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School Entry Age Policy and Adolescent Risk-Taking

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Abstract

This paper investigates the impact of the school entry age policy on adolescent risk–taking behaviors. The policy mandates that children begin primary education in the year they turn six, creating relative age differences within cohorts due to a January 1st cutoff date. Using data from the Spanish School Survey on Drug Use, we analyze a comprehensive set of risky behaviors, including substance use, gambling, gaming, internet use, and sexual activity among students in the early adolescence in compulsory education. Employing an empirical strategy that compares students born in December (young–for–grade) and January (old–for–grade) while controlling for potential confounders, we find that young–for–grade students are less likely to engage in risky behaviors. Findings are consistent across various robustness checks. Further analysis suggests that both absolute age differences and educational cycle effects contribute to these findings. Gender–specific patterns reveal distinct effects for boys and girls, while school type shows limited variation. Notably, most behavioral differences diminish 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, school entry age, young–for–grade and old–for–grade students, education policy

JEL Classification: 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 just before and after the cutoff can differ in age by up to twelve months, leading to significant variations in cognitive development and maturity among cohort–mates, potentially impacting their academic performance. Many studies find that, compared to older students in the cohort, younger pupils have lower test scores (Bedard and Dhuey, 2006; Sprietsma, 2010; Crawford et al., 2014; Peña, 2017; Dhuey et al., 2019) and higher retention rates (Manacorda, 2012; Bedard and Dhuey, 2006; Jerrim et al., 2022). The evidence on longer–term effects on human capital development is mixed. Some studies find that the educational disadvantage of younger students fades out as students advance to higher grades and has no effect neither on the probability of graduating from college (Oosterbeek et al., 2021) nor on adult labor market outcomes, such as wages (Dobkin and Ferreira, 2010). In contrast, Bedard and Dhuey (2006) show that the academic disadvantage does not disappear and leads to lower university attendance for younger students.

The disparities in maturity and cognitive development caused by school entry cutoffs likely extend beyond academic performance. However, while broad research exists on the academic effects of the school entry age, studies on the non–academic impact are comparatively limited. Most of these studies have focused on the attention deficit and hyperactivity disorder (ADHD) and find that younger students suffer from systematically higher ADHD diagnoses rates (Elder (2010) and Nicodemo et al. (2024) for UK; Layton et al. (2018) for the US; Schwandt and Wuppermann (2016) for Germany). Some authors have explored the effect on psychological traits, such as self–confidence and leadership, finding that older students have higher confidence in their academic competence (Crawford et al., 2014) and are more likely to be high school leaders (Dhuey and Lipscomb, 2008). An area that remains under–explored is the adoption of risky behaviors, such as substance use and sexual activity. Among the few studies, Shin (2023), for South Korea, finds that younger students are more likely to engage in drinking and sexual intercourse. For Denmark, Johansen (2021) obtains that being among the youngest in a cohort increases the probability of abortion, alcohol poisoning, childbirth, and cohabitation by early 20s for women. For men, however, being younger does not have a significant effect on risky sex and heavy drinking. Argys and Rees (2008) find, for the US, that younger girls are more prone to smoking tobacco and marijuana and drinking alcohol compared to their average grademates, while no such evidence is found for boys.

This study examines whether the Spanish school entry age policy, which induces age differences among cohort–mates who enter school the same academic year, leads to different risky behavior adoption in adolescence. We analyze 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 already over 6 years old. 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.

This study expands the limited existing literature on the effects of school entry age policies on risky behaviors in several key ways. First, we analyze outcomes that have not yet been examined, such as gambling, gaming, non–prescribed tranquilizers' use, vaping and internet use. Second, we provide additional empirical evidence to the still sparse research on other previously analyzed behaviors, such as alcohol use, smoking tobacco and marijuana, and sexual intercourse. Finally, by analyzing the Spanish policy, we broaden the geographical scope of research about the effect of the school entry age policy on risky behaviors to a previously unexamined country. This offers novel insights on the interaction of risky behavior adoption and the entry policy in a social context different from the ones formerly analyzed. Overall, our study contributes to a more comprehensive understanding of how school entry age policies affect outcomes beyond academic performance, which has been the primary focus of existing studies. This expanded perspective is crucial for policymakers considering adjustments to school entry age regulations or to remedial measures aimed at addressing cognitive performance gaps created by the cutoff date. Our findings underscore the importance of studying the wide–ranging, non–academic impacts of the school entry age policy.

We use the the Spanish School Survey on Drug Use (SSSDU), which is a school–based and nationally representative 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. In addition to information on risky behaviors, SSSDU also collects students' socio–demographic data, including birth date, which we use to identify individuals born before and after the cutoff date.

Our empirical strategy compares the adoption of risky behaviors of students born on either side of the school entry cutoff date. A crucial aspect of the Spanish educational context that ensures the comparability of both groups of students is that it prohibits parents from advancing or delaying their child's school entry to compensate for developmental differences. This policy feature eliminates this source of selection bias in the timing of a child's school entry. 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 around the cutoff and estimate by OLS the effect of being young–for–grade on

the adoption of risky behaviors using the sample of students born in January and December. As a robustness check, we use two– and three–month bandwidths for the OLS estimation of this effect, and, alternatively, employ a Regression Discontinuity Design (RDD) estimation with a three–month bandwidth.

In the main analysis, we focus on students enrolled in compulsory education in order to disregard that the results may be affected by the potential bias from the positive students' self–selection into upper secondary (post–compulsory) education. We find that young–for–grade students are less likely to engage in risky behavior than old–for–grade ones for most of the outcomes: gambling, drinking alcohol, smoking tobacco and marijuana, vaping, visiting adult websites and risky sexual activity. The estimated effects range from -9 percentage points (pp) for drinking alcohol to -1 pp for online gambling. Our results are robust to dropping socio–demographic controls from the regression specification —as indication of randomness around the cutoff date—, to the use of different bandwidths and to employing an RDD estimation strategy.

In the Spanish educational system, the retention policy allows holding back low–performing students. As a result, around 30% of the students who enter school together, according to their birth year, are held back and do not longer continue with their cohort–mates. These retained students join the cohort that started one year later. Retention decisions mainly take place by the end of primary education and during lower secondary education. At that point, they break the one–to–one link between birth cohort and grade of enrollment. In order to disentangle the extent to which differences in absolute age—i.e., year of birth—and differences in the schooling cycle—i.e., grade of enrollment—may contribute to the overall young–for–grade effect during adolescence, we estimate additional specifications. First, we consider 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 estimating separately by grade. Third, we further examine the role of the schooling cycle by re–estimating the effect on two adjacent cohorts of birth. The resulting evidence suggests that both differences in absolute age and educational cycle are likely to explain the lower propensity for young–for–grade students to engage in risky behavior compared to older students. We also explore whether differences in maturity may be a channel that drives differences in absolute age. We use the available survey information on family rules around social behavior and drug consumption. We find that these rules are stricter for young–for–grade students, which is in line with different maturity development as a potential driver of the main results.

