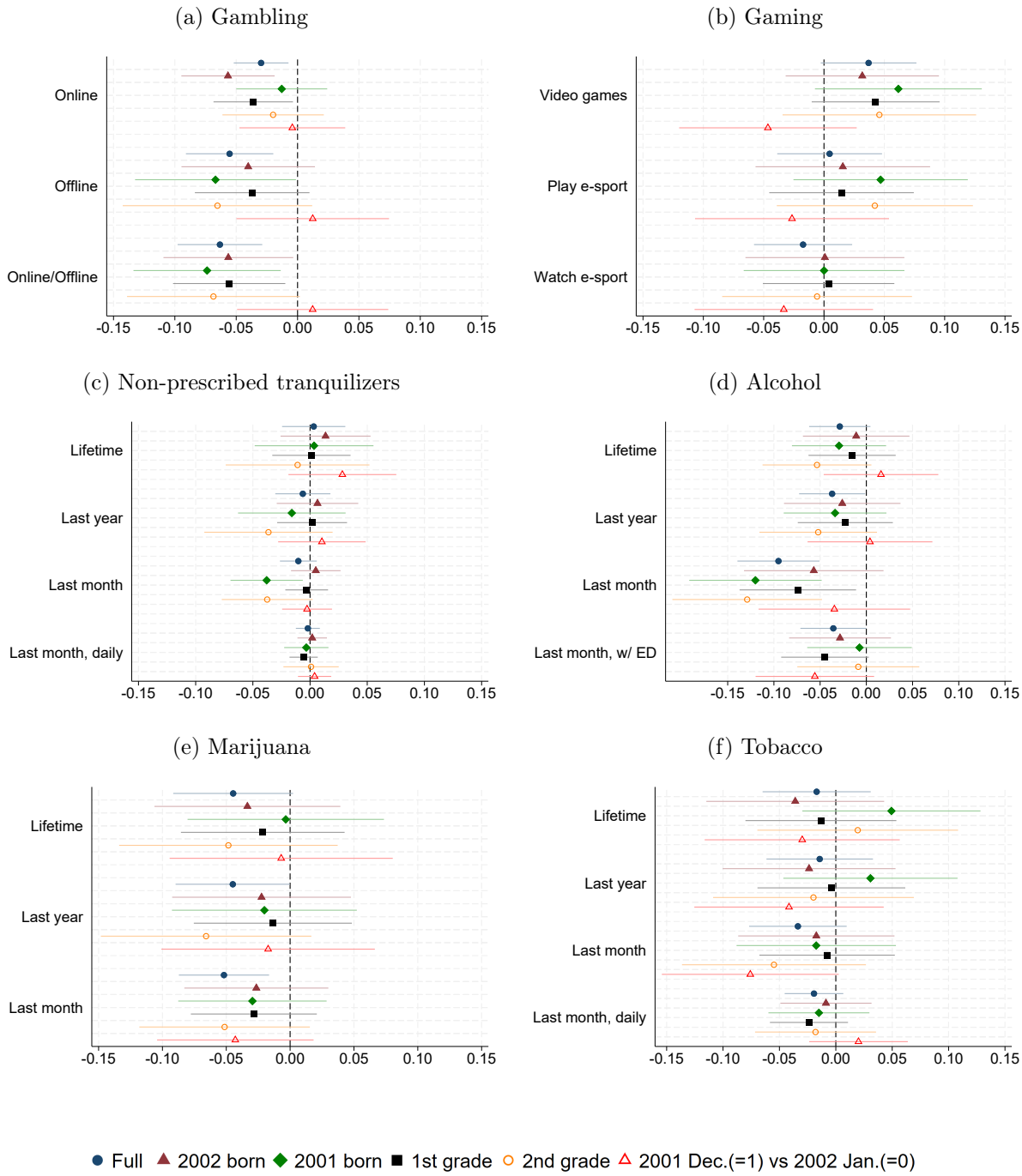
Note: Compulsory education sample. Number of observations in the sample used to estimate each specification: 1,832 girls and 1,741 boys, respectively, full specifications; 760 girls and 651 boys born in 2004, and 807 girls and 789 boys born in 2003; 988 girls and 945 boys enrolled in 3rd grad, and 844 girls and 796 boys enrolled in 4h grade; 676 girls and 629 boys born in December 2003 and January 2004. OLS estimates of the young–for–grade dummy. One separate estimation for each outcome in each specification.

Figure 7: Young–for–grade effects, by gender — All specifications (continued)



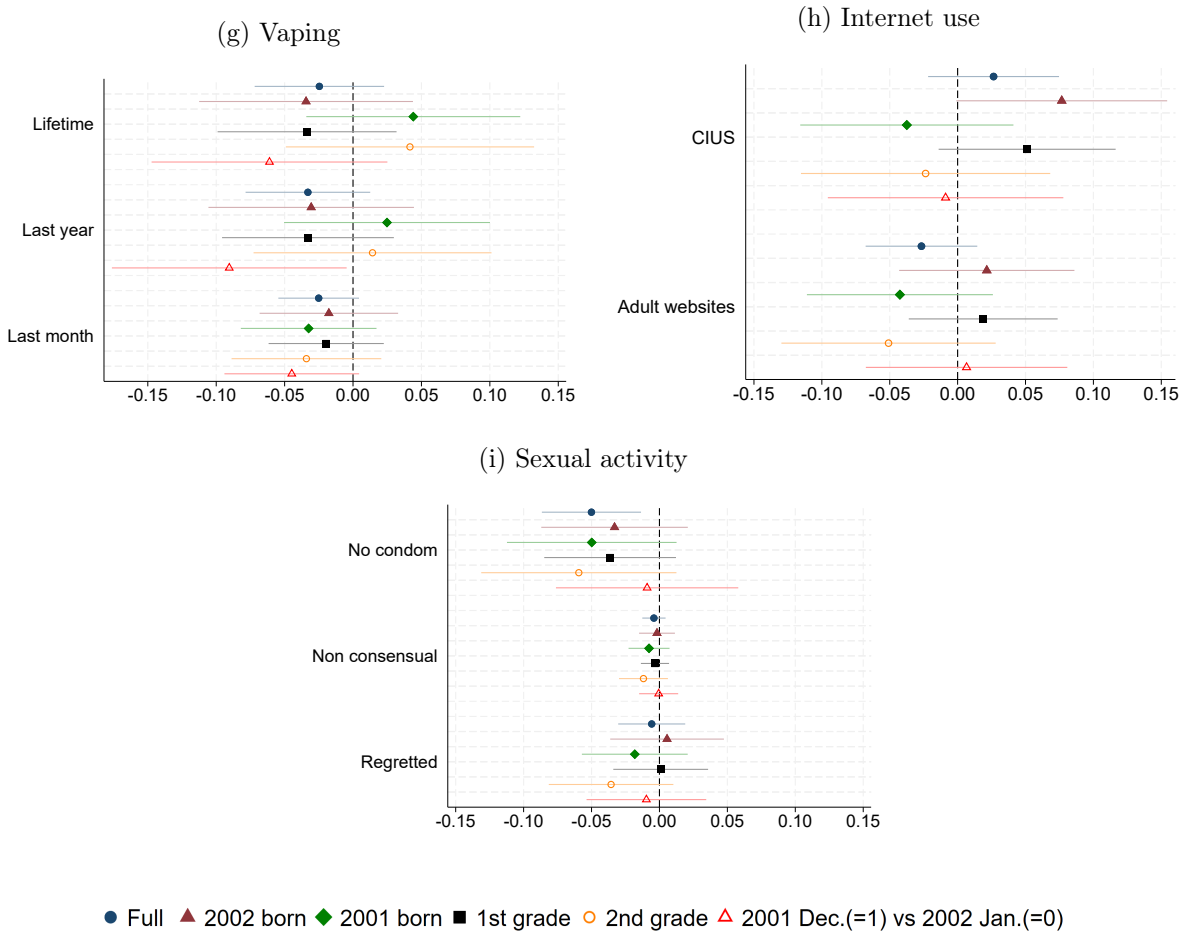
Note: Compulsory education sample. Number of observations in the sample used to estimate each specification: 1,832 girls and 1,741 boys, respectively, full specifications; 760 girls and 651 boys born in 2004, and 807 girls and 789 boys born in 2003; 988 girls and 945 boys enrolled in 3rd grad, and 844 girls and 796 boys enrolled in 4h grade; 676 girls and 629 boys born in December 2003 and January 2004. OLS estimates of the young–for–grade dummy. One separate estimation for each outcome in each specification.

Figure 8: Young–for–grade effects in high school — All specifications



Note: Number of observations in the sample used to estimate each specification: 2,089 students in the full specification; 1,024 and 968 born, respectively in 2002 and 2001; 1,298 and 791 enrolled, respectively, in 1st and 2nd grade; 928 born in December 2001 and January 2002. OLS estimates of the young–for–grade dummy. One separate estimation for each outcome in each specification.

Figure 8: Young–for–grade effects in high school — All specifications (continued)



Note: Number of observations in the sample used to estimate each specification: 2,089 students in the full specification; 1,024 and 968 born, respectively in 2002 and 2001; 1,298 and 791 enrolled, respectively, in 1st and 2nd grade; 928 born in December 2001 and January 2002. OLS estimates of the young–for–grade dummy. One separate estimation for each outcome in each specification.

Appendices

A Supplementary tables

Table A.1: Prevalence rates (%) — 16–year–old students

	Spain	Average 35 European countries
Tobacco, lifetime	41	41
Tobacco, last month	21	20
E-cigarettes, lifetime	42	40
E-cigarettes, last month	9.4	14
Alcohol, lifetime	78	79
Alcohol, last month	47	47
Alcohol, intoxication last month	17	13
Cannabis, lifetime	23	16
Non–prescribed tranquilizers, lifetime	4	6.6
Non–prescribed painkillers, lifetime	1.1	4
Gambling, last year	17	22
Online gambling, last year	4.2	7.9

Own elaboration using ESPAD data retrieved from Tables 5, 6, 7, 8a, 10b, 11a in ESPAD (2020). ESPAD: European School Survey Project on Alcohol and Other Drugs conducted by the European Monitoring Centre for Drugs and Drug Addiction.

Table A.2: Definition of outcome variables

Outcome	Definition
Gambling	
Online	= 1 if the student has gambled online in the past twelve months
Offline	= 1 if the student has gambled in person in the past twelve months
Online/Offline	= 1 if the student has gambled either online or in person in the past twelve months
Gaming	
Video games	= 1 if the student has played video games in the past twelve months
Play e–sports	= 1 if the student has played e–sports in the past twelve months
Watch e–sports	= 1 if the student has watched e–sports in the past twelve months
Non–prescribed tranquilizers	
Lifetime	= 1 if the student has ever used non–prescribed tranquilizers in her/his lifetime
Last year	= 1 if the student has used non–prescribed tranquilizers daily in the past twelve months
Last month	= 1 if the student has used non–prescribed tranquilizers in the past month
Last month, daily	= 1 if the student has used non–prescribed tranquilizers daily in the past month
Alcohol	
Lifetime	= 1 if the student has ever drunk alcoholic beverages in her/his lifetime
Last year	= 1 if the student has drunk alcoholic beverages in the past twelve months
Last month	= 1 if the student has drunk alcoholic beverages in the past month
Last month, w/ energy drinks	= 1 if the student has drunk alcoholic beverages mixed with energy drinks in the past month
Marijuana	
Lifetime	= 1 if the student has ever smoked marijuana in her/his lifetime
Last year	= 1 if the student has smoked marijuana in the past twelve months
Last month	= 1 if the student has smoked marijuana in the past month
Tobacco	
Lifetime	= 1 if the student has ever smoked cigarettes in her/his lifetime
Last year	= 1 if the student has smoked cigarettes in the past twelve months
Last month	= 1 if the student has smoked cigarettes in the past month
Last month, daily	= 1 if the student has smoked cigarettes daily in the past month
Vaping	
Lifetime	= 1 if the student has ever smoked electronic cigarettes in her/his lifetime
Last year	= 1 if the student has smoked electronic cigarettes in the past twelve months
Last month	= 1 if the student has smoked electronic cigarettes in the past month
Internet use	
CIUS (Compulsive Internet Use)	= 1 if the student answers frequently or very frequently to any of the following statements: Difficult to stop using the internet when online; Continue to use internet despite intention to stop; Others say you should use the internet less; Thinking about the internet even if offline; Looking forward to the next internet session; Thinking I should use the internet less often; Having unsuccessfully tried to spend less time on the internet; Feeling restless, frustrated when cannot I use the internet
Adult websites	= 1 if the student has visited adult websites (sex, violence,...) in the past twelve months
Sexual activity	
No condom	= 1 if the student had sex without condom in the past twelve months
No consensual	= 1 if the student had sex without consent in the past twelve months
Regretted	= 1 if the student had sex and she/he regretted in the past twelve months

Table A.3: Characteristics of students in compulsory education by month of birth

	All	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
Grade–retained student	0.22	0.19	0.20	0.20	0.21	0.22	0.21	0.22	0.23	0.23	0.24	0.24	0.26
Girl	0.51	0.51	0.51	0.49	0.50	0.52	0.49	0.50	0.51	0.50	0.52	0.50	0.52
Non-Spanish	0.10	0.10	0.09	0.09	0.10	0.11	0.08	0.08	0.11	0.09	0.10	0.10	0.11
Mother education:													
Primary or less	0.08	0.08	0.08	0.08	0.07	0.09	0.08	0.08	0.07	0.08	0.07	0.09	0.08
Compulsory	0.11	0.12	0.12	0.13	0.12	0.10	0.11	0.10	0.10	0.10	0.11	0.10	0.11
Upper second	0.30	0.31	0.29	0.27	0.29	0.29	0.30	0.31	0.31	0.29	0.30	0.29	0.29
University	0.39	0.39	0.38	0.40	0.40	0.40	0.39	0.37	0.39	0.39	0.40	0.40	0.40
Don't know	0.12	0.11	0.14	0.12	0.13	0.13	0.12	0.13	0.13	0.13	0.12	0.12	0.12
Father education:													
Primary or less	0.09	0.10	0.10	0.09	0.09	0.09	0.08	0.10	0.09	0.09	0.09	0.09	0.09
Compulsory	0.12	0.13	0.11	0.12	0.12	0.11	0.12	0.11	0.12	0.12	0.12	0.12	0.13
Upper second	0.29	0.30	0.30	0.28	0.28	0.31	0.30	0.31	0.29	0.28	0.30	0.29	0.29
University	0.32	0.32	0.30	0.33	0.34	0.32	0.33	0.30	0.32	0.33	0.31	0.34	0.33
Don't know	0.17	0.16	0.19	0.18	0.17	0.18	0.16	0.18	0.19	0.18	0.18	0.17	0.17
Working parents:													
Working mother	0.72	0.72	0.73	0.73	0.71	0.73	0.71	0.73	0.72	0.73	0.72	0.71	0.70
Don't know - mother	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.01	0.02	0.01	0.01	0.02	0.02
Working father	0.86	0.84	0.85	0.86	0.86	0.85	0.88	0.86	0.87	0.87	0.85	0.85	0.85
Don't know - father	0.04	0.04	0.04	0.04	0.04	0.04	0.03	0.04	0.03	0.03	0.04	0.05	0.04
Observations	21156	1756	1665	1752	1803	1758	1761	1780	1753	1810	1827	1674	1817

Table A.4: Young–for–grade effects in compulsory education — All specifications

	Full	Born in 2004	Born in 2003	3rd grade	4th grade	Born in Dec 2003 vs Jan. 2004
Gambling						
Online	-0.01*	-0.02*	-0.00	-0.03**	0.00	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Offline	-0.04***	-0.02	-0.03	-0.03	-0.06***	-0.02
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Online/offline	-0.04***	-0.04*	-0.02	-0.03*	-0.06***	-0.02
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Gaming						
Videogames	-0.00	-0.00	0.02	-0.01	0.02	-0.09***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Play e-sports	-0.01	-0.03	-0.01	-0.03	-0.01	-0.02
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Watch e-sports	-0.01	-0.03	0.01	-0.03	0.01	-0.03
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Non-prescribed tranquilizers						
Lifetime	-0.01	-0.01	-0.03**	-0.01	-0.02	0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Last year	-0.01	-0.01	-0.02	-0.01	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Last month	-0.00	0.01	-0.01	-0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Last month, daily	-0.00	0.01	-0.00	0.00	-0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)
Alcohol						
Lifetime	-0.09***	-0.14***	-0.05**	-0.09***	-0.09***	0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Last year	-0.09***	-0.16***	-0.05*	-0.11***	-0.08***	0.04
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Last month	-0.07***	-0.10***	-0.05	-0.06**	-0.10***	0.06
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Last month, w/ ED	-0.02*	-0.04**	-0.01	-0.03	-0.03	0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Marijuana						
Lifetime	-0.04***	-0.03	-0.05*	-0.05***	-0.03	0.08***
	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Last year	-0.03**	-0.04*	-0.04	-0.04**	-0.03	0.07***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Last month	-0.02*	-0.03	-0.03*	-0.04**	-0.01	0.03
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Tobacco						
Lifetime	-0.06***	-0.06**	-0.07***	-0.06**	-0.06**	0.07**
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Last year	-0.05***	-0.06**	-0.07**	-0.05**	-0.06**	0.05
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Last month	-0.05***	-0.08***	-0.04*	-0.08***	-0.04	0.04
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Last month, daily	-0.01	-0.03**	-0.02*	-0.02**	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Vaping						
Lifetime	-0.03*	-0.05	-0.03	-0.05*	-0.02	0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Last year	-0.04**	-0.06*	-0.03	-0.05*	-0.02	-0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Last month	-0.00	-0.03	0.01	-0.03	0.02	-0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Internet use						
CIUS	0.03	-0.01	0.05	0.01	0.05	0.07*
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Adult websites	-0.05***	-0.07**	-0.03	-0.07***	-0.03	0.03
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Sexual activity						
No condom	-0.04***	-0.02	-0.07***	-0.03**	-0.06***	-0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
No consensual	0.00	-0.01	0.00	-0.01	0.01	-0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Regretted	-0.02*	-0.01	-0.00	-0.02	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	3,573	1,411	1,596	1,933	1,640	1,309