We then explore heterogeneous effects of the likelihood of being young–for–grade on risky behavior by gender and type of school. We find significant results for both boys and girls, but with some differences. The young–for–grade effect on gambling and visiting adult websites is statistically significant only for boys. The effect on drinking alcohol and smoking tobacco is significant for both genders, although of higher magnitude for girls. Being young–for–grade similarly reduces the likelihood of smoking marijuana and engaging in risky sexual activity for boys and girls. We find few differences in the effect of being young–for–grade according to whether students are enrolled

in public or private schools, mainly for gambling, alcohol drinking and smoking tobacco.

Finally, we provide additional evidence of longer–term effects by extending the analysis to students enrolled in high school. Although with some caution in the interpretation due to potential positive selection into this post–compulsory schooling level, results suggest that most significant differences in risky behaviors between young– and old–for grade observed at the early adolescence (15 – 16 years old) in compulsory education disappear in high school, at late adolescence (17 – 18 years old).

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.[1] 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. The school year commences in mid–September, resulting in significant age differences among students. A child born early in the calendar year, "Child 1" in the figure, is over 6 years old when starting school. 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 full year later than Child 2. Despite these age disparities, Spanish policy prohibits parents from advancing or delaying a child's school entry (*redshirting* practice). This policy applies strictly across all Spanish regions, ensuring 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

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

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.[2] The massive 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 is 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, a school–based, nationally representative survey on drug consumption conducted by the Ministry of Health every two years.[3] The target population of SSSDU is the students enrolled in the last two grades of compulsory education (grades 3 and 4 of lower secondary) and those 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.

The SSSDU sample is obtained through 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.[4]

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 paper–and–pencil collection method follow other European drug use surveys.[5]

We use the 2018 wave, which sampled students enrolled at the target grades during the 2018–2019 academic year.[6] In Spain, the academic year typically commences in mid–September and

[2]The report "Sistema estatal de indicadores de la educación 2023", Ministerio de Educación y Formación Profesional, is available here: `https://www.libreria.educacion.gob.es/libro/sistema-estatal-de-indicadores-de-la-educacion-2023_182384/`.

[3]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.

[4]Students with special education needs and recently arrived immigrant students whose mother tongue is not Spanish are surveyed although SSSDU does not include their answers into the data set.

[5]Paper–and–pencil method is less intrusive than other collection techniques and, therefore, it is used to reduce under–reporting in drug use surveys.

[6]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. Compared with other less recent waves, the 2018 wave contains all the relevant information needed for our analysis. Previous waves miss key information, such as questions on internet use, or record information differently, such as in the questions

finishes at 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 wave in question, data collection occurred in 2019, spanning from February 4^{th} to April 5^{th}. 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, this narrow time span (only two months) in the data collection process reduces potential differences in reported behavior that may arise because of the age–at–survey variation induced by differences in the date of the survey.[7] Students' response rate was 97%.

The sample contains 38,010 students allocated to 1,769 classes in 917 schools. From this initial sample, we first exclude the students enrolled in vocational education. Vocational students may be underrepresented due to workplace training commitments, potentially increasing their likelihood of absence on survey days. Excluding vocational students only involves dropping 2,928 students (around 7.7% of the initial sample) as the vocational path is only chosen by a low fraction of students following compulsory education; in Spain, the majority opts for the high school–university track. Second, we exclude few students with inconsistent responses to birth year and retained status.[8] 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 pupils in 1,574 classes and 790 schools, around 92% of the initial sample.

School–based surveys, like SSSDU, have, by design, a limitation, as the target population does not include early school leavers in the post–compulsory education samples.[9] In our case, this implies that the sample of high school students is representative of the schooling population enrolled in that education level, but it is likely to be a positively selected sample of the total population of 17 and 18 years old (the expected ages in high school, see Table 1 and Figure 1). To eliminate this confounding factor, in our main analysis we focus on the students enrolled in compulsory education. Then, in section 4.4 we extend our analysis to the high school students. The compulsory education subsample contains 21,156 students, around 60% of the final sample above.

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.[10] Using the information on the birth year, we can calculate the age that the student turned in 2019, the year when the SSSDU wave was collected. SSSDU unfortunately only identifies

on gambling and gaming behavior.

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

[8]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.

[9]Home–based surveys are less likely to suffer from the issue of selection in post–compulsory education although, since parents are present, drug–use under–reporting may be a more serious concern.

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

that the student is enrolled in compulsory education but it does not specify their grade (third or fourth). However, we can determine this information as follows. We infer students' enrollment grade by first comparing their age in 2019 (calculated from the birth year), to the expected age for third and fourth grades (15 and 16 years, respectively; see Table 1). Notice that some students may not be in the grade predicted by their age. The reason is grade retention due to their school performance. Therefore, we also integrate the data on the birth year and the expected grade with a student's reported retention status, which indicates whether she/he repeated one or two grades in prior years.[11] All together, the information about the retention status, the expected age in each grade, and the turning age in 2019 allows us to build the grade of enrollment for every student.[12]

Table 2 shows the distribution of students by birth year and grade. As expected, students born in 2004 and 2003 comprise the majority of the sample (about 85%, see column (4)). Students turning 15 and 16 in 2019, are at the expected ages for third and fourth grade. Those born in 2002 or earlier are retained students. Columns (5) and (6) show the distribution of students enrolled in third and fourth grade conditional on their birth year. Students born in 2004 are 15 and, therefore, all of them are enrolled in third grade. As SSSDU does not select students enrolled in lower grades, the students born in 2004 who were held back due to poor academic performance are not sampled. We discuss below whether this may be a concern for the analysis. 74% of students born in 2003 are enrolled in the expected (fourth) grade, while 26% are retained students enrolled in third grade. Because of the sampling design, the two–year retained students from the 2003 cohort are not observed (they were enrolled in lower grades). However, this is a less serious concern since two–year retention is infrequent (the two–year retention rate in the sample is less than 4%). Finally, columns (7) and (8) present the distribution of the students' birth cohort conditional on their grade of enrollment. In both third and fourth grade, 73% and 74% of students are at their expected age, respectively, indicating similar retention rates across grades (27% and 26%, respectively). Retention rates in SSSDU are in line with retention rates in Spain, which is among the OECD countries with the highest retention rates.[13]

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.1 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

[11] Students can be held back up to twice in compulsory education.

[12] Notice that there are no differences in the students' starting age. All students begin school in the calendar year in which they turn 6 and *redshirting* is not allowed.

[13] According to OECD (2020), using PISA 2018 data, the retention rate among 15 year–old students is around 29% in Spain, while the OECD average is 11%.

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.1). Another outcome of 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.

2.3 Descriptive evidence

Figure 3 shows that the birth month distribution of students in compulsory education is nearly uniform, suggesting no parents' manipulation of their fertility decisions around the cutoff date. Figure 4 corroborates this finding. It presents the results from the balance tests 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 on both sides of 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 advancing or delaying school entry to adjust for disparities in the child's birth month. 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 significant 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. Figure 4 shows the balance tests obtained using a one–month distance on either side of the cutoff (i.e. comparing students born in December with students born in January), but the findings hardly change if we use two— and three–month bandwidths (results are available upon request).[15]

Figures in section B.1 in Appendix B provide a preliminary graphical analysis of the effect of the cutoff date for each outcome. Each dot in the plots represents the average outcome value for

[14] Moreover, Table A.2 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.