Each cell in each column shows the OLS coefficient of the young–for–grade dummy from a separate regression. Robust standard errors in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Young–for–grade effects in compulsory education — including family rules

	Born in 2004	Born in 2003	3rd grade	4th grade	Born in Dec. 2003 vs Jan. 2004
Alcohol					
Lifetime	-0.07*	-0.08**	-0.05	-0.09***	-0.02
	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
Last year	-0.08*	-0.07*	-0.06*	-0.07**	0.01
	(0.04)	(0.04)	(0.03)	(0.03)	(0.05)
Last month	-0.04	-0.06*	-0.02	-0.09**	0.03
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Last month, w/ ED	-0.02	-0.04*	-0.02	-0.04	-0.01
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Tobacco					
Lifetime	-0.04	-0.07**	-0.05*	-0.04	0.06*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Last year	-0.05	-0.06*	-0.06**	-0.04	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Last month	-0.06**	-0.04	-0.07***	-0.02	0.03
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Last month, daily	-0.01	-0.01	-0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1,411	1,596	1,933	1,640	1,309
Family rules†	✓	✓	✓	✓	✓

Each cell in each column shows the OLS coefficient of the young–for–grade dummy from a separate regression. †Family rules: all regressions control for whether parents set the rules outside home always or almost always; regressions for alcohol outcomes control for whether the father and mother would permit (or actually permits) the student to drink alcohol; regressions for tobacco outcomes control for whether the father and mother would permit (or actually permits) the student to smoke. Robust standard errors in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Average outcomes by treatment status and birth year — Compulsory education

	Born in 2004 (15 years old)			Born in 2003 (16 years old)		
Variables	Young–for–grade	Old–for–grade	Diff.	Young–for–grade	Old–for–grade	Diff.
Gambling						
Online	0.023	0.055	-0.032***	0.046	0.054	-0.008
	(0.006)	(0.009)	(0.011)	(0.008)	(0.008)	(0.011)
Offline	0.072	0.099	-0.027*	0.112	0.142	-0.030*
	(0.010)	(0.012)	(0.016)	(0.012)	(0.013)	(0.018)
Online/ offline	0.084	0.124	-0.039**	0.132	0.156	-0.024
	(0.011)	(0.013)	(0.017)	(0.012)	(0.013)	(0.018)
Gaming						
Video games	0.843	0.866	-0.023	0.828	0.816	0.012
	(0.014)	(0.013)	(0.019)	(0.014)	(0.014)	(0.019)
Play e-sports	0.463	0.503	-0.039	0.489	0.490	-0.001
	(0.019)	(0.019)	(0.027)	(0.018)	(0.018)	(0.025)
Watch e-sports	0.322	0.345	-0.023	0.330	0.333	-0.004
	(0.018)	(0.018)	(0.025)	(0.017)	(0.017)	(0.024)
Non-prescribed tranquilizers						
Lifetime	0.049	0.051	-0.001	0.056	0.083	-0.027**
	(0.008)	(0.008)	(0.012)	(0.008)	(0.010)	(0.013)
Last year	0.034	0.043	-0.010	0.035	0.059	-0.024**
	(0.007)	(0.008)	(0.010)	(0.007)	(0.008)	(0.011)
Last month	0.020	0.019	0.001	0.018	0.025	-0.007
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.007)
Last month, daily	0.006	0.004	0.001	0.003	0.006	-0.004
	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)
Alcohol						
Lifetime	0.534	0.646	-0.111***	0.690	0.778	-0.088***
	(0.019)	(0.018)	(0.026)	(0.017)	(0.015)	(0.022)
Last year	0.468	0.588	-0.120***	0.653	0.739	-0.086***
	(0.020)	(0.019)	(0.028)	(0.018)	(0.016)	(0.024)
Last month	0.255	0.339	-0.084***	0.438	0.523	-0.085***
	(0.017)	(0.019)	(0.026)	(0.019)	(0.018)	(0.026)
Last month, w/ ED	0.090	0.109	-0.019	0.117	0.150	-0.032*
	(0.011)	(0.012)	(0.016)	(0.012)	(0.012)	(0.017)
Marijuana						
Lifetime	0.115	0.135	-0.020	0.208	0.266	-0.058***
	(0.012)	(0.013)	(0.018)	(0.015)	(0.016)	(0.022)
Last year	0.090	0.113	-0.023	0.172	0.221	-0.048**
	(0.011)	(0.012)	(0.016)	(0.014)	(0.015)	(0.020)
Last month	0.050	0.065	-0.015	0.095	0.142	-0.047***
	(0.008)	(0.009)	(0.013)	(0.011)	(0.012)	(0.016)
Tobacco						
Lifetime	0.212	0.260	-0.049**	0.318	0.391	-0.073***
	(0.015)	(0.017)	(0.023)	(0.017)	(0.017)	(0.024)
Last year	0.188	0.230	-0.042*	0.272	0.334	-0.063***
	(0.015)	(0.016)	(0.022)	(0.016)	(0.017)	(0.023)
Last month	0.101	0.155	-0.054***	0.181	0.232	-0.051**
	(0.011)	(0.014)	(0.018)	(0.014)	(0.015)	(0.023)
Last month, daily	0.022	0.031	-0.009	0.037	0.064	-0.027**
	(0.006)	(0.007)	(0.009)	(0.007)	(0.009)	(0.011)
Vaping						
Lifetime	0.367	0.420	-0.052**	0.456	0.482	-0.026
	(0.018)	(0.019)	(0.026)	(0.018)	(0.018)	(0.025)
Last year	0.295	0.350	-0.054**	0.356	0.404	-0.048*
	(0.018)	(0.018)	(0.026)	(0.018)	(0.018)	(0.025)
Last month	0.123	0.137	-0.014	0.132	0.135	-0.003
	(0.013)	(0.013)	(0.018)	(0.012)	(0.012)	(0.018)
Internet use						
CIUS	0.527	0.539	-0.012	0.582	0.557	0.026
	(0.019)	(0.019)	(0.027)	(0.018)	(0.018)	(0.025)
Adult websites	0.343	0.391	-0.047*	0.462	0.480	-0.017
	(0.018)	(0.019)	(0.026)	(0.018)	(0.018)	(0.025)
Sexual activity						
No condom	0.038	0.056	-0.018	0.069	0.136	-0.068***
	(0.007)	(0.009)	(0.011)	(0.009)	(0.012)	(0.015)
No consensual	0.006	0.010	-0.004	0.016	0.017	-0.002
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)
Regretted	0.031	0.045	-0.013	0.040	0.063	-0.023**
	(0.007)	(0.008)	(0.010)	(0.007)	(0.009)	(0.011)
Observations	711	700		780	816	

The table shows mean values for each variable for young– and old– for grade students and respective difference–in–means tests, separately for cohorts born in 2004 and 2003.

Table A.7: Young–for–grade effects in compulsory education — All specifications, excluding repeaters

	Full	Born in 2004	Born in 2003	3rd grade	4th grade	Born in Dec 2003 vs Jan. 2004
Gambling						
Online	-0.02**	-0.02*	-0.01	-0.03**	-0.01	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Offline	-0.03**	-0.02	-0.03	-0.02	-0.03	-0.02
	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Online/offline	-0.03**	-0.04*	-0.03	-0.04*	-0.04	-0.02
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Gaming						
Videogames	-0.01	-0.00	-0.00	-0.01	0.00	-0.09***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Play e-sports	-0.02	-0.03	-0.01	-0.02	-0.02	-0.02
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Watch e-sports	-0.01	-0.03	0.01	-0.03	0.01	-0.03
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Non-prescribed tranquilizers						
Lifetime	-0.01	-0.01	-0.03**	-0.00	-0.04**	0.00
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Last year	-0.00	-0.01	-0.01	-0.00	-0.02	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Last month	0.01	0.01	-0.00	0.01	-0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Last month, daily	0.00	0.01	-0.00	0.01	-0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)
Alcohol						
Lifetime	-0.12***	-0.14***	-0.09***	-0.13***	-0.10***	0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Last year	-0.13***	-0.16***	-0.09***	-0.14***	-0.10***	0.04
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Last month	-0.09***	-0.10***	-0.09**	-0.08**	-0.10***	0.06
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Last month, w/ ED	-0.03**	-0.04**	-0.02	-0.04**	-0.02	0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Marijuana						
Lifetime	-0.04**	-0.03	-0.04	-0.04	-0.03	0.08***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Last year	-0.04***	-0.04*	-0.04*	-0.04*	-0.04	0.07***
	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Last month	-0.02**	-0.03	-0.03	-0.03	-0.03	0.03
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Tobacco						
Lifetime	-0.07***	-0.06**	-0.08***	-0.05*	-0.08**	0.07**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Last year	-0.07***	-0.06**	-0.09***	-0.06**	-0.08**	0.05
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Last month	-0.06***	-0.08***	-0.04	-0.08***	-0.03	0.04
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Last month, daily	-0.02***	-0.03**	-0.02*	-0.03**	-0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Vaping						
Lifetime	-0.04**	-0.05	-0.05	-0.04	-0.05	0.01
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
Last year	-0.06***	-0.06*	-0.06	-0.05	-0.06	-0.01
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
Last month	-0.01	-0.03	0.01	-0.03	0.01	-0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Internet use						
CIUS	0.01	-0.01	0.03	-0.01	0.03	0.07*
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
Adult websites	-0.04**	-0.07**	0.00	-0.07**	-0.01	0.03
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Sexual activity						
No condom	-0.04***	-0.02	-0.07***	-0.02	-0.07***	-0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
No consensual	-0.00	-0.01	0.00	-0.01*	0.01	-0.00
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Regretted	-0.01	-0.01	-0.00	-0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	2,760	1,411	1,301	1,546	1,214	1,309