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 indicate an upward shift in risky behaviors at the cutoff with January–born students generally showing higher rates of risky behaviors compared to December–born.

Finally, Table 3 shows the proportion of students engaging in each risky behavior in the full sample and in the subsamples of students born in December (young–for–grade) and in January (old–for–grade). The last column displays the results of differences–in–means tests between these subgroups. Positive significant differences emerge for gambling, alcohol use, tobacco use and sexual activity, indicating higher prevalence of these behaviors among old–for–grade compared to young–for–grade students.

The evidence provided so far indicates that being young–for–grade is negatively associated with the adoption of risky behaviors among students enrolled in the last grades of compulsory education, who are approximately in the early adolescence (15 –16 years old). In the next sections, we present the econometric analysis and estimation results.

3 Empirical strategy

The school entry cutoff determines treated and control groups in the analysis. The treated group consists of the students born below the cutoff date; they are the young–for–grade students. The control group consists of the students born above the cutoff; they are the old–for–grade students. 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:

$$y_{is} = \alpha + \beta youngforgrade_{is} + \gamma X_{is} + \delta_i + \phi_s + \theta_i + \varepsilon_{is} \tag{1}$$

where y_{is} is the outcome for student i in school s; $youngforgrade_{is}$ is a dummy variable equal to one if student i in school s is born one month below the cutoff (i.e., in December) and 0 is she/he is born one month above the cutoff (i.e., in January); X_{is} is the vector of control variables that includes the socio–demographic characteristics and the grade–retained dummy variable; δ_i is a vector of birth year fixed effects; ϕ_s is a vector of school fixed effects; and θ_i 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_i) and birth year (δ_i) fixed effects are incorporated to control for potential unobserved 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 throughout the schooling cycle). Finally, our specification 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. We should remark that the socio–demographic variables should not play an essential role in driving the results, as suggested by the balance tests shown in Figure 4. Indeed, in the robustness section, we estimate equation (1) without controlling for student's socio–demographic characteristics and we get the same results, which is a good indication of randomness around the cutoff date. Our parameter of interest, β, captures the average effect of being born in December as opposed to January on the propensity to engage in the outcome in question among the students enrolled in the same school and conditional on grade of enrollment and on the birth year. We estimate equation (1) separately for each outcome. The estimation method is OLS, which, in this setting, it 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).

In addition to estimating equation (1) by pooling all students across birth years and grades, we also estimate three alternative specifications to gain deeper insights into the effect of being young–for–grade on risky behaviors. These specifications are motivated by the high rate of students being held back, around 27% as shown in Table 2, which are the consequence of the Spanish retention policy. Retained students do not longer continue their schooling journey with their cohort–mates, that is, with the students they enter school with; instead, they are incorporated to the cohort that began one year later. The retention policy, thus, breaks the identification of birth year with grade of enrollment. 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 of enrollment—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 focus on these cohorts as they turn 15 and 16 years old in 2019, respectively, which are the expected ages in the SSSDU targeted grades, as shown in Table 1.[16] The separate regressions by birth year follow the same specification as equation (1), with the exclusion of the birth year fixed effects.[17] Interpreting these regression results requires considering several factors. First, as explained above, retention decisions break the one–to–one link between

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

[17]The regression for the 2004 cohort does not include the grade–retained dummy and the grade fixed effects, because all the observed students from the 2004 cohort are non–repeaters enrolled in third grade, the expected grade. Repeaters from this cohort are enrolled in lower, not surveyed, grades (see Table 2).

birth cohort and grade of enrollment. Retained students from the 2003 and 2004 cohorts no longer align with their cohort–mates' educational cycle, which means that they are not observed in the expected grade given by 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 discussed in section 2.2, one– and two–year retained students from the 2004 cohort and two–year retained students from the 2003 cohort are not observed in the data set due to the survey design. This creates a positive selection bias 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 in both cohorts, 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, we discuss how these factors may impact our results.

The second specification involves separate estimations for students in third and fourth grade. These regressions follow the same specification as equation (1), excluding only the grade fixed effects. This approach aims at analyzing the extent to which differences in the schooling cycle may influence the overall effect of being young–for–grade. In order to interpret the results from this second approach, we should take into account 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 at the expected age, while the others are repeaters. 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, despite they did not start school together (since retained students began earlier). Unlike this second approach, in the first one, we estimate that effect among the students who belong to the same birth cohort and, therefore, entered school together, although, at the time of the survey, some of them no longer share the educational cycle. Notice that in the absence of retention decisions, both approaches would be equivalent, as the cohort of students who begin school according to their birth year would continue together throughout all schooling years.

Finally, our third specification compares students with similar absolute ages but who entered school in different years due to the cutoff. We do this for students born in December 2003 and in January 2004 as this is the only viable comparison group. 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– and January 2004– born students belong to different educational cycles, with January 2004 students entering school a year later. Therefore, the purpose of this third approach is to obtain additional insights into how the educational cycle may drive the results. 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 January 2004 and zero if she/he

is born in December 2003. Consequently, this specification excludes birth year and grade fixed effects. In this specification, we exclude the retained students born in December 2003 to avoid this confounding factor in the comparison, as these retained students were held back one year and became grade–mates of the 2004–born students who are enrolled in third grade.[18]

4 Results

4.1 Main estimation results

Figure 5 shows the OLS estimate of the young–for–grade dummy in equation (1) for each outcome, pooling all students enrolled in third and fourth grades, across all birth years. The first column in Table A.3 in Appendix A reports all the coefficients and robust standard errors. When young–for–grade is significant, it always takes on a negative value, indicating December–born students are less prone to engage in risky health behaviors than students born in January. We observe this lower propensity in almost all behaviors: gambling (both online and offline), drinking alcohol, smoking tobacco and marijuana, vaping, navigating adult websites, and having a risky sexual activity (i.e. without using a condom) or intercourse later regretted. The effect ranges from -9 pp for drinking alcohol in the last year to -1 pp for online gambling and the strongest effects are found for using alcohol and tobacco, and visiting adult websites. We do not find significant differences in the use of non–prescribed tranquilizers, gaming and compulsive internet use. Our findings contrast with Shin (2023), a closely related study—as it uses data similar to SSSDU—, that finds that young–for–grade students are more likely to drink and engage in sexual intercourse. A possible explanation is the different social context. For instance, in Spain, adolescents show higher rates of alcohol use than in South Korea. On average, 69% and 42% of the full sample have used alcohol, respectively, in their lifetime and in the last month (see Table 3), while these figures are 47% and 18% among South Korean adolescents (see Table 1 in Shin (2023)). These differences might reflect more rigid social norms in South Korea, potentially making norm deviation more attractive for young–for–grade students there than in Spain.