Each cell in each column shows the OLS coefficient of the young–for–grade dummy from a separate regression. Robust standard errors in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Young–for–grade effects in compulsory education, by gender — All specifications

	Full		Born in 2004		Born in 2003		3rd grade		4th grade		Born in Dec. 2003 vs Jan. 2004	
	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys
Gambling												
Online	-0.01	-0.03	-0.01	-0.05	-0.00	-0.00	-0.01	-0.06*	0.00	0.03	-0.02	-0.04
	(0.01)	(0.02)	(0.02)	(0.04)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.04)	(0.02)	(0.05)
Offline	-0.01	-0.08***	-0.01	-0.05	-0.03	-0.03	-0.01	-0.04	-0.02	-0.09*	0.03	-0.06
	(0.02)	(0.02)	(0.03)	(0.05)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.05)	(0.06)
Online/offline	-0.01	-0.07***	-0.01	-0.08*	-0.03	-0.03	-0.01	-0.06	-0.03	-0.09*	0.03	-0.03
	(0.01)	(0.02)	(0.03)	(0.05)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.04)	(0.06)
Gaming												
Videogames	0.01	-0.00	0.04	0.01	0.01	0.01	-0.01	0.02	0.05	-0.00	-0.15**	-0.06**
	(0.03)	(0.01)	(0.06)	(0.02)	(0.05)	(0.05)	(0.05)	(0.02)	(0.05)	(0.02)	(0.07)	(0.03)
Play e-sports	-0.02	-0.00	-0.05	0.05	0.01	0.01	-0.02	0.01	-0.02	0.00	-0.07	-0.09
	(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)
Watch e-sports	-0.02	-0.01	-0.02	0.05	0.01	0.01	-0.03	-0.02	0.01	0.02	-0.08	-0.02
	(0.02)	(0.03)	(0.05)	(0.07)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.06)	(0.05)	(0.08)
Non-prescribed tranquilizers												
Lifetime	-0.02	-0.01	0.00	0.00	-0.04	-0.04	-0.00	-0.01	-0.02	-0.03	-0.02	-0.00
	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Last year	-0.02	-0.01	0.01	0.00	-0.03*	-0.03*	-0.00	-0.00	-0.01	-0.00	-0.03	-0.02
	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Last month	-0.00	-0.01	0.02	0.02	-0.01	-0.01	0.00	0.00	0.01	-0.00	-0.02	0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Last month, daily	-0.00	-0.00	0.01	0.01	-0.01	-0.01	0.00	0.00	-0.00	0.00	-0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.00)	(0.00)
Alcohol												
Lifetime	-0.11***	-0.10***	-0.21***	-0.12*	-0.08	-0.08	-0.14***	-0.12**	-0.09**	-0.09**	-0.05	0.08
	(0.02)	(0.03)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)	(0.07)
Last year	-0.12***	-0.10***	-0.23***	-0.13*	-0.07	-0.07	-0.16***	-0.13**	-0.07	-0.08	-0.07	0.08
	(0.03)	(0.03)	(0.05)	(0.07)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.07)	(0.08)
Last month	-0.08***	-0.08**	-0.16***	-0.05	-0.07	-0.07	-0.08*	-0.08	-0.07	-0.10*	0.02	0.08
	(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.07)	(0.07)
Last month, w/ ED	-0.04**	-0.02	-0.04	-0.06*	-0.01	-0.01	-0.05	-0.05	-0.03	-0.01	-0.01	0.00
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Marijuana												
Lifetime	-0.04*	-0.05**	-0.01	-0.06	-0.06	-0.06	-0.05	-0.10***	-0.04	0.00	0.01	0.10*
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)
Last year	-0.04*	-0.03	-0.02	-0.04	-0.06	-0.06	-0.04	-0.06*	-0.05	-0.02	0.01	0.10*
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)
Last month	-0.02	-0.03	-0.01	-0.04	-0.04	-0.04	-0.02	-0.07**	-0.04	0.01	0.00	0.06
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Tobacco												
Lifetime	-0.06**	-0.06**	-0.09*	-0.03	-0.09*	-0.09*	-0.07*	-0.10***	-0.06	-0.01	-0.02	0.13**
	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)
Last year	-0.07***	-0.04	-0.10**	-0.04	-0.11**	-0.11**	-0.09**	-0.10***	-0.09*	0.02	-0.02	0.11*
	(0.03)	(0.02)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)
Last month	-0.08***	-0.03	-0.13***	-0.07**	-0.09**	-0.09**	-0.14***	-0.11***	-0.06	0.03	-0.03	0.14***
	(0.02)	(0.02)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)
Last month, daily	-0.04***	0.00	-0.03	-0.04	-0.05**	-0.05**	-0.03	-0.04**	-0.06**	0.02	-0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Vaping												
Lifetime	-0.02	-0.04	-0.03	-0.06	-0.02	-0.02	-0.01	-0.10**	0.01	0.02	0.01	0.06
	(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.07)	(0.07)
Last year	-0.03	-0.03	-0.07	-0.03	-0.01	-0.01	-0.06	-0.05	0.01	0.03	-0.03	0.07
	(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)
Last month	-0.02	0.02	-0.07*	-0.01	0.04	0.04	-0.04	-0.01	0.02	0.05	-0.06	0.05
	(0.02)	(0.02)	(0.04)	(0.05)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)	(0.05)
Internet use												
CIUS	-0.03	0.05*	-0.08	0.03	0.01	0.01	-0.06	0.08	0.01	0.06	-0.04	0.16**
	(0.03)	(0.03)	(0.05)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)	(0.08)
Adult websites	-0.03	-0.07***	-0.03	-0.10*	-0.01	-0.01	-0.04	-0.11**	-0.02	-0.01	0.02	0.10
	(0.02)	(0.03)	(0.04)	(0.06)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)	(0.05)	(0.06)	(0.06)
Sexual activity												
No condom	-0.04**	-0.04**	-0.01	-0.03	-0.09***	-0.09***	-0.03	-0.04	-0.06*	-0.06*	-0.03	0.02
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
No consensual	0.00	-0.00	-0.01	-0.00	0.02	0.02	-0.01	0.00	0.02	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Regretted	-0.01	-0.02	-0.00	-0.01	-0.00	-0.00	-0.00	-0.02	-0.01	-0.02	-0.03	0.01
	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Observations	1,832	1,741	760	651	807	789	988	945	844	796	677	632

Each cell in each column shows the OLS coefficient of the young–for–grade dummy from a separate regression. Robust standard errors in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Young–for–grade effects in compulsory education, by school type — All specifications

	Full		Born in 2004		Born in 2003		3rd grade		4th grade		Born in Dec. 2003 vs Jan. 2004	
	Public	Private	Public	Private	Public	Private	Public	Private	Public	Private	Public	Private
Gambling												
Online	-0.02**	-0.00	-0.01	-0.04*	-0.01	0.02	-0.02	-0.04**	-0.02	0.04*	-0.03	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Offline	-0.05***	-0.03	-0.02	-0.03	-0.04	0.01	-0.02	-0.03	-0.06**	-0.03	-0.02	-0.01
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Online/offline	-0.05***	-0.02	-0.03	-0.05	-0.03	0.02	-0.02	-0.04	-0.08***	-0.03	-0.02	-0.01
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Gaming												
Videogames	-0.00	-0.00	0.02	-0.02	0.00	0.02	0.00	-0.02	0.01	0.02	-0.11***	-0.06*
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Play e-sports	-0.00	-0.03	-0.00	-0.06	-0.01	-0.00	-0.01	-0.05	-0.01	-0.01	0.01	-0.05
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Watch e-sports	-0.03	0.02	-0.04	0.00	-0.01	0.04	-0.05	0.01	-0.02	0.05	-0.05	0.00
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Non-prescribed tranquilizers												
Lifetime	-0.02	0.00	-0.01	-0.01	-0.05**	-0.01	-0.01	-0.02	-0.03	0.00	-0.02	0.03
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Last year	-0.01	-0.00	0.00	-0.02	-0.03*	-0.00	-0.00	-0.03*	-0.02	0.02	-0.02	0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Last month	0.00	-0.01	0.03	-0.01	-0.01	-0.00	0.01	-0.03**	0.00	0.01	0.00	0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Last month, daily	0.00	-0.01	0.01	0.00	-0.00	-0.00	0.01	-0.01	-0.01	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.01)	(.)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(.)
Alcohol												
Lifetime	-0.08***	-0.12***	-0.12***	-0.17***	-0.02	-0.11***	-0.06*	-0.16***	-0.08**	-0.10***	-0.00	0.03
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)	(0.05)
Last year	-0.08***	-0.12***	-0.15***	-0.18***	-0.01	-0.12**	-0.08**	-0.18***	-0.08**	-0.09**	0.03	0.04
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)	(0.05)
Last month	-0.06***	-0.10***	-0.10**	-0.10**	-0.00	-0.10**	-0.05	-0.09**	-0.07*	-0.13***	0.03	0.10**
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Last month, w/ ED	-0.03*	-0.02	-0.06**	-0.03	0.02	-0.04	-0.03	-0.03	-0.02	-0.04	-0.00	0.02
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Marijuana												
Lifetime	-0.06***	-0.02	-0.05	-0.03	-0.06*	-0.02	-0.06**	-0.06*	-0.06*	0.02	0.05	0.12***
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Last year	-0.04**	-0.02	-0.04	-0.04	-0.04	-0.03	-0.03	-0.06*	-0.05	-0.01	0.06	0.08**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
Last month	-0.02	-0.02	-0.02	-0.04	-0.03	-0.03	-0.03	-0.06**	-0.03	-0.01	0.03	0.04
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Tobacco												
Lifetime	-0.05**	-0.07***	-0.07*	-0.06	-0.06*	-0.06	-0.06*	-0.07*	-0.03	-0.10**	0.08	0.06
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Last year	-0.05**	-0.06**	-0.06*	-0.07*	-0.04	-0.07*	-0.06**	-0.06	-0.03	-0.11**	0.06	0.04
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)	(0.04)
Last month	-0.03*	-0.08***	-0.08***	-0.09**	-0.00	-0.09**	-0.08***	-0.10***	-0.00	-0.09**	0.08*	0.01
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Last month, daily	0.00	-0.04***	-0.03*	-0.03	-0.02	-0.03	-0.01	-0.06***	0.01	-0.04	0.00	0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Vaping												
Lifetime	-0.03	-0.04	-0.01	-0.10**	-0.02	-0.10**	-0.03	-0.08*	-0.03	-0.01	0.00	0.02
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Last year	-0.04*	-0.03	-0.05	-0.07	-0.02	-0.07	-0.05	-0.06	-0.02	-0.02	-0.02	0.01
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Last month	-0.00	-0.01	-0.02	-0.06	0.01	-0.06	-0.01	-0.06*	0.02	0.00	-0.02	-0.00
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Internet use												
CIUS	0.01	0.05*	-0.02	-0.01	0.05	-0.01	0.01	0.01	0.05	0.04	0.06	0.05
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)	(0.05)
Adult websites	-0.05***	-0.05**	-0.06	-0.08*	-0.03	-0.08*	-0.06**	-0.09**	-0.04	-0.01	-0.00	0.06
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)	(0.04)
Sexual activity												
No condom	-0.04***	-0.04**	-0.02	-0.02	-0.07***	-0.02	-0.04*	-0.03	-0.06**	-0.07**	-0.00	-0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
No consensual	-0.01	0.01**	-0.01	-0.00	-0.01	-0.00	-0.01	-0.00	-0.01	0.03**	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Regretted	-0.02*	-0.01	-0.01	-0.01	-0.00	-0.01	-0.02	-0.02	-0.03	0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	2,195	1,378	831	580	945	651	1,193	740	1,002	638	742	567

Each cell in each column shows the OLS coefficient of the young–for–grade dummy from a separate regression. Robust standard errors in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Average outcomes by treatment status and birth year — High school

	Born in 2002 (17 years old)			Born in 2001 (18 years old)		
Variables	Young–for–grade	Old–for–grade	Diff.	Young–for–grade	Old–for–grade	Diff.
Gambling						
Online	0.028	0.075	-0.047***	0.056	0.069	-0.013
	(0.008)	(0.012)		(0.011)	(0.012)	(0.016)
Offline	0.109	0.142	-0.032	0.146	0.238	-0.092***
	(0.015)	(0.016)		(0.017)	(0.021)	(0.027)
Online/ offline	0.112	0.160	-0.048**	0.149	0.242	-0.094***
	(0.014)	(0.016)		(0.016)	(0.020)	(0.026)
Gaming						
Video games	0.791	0.800	-0.009	0.759	0.742	0.017
	(0.019)	(0.018)		(0.019)	(0.020)	(0.028)
Play e-sports	0.409	0.444	-0.035	0.372	0.360	0.011
	(0.022)	(0.022)		(0.022)	(0.022)	(0.031)
Watch e-sports	0.284	0.301	-0.017	0.257	0.282	-0.025
	(0.020)	(0.020)		(0.020)	(0.021)	(0.029)
Non-prescribed tranquilizers						
Lifetime	0.091	0.065	0.026	0.088	0.101	-0.013
	(0.013)	(0.011)		(0.013)	(0.014)	(0.019)
Last year	0.077	0.050	0.027*	0.059	0.084	-0.025
	(0.012)	(0.009)		(0.011)	(0.013)	(0.017)
Last month	0.038	0.017	0.021**	0.022	0.050	-0.028**
	(0.009)	(0.006)		(0.007)	(0.010)	(0.012)
Last month, daily	0.022	0.008	0.015*	0.014	0.021	-0.007
	(0.007)	(0.004)		(0.005)	(0.007)	(0.009)
Alcohol						
Lifetime	0.830	0.832	-0.003	0.880	0.893	-0.013
	(0.017)	(0.016)		(0.015)	(0.014)	(0.020)
Last year	0.803	0.815	-0.013	0.862	0.880	-0.018
	(0.018)	(0.017)		(0.016)	(0.015)	(0.022)
Last month	0.634	0.674	-0.040	0.696	0.773	-0.077***
	(0.022)	(0.021)		(0.021)	(0.020)	(0.029)
Last month, w/ ED	0.159	0.176	-0.017	0.173	0.187	-0.013
	(0.016)	(0.017)		(0.017)	(0.018)	(0.025)
Marijuana						
Lifetime	0.292	0.316	-0.025	0.367	0.384	-0.017
	(0.021)	(0.021)		(0.022)	(0.023)	(0.032)
Last year	0.244	0.269	-0.025	0.295	0.317	-0.023
	(0.020)	(0.020)		(0.021)	(0.022)	(0.030)
Last month	0.119	0.157	-0.038*	0.143	0.186	-0.043*
	(0.015)	(0.016)		(0.016)	(0.018)	(0.024)
Tobacco						
Lifetime	0.419	0.441	-0.022	0.459	0.434	0.025
	(0.022)	(0.022)		(0.023)	(0.023)	(0.032)
Last year	0.364	0.370	-0.006	0.376	0.356	0.019
	(0.022)	(0.021)		(0.022)	(0.022)	(0.031)
Last month	0.244	0.255	-0.011	0.258	0.262	-0.004
	(0.020)	(0.019)		(0.020)	(0.020)	(0.029)
Last month, daily	0.068	0.060	0.008	0.085	0.091	-0.006
	(0.011)	(0.011)		(0.013)	(0.013)	(0.018)
Vaping						
Lifetime	0.398	0.420	-0.022	0.406	0.388	0.018
	(0.022)	(0.022)		(0.022)	(0.022)	(0.032)
Last year	0.305	0.325	-0.020	0.294	0.296	-0.002
	(0.021)	(0.021)		(0.021)	(0.022)	(0.030)
Last month	0.095	0.096	-0.001	0.090	0.099	-0.009
	(0.014)	(0.013)		(0.013)	(0.014)	(0.020)
Internet use						
CIUS	0.616	0.552	0.064**	0.177	0.238	-0.006
	(0.022)	(0.022)	(0.031)	(0.022)	(0.023)	(0.032)
Adult websites	0.468	0.466	0.002	0.470	0.547	-0.077**
	(0.023)	(0.022)	(0.031)	(0.023)	(0.023)	(0.032)
Sexual activity						
No condom	0.122	0.164	-0.042*	(0.017)	(0.020)	-0.061**
	(0.015)	(0.016)	(0.022)	0.008	0.011	(0.026)
No consensual	0.010	0.015	-0.005	(0.004)	(0.005)	-0.002
	(0.005)	(0.005)	(0.007)	0.061	0.066	(0.006)
Regretted	0.071	0.079	-0.008	(0.011)	(0.011)	-0.004
	(0.012)	(0.012)	(0.017)	0.582	0.589	(0.016)
Observations	499	525		491	477	

The table shows mean values for each variable for young– and old– for grade students and respective difference–in–means tests, separately for cohorts born in 2002 and 2001.