As outlined in the empirical strategy section, we also estimate alternative specifications aimed at disentangling the extent to which the overall estimated effects are driven by differences in absolute age and the schooling cycle as a result of retained decisions breaking the one–to–one link between birth cohort and grade of enrollment around lower secondary education. First, to explore the role of differences in absolute age, we estimate the specification explained in section 3, separately for the cohorts of students born in 2004 and 2003 (turning 15 and 16, respectively, in 2019). Figure 6 presents the results and columns two and three of Table A.3 in Appendix A report the OLS estimates. Similar to Figure 5, when the coefficients are significant, they are negative, pointing towards young–for–grade students being less likely to adopt risky behaviors. However,

[18] Results hardly change when we include retained students born in December 2003, and add the grade–retained dummy as control. They are available upon request.

results 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 consistent across cohorts, and new differences emerge in the old cohort for the use of non–prescribed tranquilizers and unprotected sexual activity.

Second, we explore the role of the schooling cycle by separately estimating the effect of being young–for–grade for third and fourth grade students, as explained in section 3. Figure 7 shows the results and columns four and five of Table A.3 in Appendix A report the OLS estimates. These results largely mirror those found for the separate estimation by birth year in Figure 6, with the few exceptions observed in gambling, alcohol and marijuana use. By comparing the results from the 2004 cohort with those from third graders—the expected grade for the 2004–born students—we observe that third graders exhibit smaller differences between the young– and old–for–grade students in alcohol and larger differences in marijuana. Fourth graders exhibit more marked and significant differences in gambling behavior and alcohol consumption compared to the 2003 cohort, while differences in marijuana use become non–significant for the fourth graders.

Finally, Figure 8 presents the results from a third approach that compares students born in December 2003 with those born in January 2004. OLS estimates are reported in column six of Table A.3 in Appendix A. 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 significant differences between these two groups. Students born in January 2004, who are among the oldest in their academic cohort, are less likely to use alcohol in the last month, to smoke marijuana (in lifetime or last year) and to use internet compulsively than students born in December 2003, who are among the youngest in their academic cohort. The former are also more likely to play video games than the latter.

Altogether, findings from the separate estimation by birth year and grade, and from the comparison of the students born in December 2003 and January 2004, show that the effect of being young–for–grade is driven both by differences in absolute age and in the schooling cycle. From the separate estimation by year of birth, we observe that most of the significant differences in behaviors due to the month of birth do not remain, or drop substantially, between ages 15 and 16. This suggests that differences in absolute age are a driver of the observed differences in risky behaviors between relatively young and old, and as students age, young–for–grade tend to catch up old–for–grade students. As shown in Table A.5, the adoption rate of young–for–grade is higher for those aged 15 compared to those aged 16 years old in most behaviors. The average prevalence among old–for–grade students also increases but at a lower rate for some behaviors such as gambling, alcohol and vaping, resulting in young–for–grade catching up old–for–grade as they age. Estimation results above show that some new differences appear, such as in risky sex

and using tranquilizers ever, which may be explained because these behaviors are more prevalent at older ages and the gap broadens, as shown in Table A.5.

However, when interpreting the results by birth year, we should make some considerations. As explained in section 2.2, the sampled 2004 cohort suffers from some degree of positive selection since 2004–born students retained due to poor academic performance are enrolled in lower, not surveyed, grades. In the case of the 2003 cohort, this positive selection is less pronounced as we do observe one–year retained students because they are in third grade. Only the two–year retained students are not surveyed, but, as explained above, the proportion of students retained two years is quite low in compulsory education. The lack of one–year retained students in the 2004 cohort compared to the 2003 cohort may introduce a higher degree of academic selection in the former. For instance, if low–performing students are more likely to engage in risky behaviors, the observed students from the 2004 cohort would exhibit an overall lower adoption rate. This may bias the effect of being young–for–grade, and thus the comparison across cohorts, since the retention rate is not balanced among young– and old–for–grade students (see Figure 4), which is a common finding in the literature.

To shed more light on this issue, Table 4 shows the percentage of old–for–grade and young–for–grade in the sample by birth year. Young–for–grade are over–represented in the cohorts born in 2002 or earlier, which exclusively include retained students (see columns (3) and (5)). For instance, in the 2002 cohort, 8% are old–for–grade while 9.8% are young–for–grade. For the 2003 and 2004 cohorts, however, the pattern is less clear as percentages are quite similar. The proportion is slightly higher for young–for–grade in the 2004 cohort but somewhat lower in the 2003 cohort. Therefore, the concern of a potential bias from the lack of surveyed retained students from the 2004 cohort, if anything, would play a minor role in explaining estimation differences across both cohorts.

A potential channel through which observed differences between young– and old–for–grade by absolute age—birth year—may operate is differences in maturity. To gain more insights into this potential mechanism, we examine the information on family rules collected in the survey and test for any difference between young–for–grade and old–for–grade students. SSSDU asks students about the extent to which their parents set rules at home or outside the home and, for tobacco, alcohol and marijuana, it collects the students' perceptions of whether their parents would allow them to use these substances. In addition, the survey records whether students observe their parents drinking alcohol or whether any household member smokes daily. Figure 9 shows that young–for–grade students are more likely to report that their parents set the rules in the social environment (outside the home) and would not allow them to smoke tobacco or to drink alcohol. Not surprisingly, no difference is found in the students' perceptions about whether their parents would permit them using marijuana, a psychoactive and illegal drug. The reported proportions of parents drinking alcohol and household members smoking are not significantly different between young– and old–for–grade. If parents set rules according to their children's maturity, then the

evidence from this figure suggests that differences in the degree of maturity between young– and old–for–grade, as reflected by differences in parents' rules, are a possible mechanism behind our findings.

We replicate the analysis for alcohol and tobacco consumption for cohorts born in 2003 and 2004, where we augment equation (1) with family rules' variables that present significant differences between young– and old–for grade students as per Figure 9. For both outcomes, we control for whether parents' set the rules outside home. Further, we control for whether parents would permit the specific behavior in the corresponding regression. The inclusion of these covariates does reduce the magnitude of the young–for–grade treatment effects, especially for the young cohort (2004–born). This indicates that family rules related to differences in maturity may be a potential channel for the observed absolute–age differences in behaviors between young– and old–for–grade students.

Finally, differences in the schooling cycle are also a driver of our results, as indicated by the differences found in the comparison of similar age students (born in December 2003 and January 2004) and in the separate estimation by grade. 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. Regarding the latter, retained students (the oldest in the class) may act as role models for 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 from the estimation by grade than in findings from the estimation 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, 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.

4.2 Heterogeneity analysis: differences across gender and type of school

We investigate whether the negative effect of being young–for–grade on risky behaviors may differ across gender and type of school. Results here are obtained for the regression model (1) as well separately by birth year and grade.

Table A.6 in Appendix A shows the results for boys and girls. The estimated effects of being young–for–grade on offline gambling in the full sample are driven by boys, a finding in line with the higher gambling prevalence among boys documented in Calado et al. (2017). Being young–for–grade increases the probability of drinking both for boy and girls, similar to findings in Shin (2023), although unlike this study, we obtain that the young–for–grade effect is larger for girls than for boys, especially in the young cohort (born in 2004). Other studies only find significant

differences among girls (Argys and Rees, 2008; Johansen, 2021). While being young–for–grade has a similar negative impact in smoking marijuana across gender, with a slightly stronger effect for boys in third grade, the negative effect on smoking tobacco is larger for girls, especially among the youngest ones (born in 2004). This indicates more marked gender patterns in tobacco use among young– and old–for–grade students. In contrast, Argys and Rees (2008) find positive and larger effects of being younger on marijuana use and smoking for girls as compared to boys, which they interpret as the result of a higher young–for–grade girls' susceptibility to older peer pressure. Regarding sex–related activities, the negative effect on adult website navigation in the full sample is driven by boys, while the effect on engaging in risky sexual behavior shows no gender variation. Similar to the results from the full sample, most of the significant differences of being young–for–grade disappear or are less pronounced both for boys and girls as they age or advance in educational cycle.

Table A.7 reports the results separately for students attending public and private schools. Around 63% of the students in the compulsory schooling sample attend public schools. This figure is in line with the percentage of students (66%) who attended compulsory education in a public school in the year 2018/2019 in Spain.[19] SSSDU data categorizes semiprivate schools —known as *colegios concertados*, which are private schools receiving public funding— under the private schools category. Unfortunately, the survey does not differentiate between these two types of schools. However, it is likely that the majority of the schools categorized as private in the survey are semiprivate. Statistics from the Spanish Ministry of Education for the survey year indicate that, out of the total number of students attending compulsory education in non–public schools, 88% are in semiprivate schools. These schools adhere to the same regulations as public schools, including the requirement to accept all students without discrimination. However, they are allowed to charge fees for extracurricular activities, which can result in a relatively higher enrollment of students from wealthier backgrounds. Fully private schools tend to be located in affluent neighborhoods and predominantly select students from high–income families. Notice that the self–selection bias into different types of schools is accounted for in all the estimations through the school fixed effects included in equation (1).

Results in Table A.7 show that the overall negative effect of being young–for–grade on gambling observed in Figure 5 is concentrated among students in public schools. For alcohol use, the effect is slightly more negative for students in private than in public schools, with more marked differences in the old cohort (born in 2003). A similar pattern arises for smoking tobacco, while no relevant differences are observed across school types in smoking marijuana, vaping, using non–prescribed tranquilizers and gaming. As regards navigating adult websites and sexual behavior, we find no substantial differences according to the type of school.

[19]Figure calculated from the data on the Spanish schooling system for the school year 2018/2019 retrieved from the website of the Ministry of Education: `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`.

4.3 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 (but keeping the grade–retained dummy). The results stay the same as the ones shown in Figure 5.[20] The lack of influence of socio–demographic characteristics on the results is a good indication of randomness around the cutoff date.

Second, we re–run the analysis 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 the sample to estimate equation (1). In both cases, those born in months below the cutoff are in the treated group (young–for–grade) while those born above the cutoff are in the control group (old–for–grade). As explained in section 2.3, the balance tests for the socio–demographic variables also hold in these two, wider, samples. Figure B.1.1 in Appendix B shows the results. They mirror our main findings in Figure 5.

Finally, we assess the robustness of our results to employing an alternative estimation strategy, a sharp regression discontinuity approach. We use a sharp RDD since the month of birth (running variable) fully determines the treatment status (being young–for–grade in the cohort). We impose a three–month bandwidth on either side of the cutoff with linear functions of the running variables. The RDD estimates for being young–for–grade are plotted in Figure B.1.2 in Appendix B and they hardly change compared to our main results in Figure 5.

4.4 High school sample

There is very little evidence on the effects of school entry policies on the risky behavior of students in high school. For South Korea, Shin (2023) finds that the young–for–grade students are more likely to drink alcohol than older students and that this effect increases as students grow. Similar findings are shown by Argys and Rees (2008) for marijuana, smoking, alcohol drinking and sexual activity among US students. Here, we explore whether our findings in compulsory education remain in high school.

Figure B.5 in Appendix B shows the distribution of the month of birth of students enrolled in high school. In contrast to the nearly uniform distribution observed for students in compulsory education (Figure 3), the high school distribution exhibits a subtle but noticeable deviation from uniformity. This slight irregularity may be attributed to the positive selection in the high school sample discussed in section 2.2 as a result of SSSDU not surveying early school leavers, as they have left education. Consequently, the high school sample consists of students who have successfully transitioned from compulsory education, which may result in a non–uniform birth month distribution. While the difference in distributions between compulsory education and high

[20] For the sake of brevity, we do not report the results excluding the socio–demographic variables, but they are available upon request.

school is not dramatic, it is nonetheless noteworthy and serves as an important consideration when interpreting results related to relative age effects in the high school context.

Table A.9 in Appendix A shows the estimated effects of being young–for–grade for high school students by birth cohort and grade. As in our main analysis, young–for–grade students are born in December and old–for–grade students are born in January. For the separate estimation by birth year and grade, we use the same specification explained in section 3 for the compulsory schooling sample.[21] Notice that in the high school sample, the students from the birth years 2002 and 2001 are at the expected age in 2019, respectively, of first and second high school graders (17 and 18). Table A.9 shows that most significant negative differences in behavior between young–for–grade fade out from compulsory education to high school. The two exceptions are gambling and alcohol drinking in the last month, for which we detect persistent negative effects, indicating that young–for–grade are still less likely to adopt those behaviors compared to old–for–grade. Interestingly, the magnitude of the effect on gambling behavior is stable across birth cohorts, at 6 and 7 pp, respectively, for 2002 and 2001–born students. As for alcohol drinking, the effect (12 pp) in the young cohort (2002–born) is significant while it is not in the old cohort (2001–born). The results from the separate estimation by grade of enrollment lead to similar conclusions.

Therefore, while there may be more forces at play, and bearing in mind the issue of self–selection into post–compulsory education, results from the high school sample suggest that most significant differences in risky behaviors between young– and old–for–grade observed at 15 and 16 tend to disappear as they age to 17 and 18. As in compulsory education, young–for–grade high school students tend to catch up old–for–grade in many behaviors (see Table A.8 in Appendix A). The fact that we observe that significant differences fade out as students age is consistent with maturity as a possible mechanism. As students age, young–for–grade's maturity evolves and may catch up old–for–grade's degree of maturity, leading to the disappearance of most differences. Another plausible mechanism, which is not in conflict with the maturity channel, is that young–for–grade catch up old–for–grade because the imitation process, whereby old–for–grade act as role models for young–for–grade, intensifies as they transit during the adolescence period.

5 Conclusions

This study estimates the effect of the school entry age policy in Spain on the adolescents' adoption of risky health 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. In our main analysis, we focus on students enrolled in the last grades of compulsory education, which correspond with the early adolescence years (15–16 years old). We find that being young–for–grade reduces the likelihood of engagement

[21] Similarly, we exclude from the analysis the birth cohorts enrolled in high school but solely composed by retained students (born in 2000 or earlier).

in various risky behaviors, including gambling, alcohol and tobacco use, and sexual activity. These findings contrast with some previous studies in other countries, highlighting the importance of considering cultural and social contexts when examining relative age effects created by the school entry age policies.

The Spanish schooling system is characterized by high retention rates. Retained students have been held back due to low–academic performance and, therefore, they join the cohort that entered school one year later. Retention decisions, thus, break the one–to–one link between birth cohort and grade of enrollment. In our analysis, we take into account this and try to disentangle the extent to which results may be driven by differences in absolute age—i.e., year of birth—and differences in the schooling cycle—i.e., grade of enrollment.