Table A.11: Young–for–grade effects in high school — All specifications

	Full	Born in 2002	Born in 2001	1st grade	2nd grade	Born in Dec. 2001 vs Jan. 2002
Gambling						
Online	-0.03***	-0.06***	-0.01	-0.04**	-0.02	-0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Offline	-0.06***	-0.04	-0.07**	-0.04	-0.07*	0.01
	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)
Online/offline	-0.06***	-0.06**	-0.07**	-0.06**	-0.07*	0.01
	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)
Gaming						
Videogames	0.04*	0.03	0.06*	0.04	0.05	-0.05
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
Play e-sports	0.00	0.02	0.05	0.01	0.04	-0.03
	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Watch e-sports	-0.02	0.00	0.00	0.00	-0.01	-0.03
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Non-prescribed tranquilizers						
Lifetime	0.00	0.01	0.00	0.00	-0.01	0.03
	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
Last year	-0.01	0.01	-0.02	0.00	-0.04	0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Last month	-0.01	0.01	-0.04**	-0.00	-0.04*	-0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Last month, daily	-0.00	0.00	-0.00	-0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Alcohol						
Lifetime	-0.03*	-0.01	-0.03	-0.02	-0.05*	0.02
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Last year	-0.04**	-0.03	-0.03	-0.02	-0.05	0.00
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Last month	-0.10***	-0.06	-0.12***	-0.07**	-0.13***	-0.03
	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Last month, w/ ED	-0.04**	-0.03	-0.01	-0.04*	-0.01	-0.06*
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Marijuana						
Lifetime	-0.04*	-0.03	-0.00	-0.02	-0.05	-0.01
	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Last year	-0.04**	-0.02	-0.02	-0.01	-0.07	-0.02
	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Last month	-0.05***	-0.03	-0.03	-0.03	-0.05	-0.04
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Tobacco						
Lifetime	-0.02	-0.04	0.05	-0.01	0.02	-0.03
	(0.02)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
Last year	-0.01	-0.02	0.03	-0.00	-0.02	-0.04
	(0.02)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
Last month	-0.03	-0.02	-0.02	-0.01	-0.05	-0.08*
	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Last month, daily	-0.02	-0.01	-0.01	-0.02	-0.02	0.02
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Vaping						
Lifetime	-0.02	-0.03	0.04	-0.03	0.04	-0.06
	(0.02)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
Last year	-0.03	-0.03	0.02	-0.03	0.01	-0.09**
	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Last month	-0.03*	-0.02	-0.03	-0.02	-0.03	-0.04*
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Internet use						
CIUS	0.03	0.08*	-0.04	0.05	-0.02	-0.01
	(0.02)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
Adult websites	-0.03	0.02	-0.04	0.02	-0.05	0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Sexual activity						
No condom	-0.05***	-0.03	-0.05	-0.04	-0.06	-0.01
	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)
No consensual	-0.00	-0.00	-0.01	-0.00	-0.01	-0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Regretted	-0.01	0.01	-0.02	0.00	-0.04	-0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	2,089	1,024	968	1,298	791	928

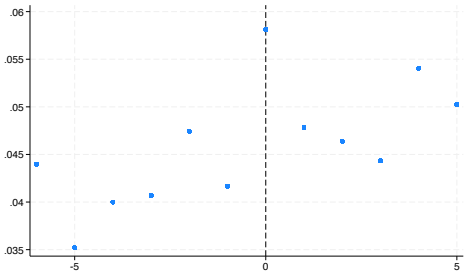
Each cell in each column shows the OLS coefficient of the young–for–grade dummy from a separate regression. Robust standard errors in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Supplementary figures

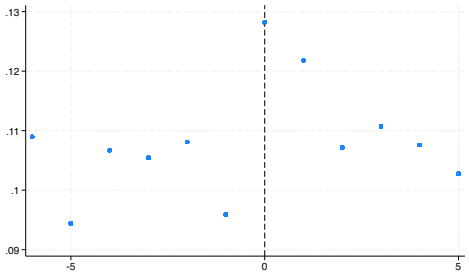
B.1 Descriptive evidence of the effect of the cutoff on risky behaviors

Each dot in the graphs represents the average value of the outcome for students born in the month, ordered by normalized month of birth, which ranges from −6 (for students born in July) to 5 (for students born in June) and takes value 0 for students born in January.