Our findings appear to be driven by a combination of absolute age differences, as many significant differences diminish as students age from 15 to 16, and by the educational cycle, particularly for behaviors like gambling, alcohol, tobacco, and marijuana use. This suggests that both maturity development as students age and school contribute to the observed differences in risky behaviors between young–for–grade and old–for–grade students. The maturity mechanism is supported by the evidence from the high school sample, although findings from high school students should be considered with caution due to the positive selection in post–compulsory education. The role of the educational cycle 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. A deeper analysis of the classroom dynamics is beyond the scope of this paper and is left for future research.

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, they may face other challenges related 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 should implement targeted prevention programs for old–for–grade 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.

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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

	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							
Video games	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/ energy drinks	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 (Compulsive Internet Use)	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 a test of differences in mean. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Distribution of old– and young–for–grade students by birth year

		Old–for–grade		Young–for–grade	
Birth year	N	(%)	N	(%)	N
(1)	(2)	(3)	(4)	(5)	(6)
2004	8,500	8.2	700	8.4	711
2003	9,518	8.6	816	8.2	780
2002	2,395	8.0	191	9.8	235
2001	692	7.1	49	11.7	81
2000	51	–	0	19.6	10

Figures

Figure 1: Spanish schooling system

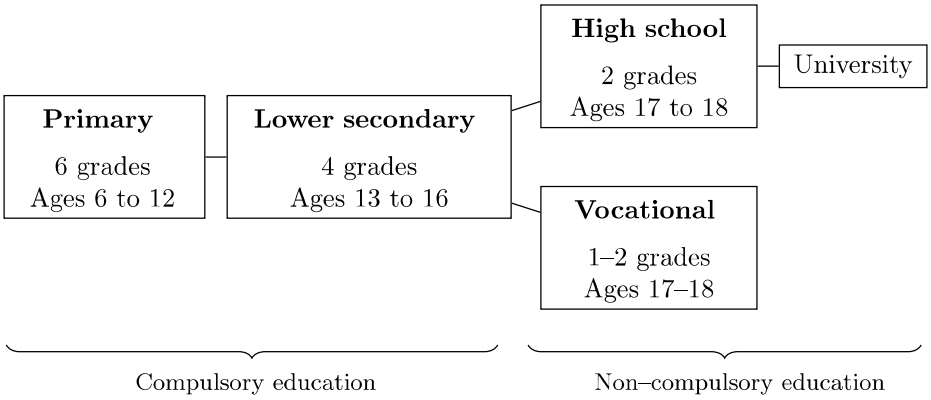


Figure 2: Spanish school entry policy

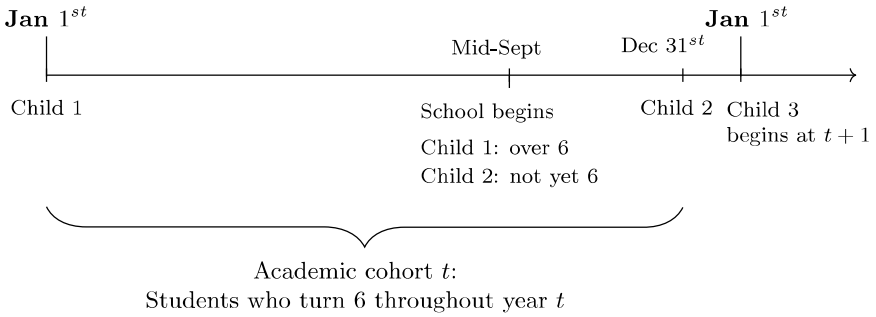
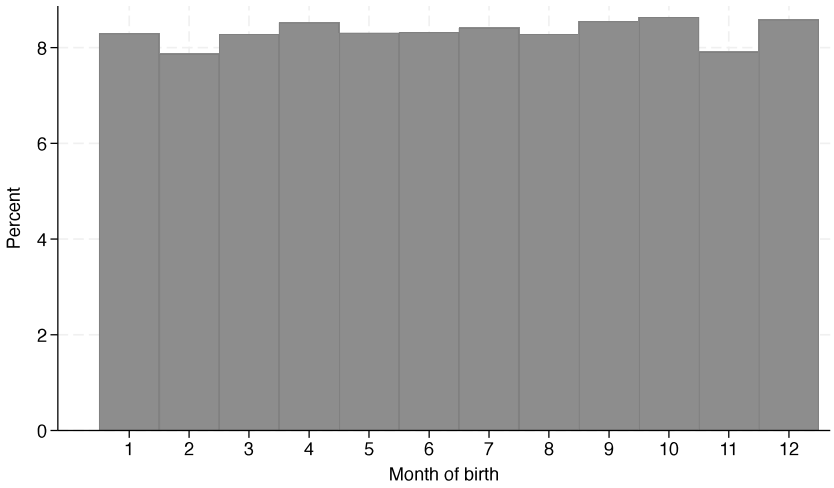
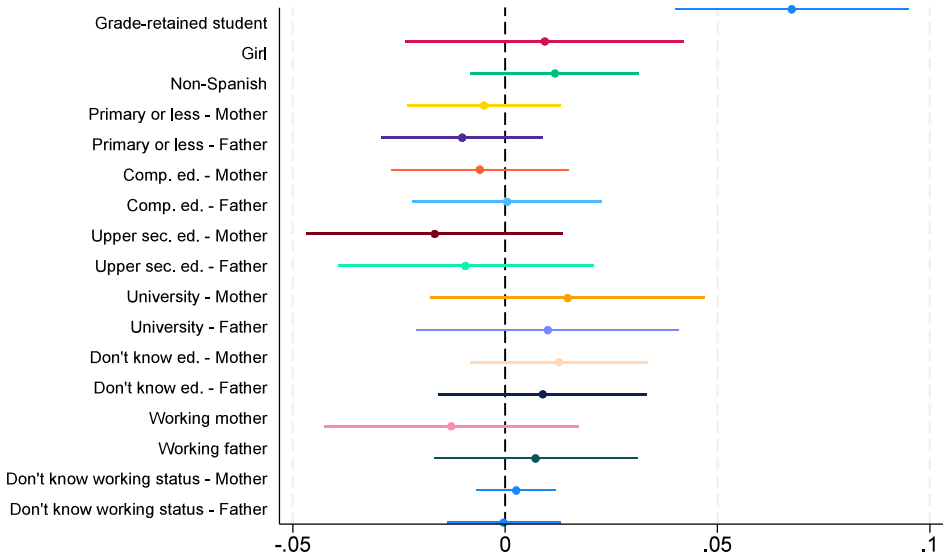


Figure 3: Distribution of month of birth - Students in compulsory education



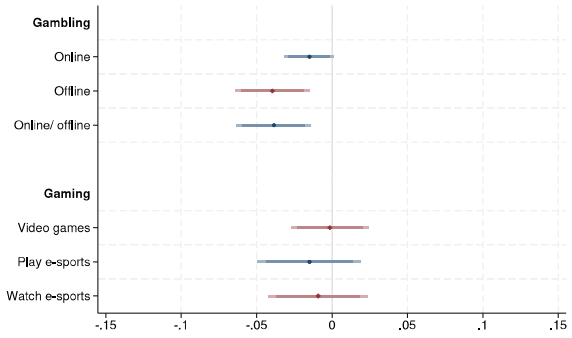
Note: SSSDU 2018 wave, N = 21,156 students.

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

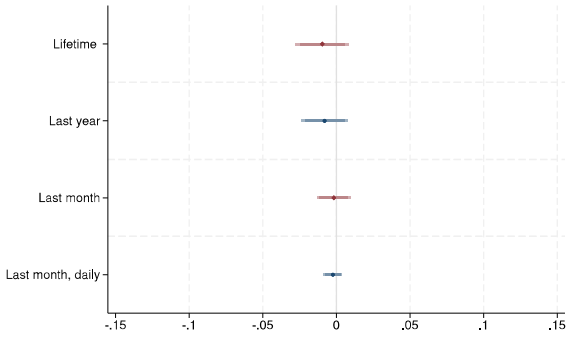


Note: $N = 3,573$ students. 1,756 old–for–grade students (born in January); 1,817 young–for–grade students (born in December).

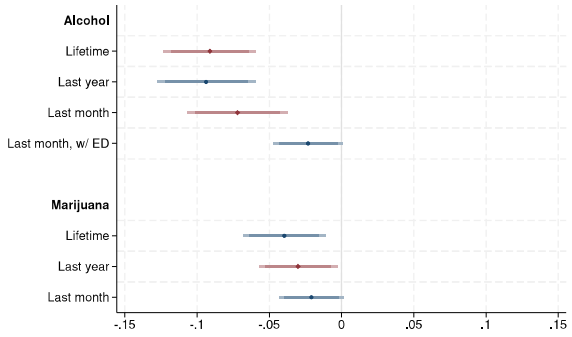
Figure 5: Young–for–grade results (all grades and birth years)



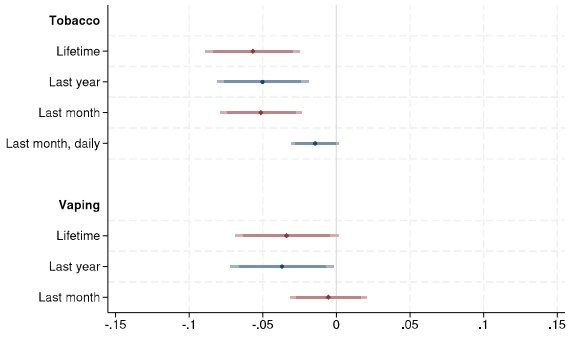
(a) Gambling and gaming



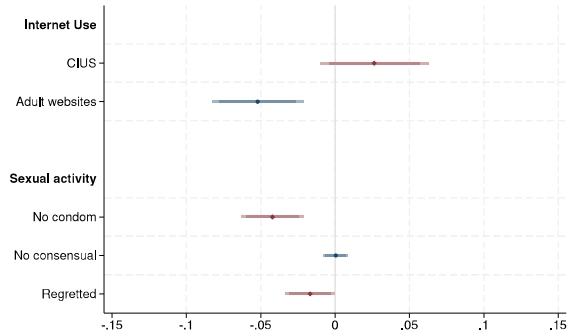
(b) Non–prescribed tranquilizers' use



(c) Alcohol and marijuana use



(d) Tobacco and vaping

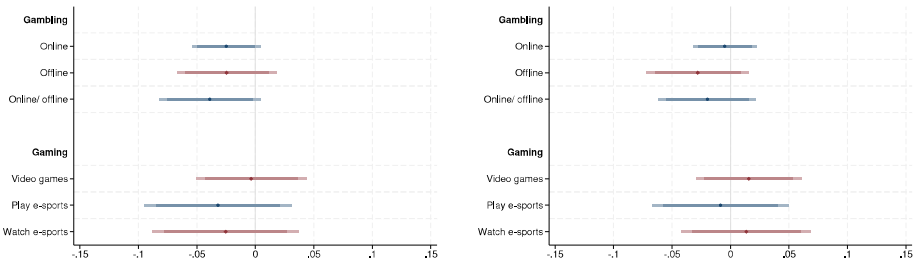


(e) Other risky behaviors

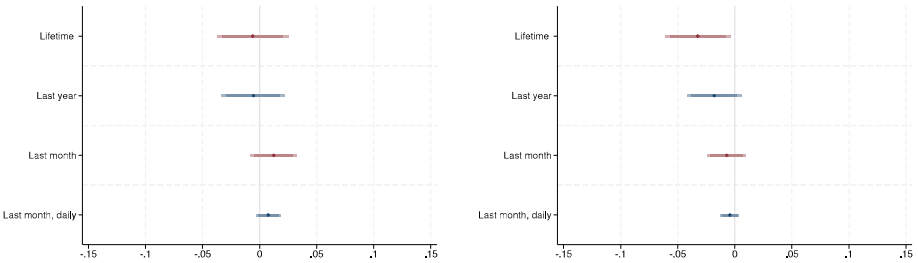
Note: $N = 3,573$ students. 1,756 old–for–grade students (born in January); 1,817 young–for–grade students (born in December). OLS estimates of the young–for–grade dummy. One separate estimation for each outcome. All regressions control for socio–demographic variables, repeater, birth year fixed effects, grade fixed effects and school fixed effects. ED: energy drinks.

Figure 6: Young–for–grade results, by birth year

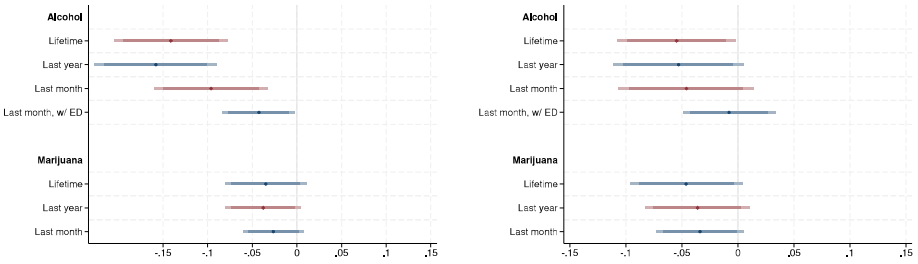
Born in 2004 – Turn 15 years old Born in 2003 – Turn 16 years old