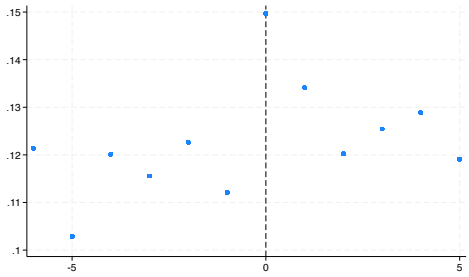
Gambling



Online

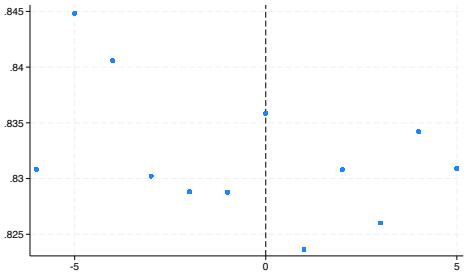


Offline

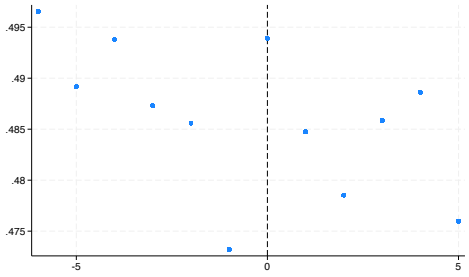


Online/ offline

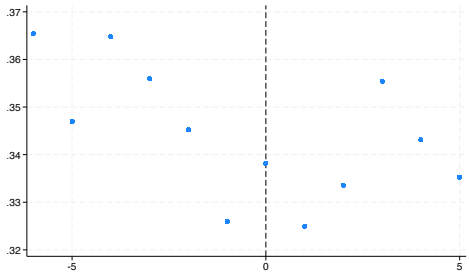
Gaming



Video games

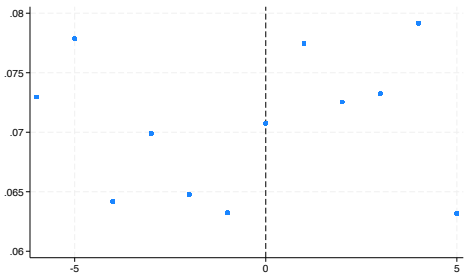


Play e–sports

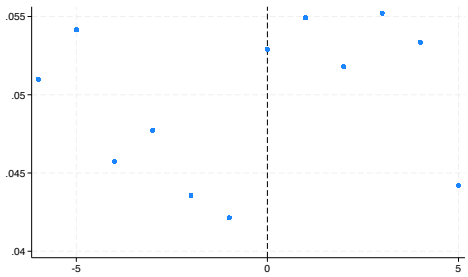


Watch e–sports

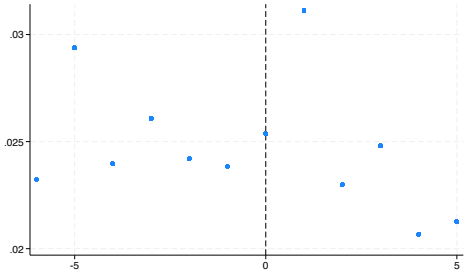
Non-prescribed tranquilizers' use



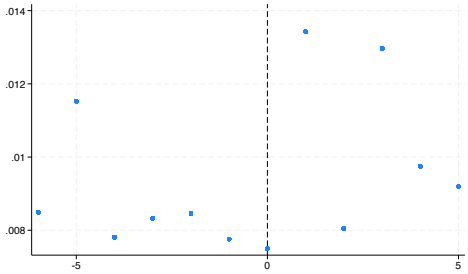
Lifetime



Last year

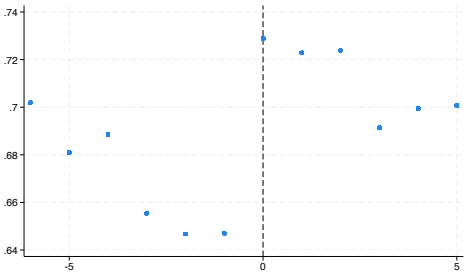


Last month

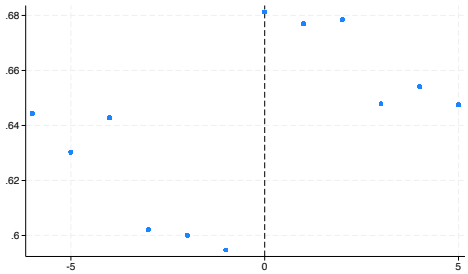


Last month, daily

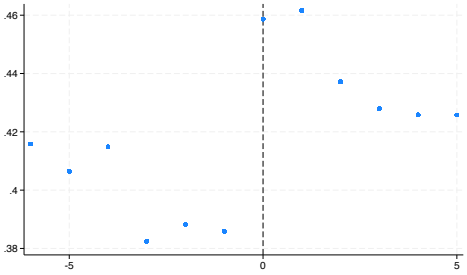
Alcohol



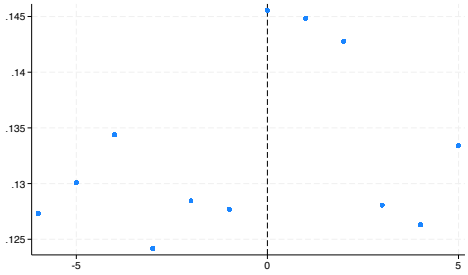
Lifetime



Last year

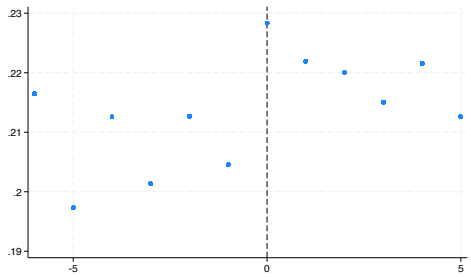


Last month

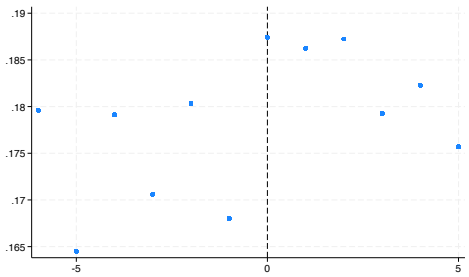


Last month, w/ energy drink

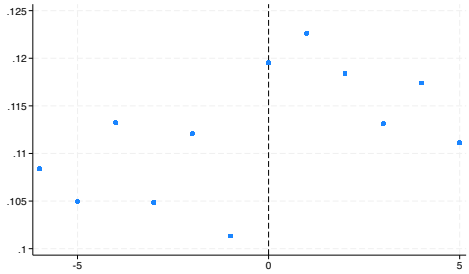
Marijuana



Lifetime

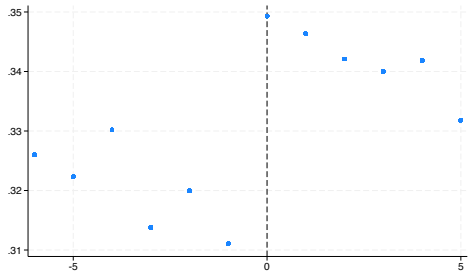


Last year

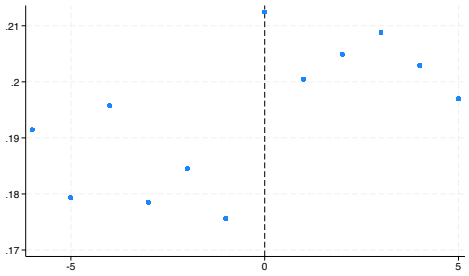


Last month

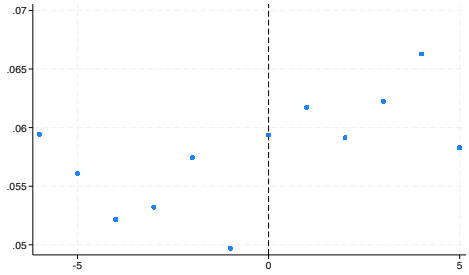
Tobacco



Lifetime

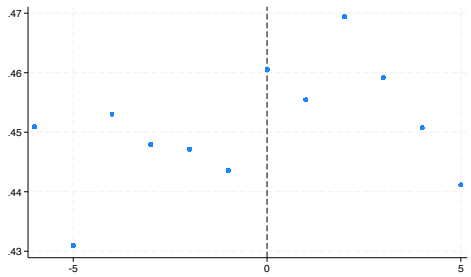


Last month

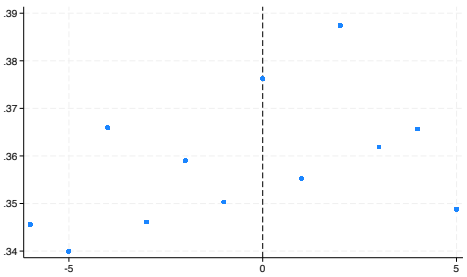


Last month, daily

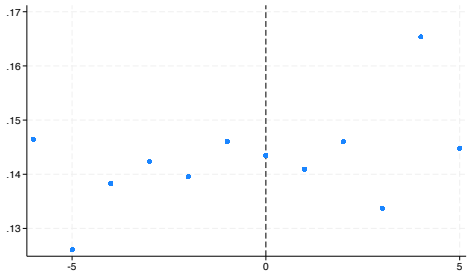
Vaping



Lifetime

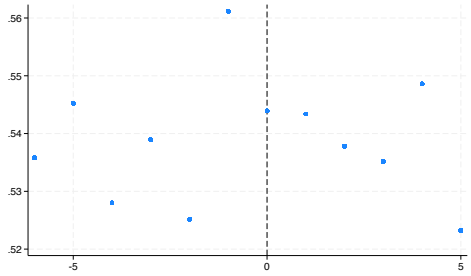


Last year

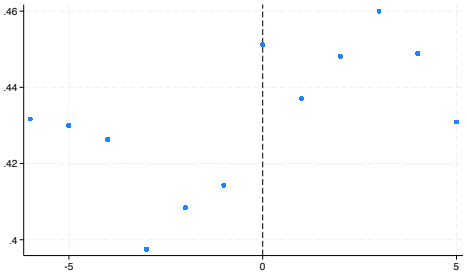


Last month

Internet use

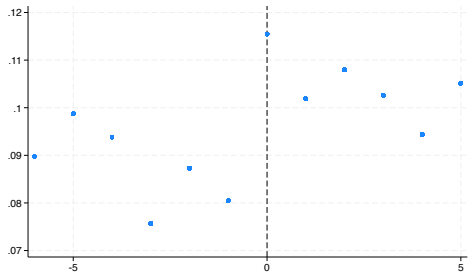


CIUS

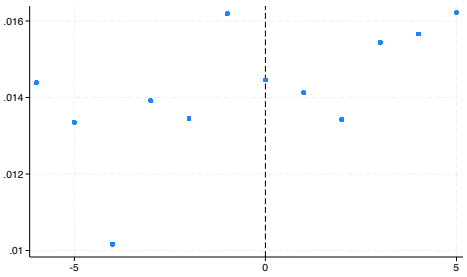


Adult websites

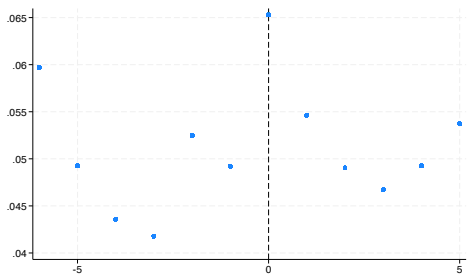
Sexual activity



No condom



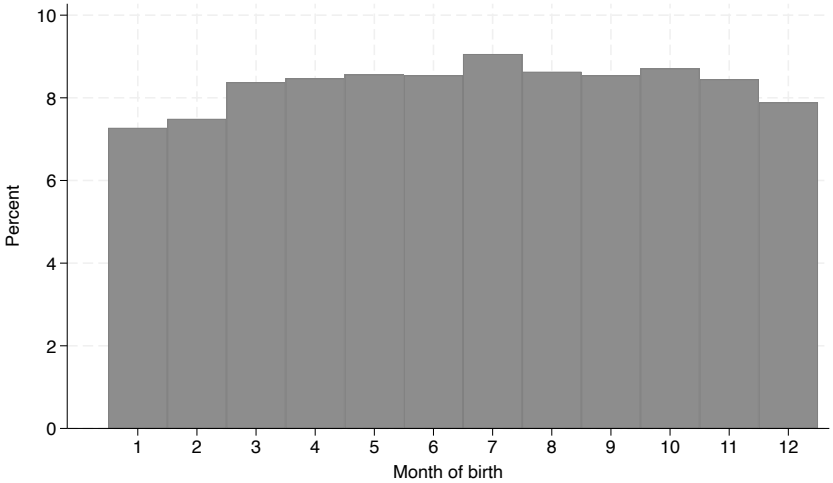
No consent



Regretted

B.2 Other figures

Figure B.2.1: Distribution of month of birth - Students enrolled in high school



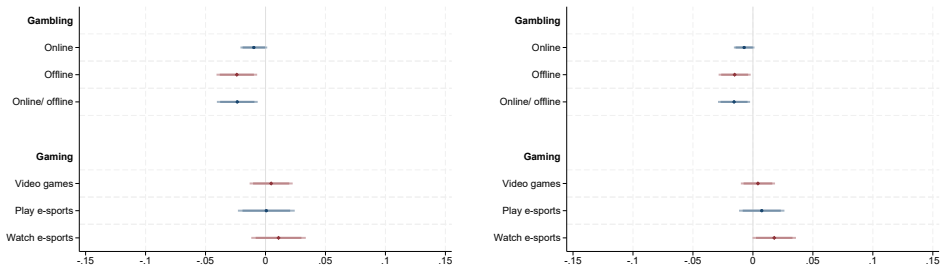
Note: SSSDU 2018 wave, N = 13,777 students.

B.3 Different bandwidths

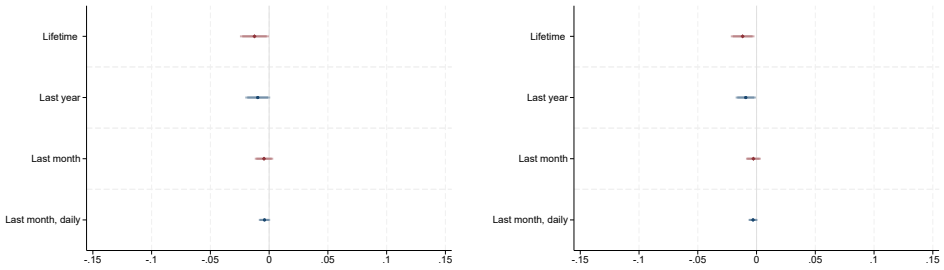
Figure B.3.1 shows the OLS estimates of the young–for–grade (YFG) dummy obtained using the compulsory students sample with different bandwidths around the cutoff date. We consider a two–month and three–month bandwidth and estimate equation (1) in the main text by pooling students from all birth years and grades. When we use a two–month bandwidth, the treated group (young–for–grade) are the students born in November and December, and the control group (old–for–grade) are the students born in January and February. When we use a three–month bandwidth, the treated group are those born from October to December and the control group, the ones born from January to March.

Figure B.3.1: Young–for–grade results using different bandwidths — Full specification

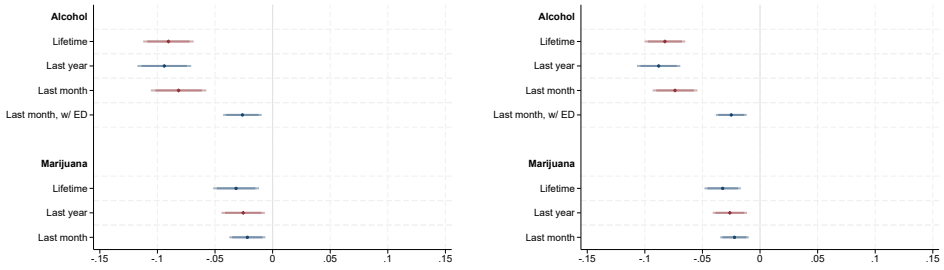
2–month bandwidth 3–month bandwidth



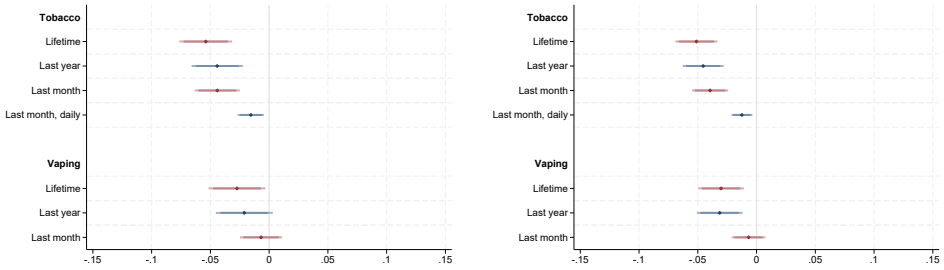
(a) Gambling and gaming



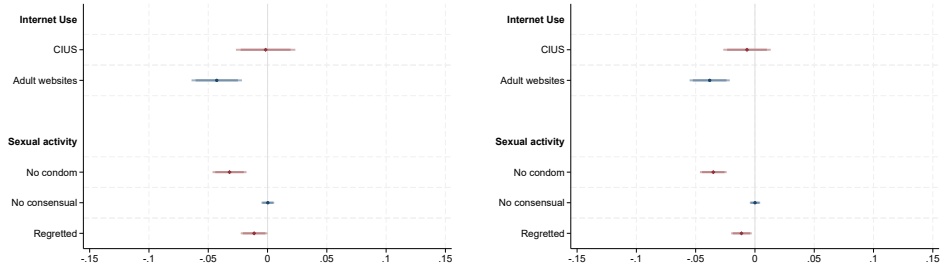
(b) Non–prescribed tranquilizers' use



(c) Alcohol and marijuana use



(d) Tobacco and vaping

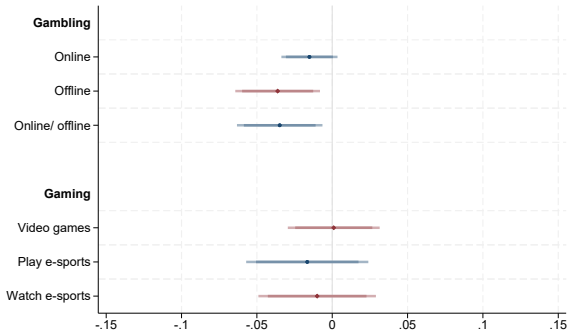


(e) Other risky behaviors

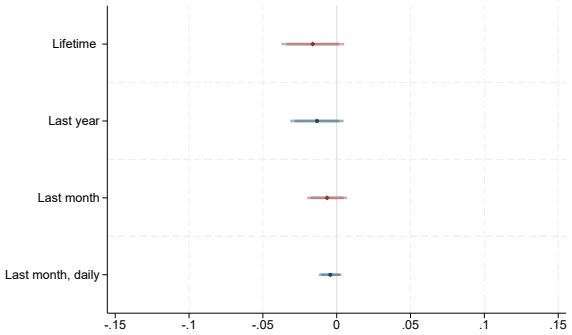
Note: 6,912 students using a 2–month bandwidth; YFG are students born in November and December, and OFG are students born in January and February. 10,491 students using a 3–month bandwidth; YFG are born from October to December, and OFG from January to March.

B.4 Regression Discontinuity Design

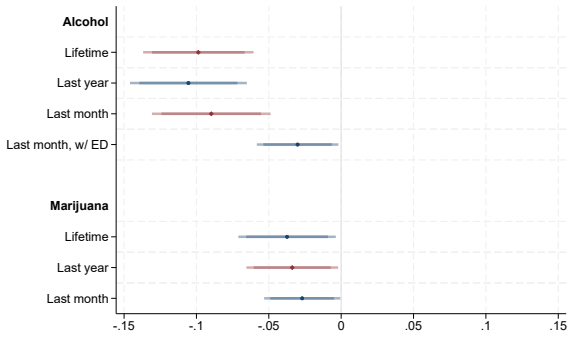
Figure B.4.1: Young–for–grade results using a 3–month bandwidth — Full specification



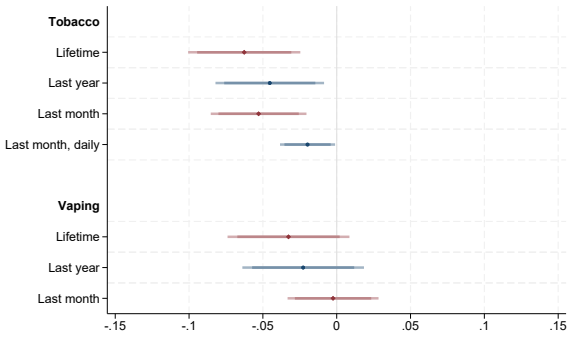
(a) Gambling and gaming



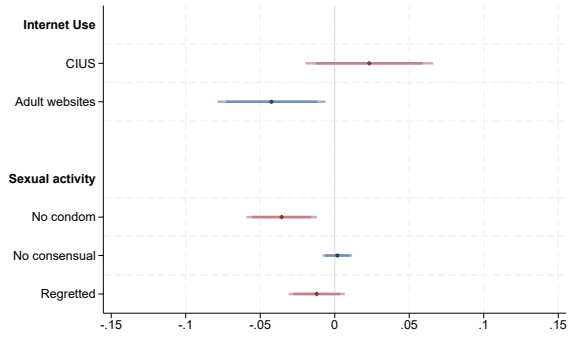
(b) Non–prescribed tranquilizers' use



(c) Alcohol and marijuana use



(d) Tobacco and vaping



(e) Other risky behaviors

Note: $N = 10,491$ students. 5,173 old–for–grade students (born from January to March); 5,318 young–for–grade students (born from October to December). One separate estimation for each outcome. All regressions control for a linear function of the running variable, socio–demographic variables, grade–retained dummy, and birth year, grade and school fixed effects.