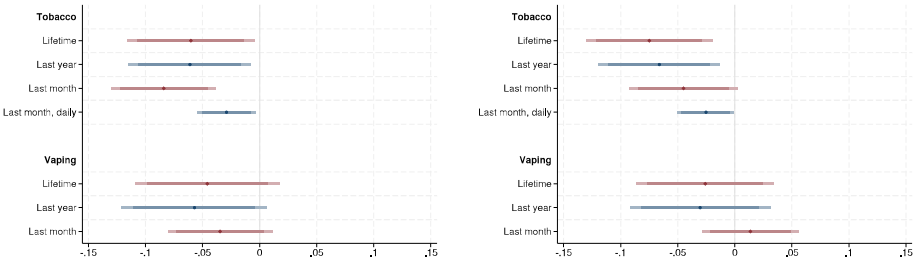
(a) Gambling and gaming



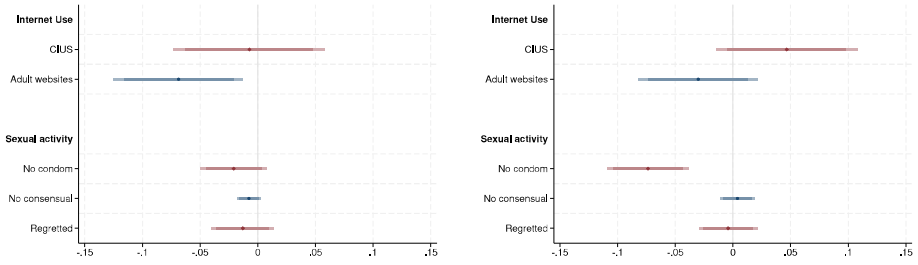
(b) Non–prescribed tranquilizers' use



(c) Alcohol and marijuana use



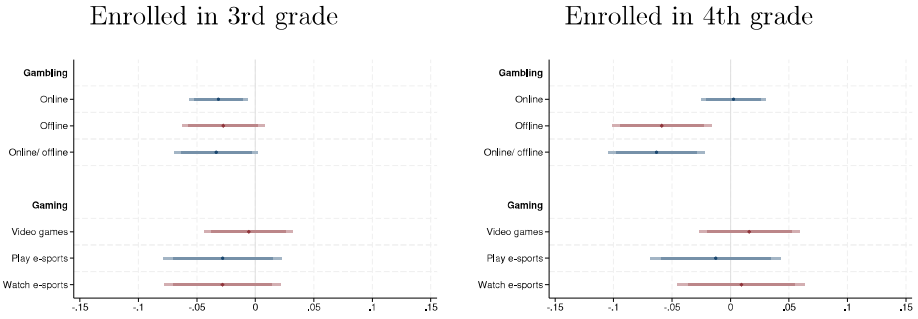
(d) Tobacco and vaping



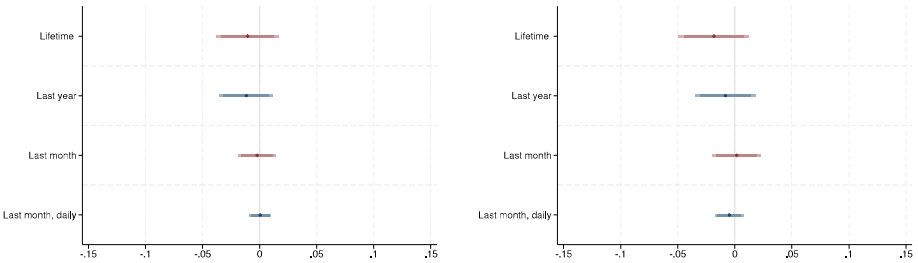
(e) Other risky behaviors

Note: $N = 3,573$ students born in January or December; 1,411 born in 2004 and 1,596 born in 2003.

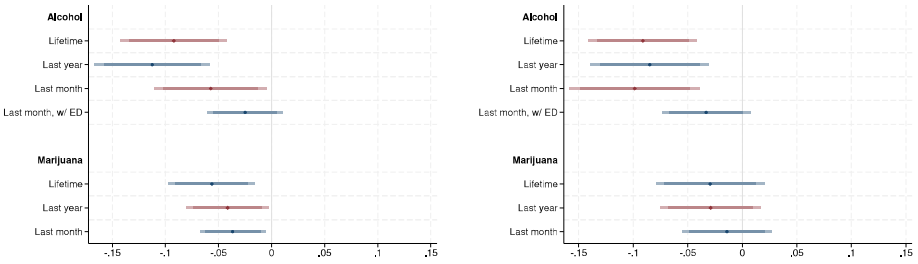
Figure 7: Young–for–grade results, by grade



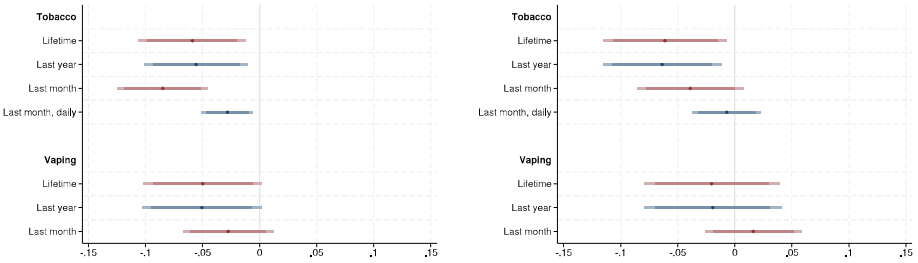
(a) Gambling and gaming



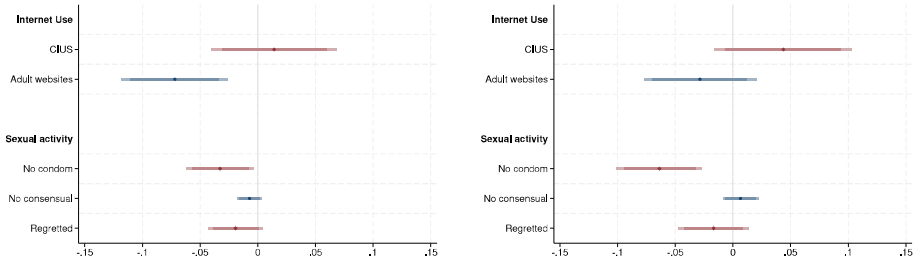
(b) Non–prescribed tranquilizers' use



(c) Alcohol and marijuana use



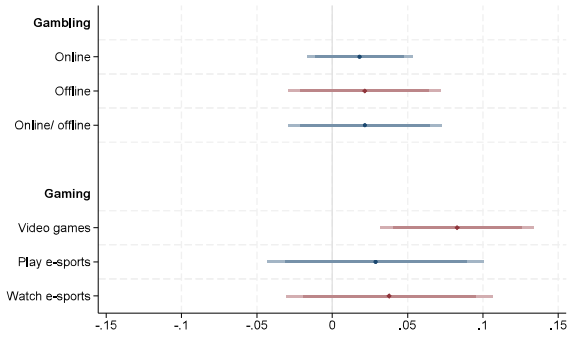
(d) Tobacco and vaping



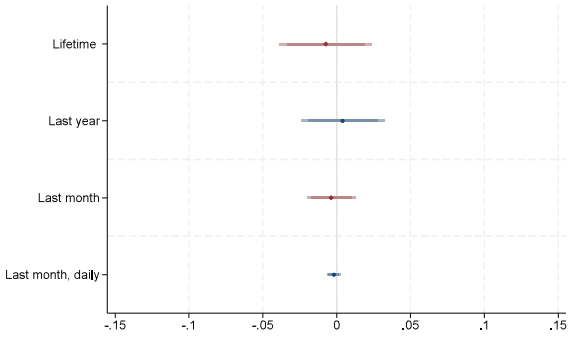
(e) Other risky behaviors

Note: $N = 3,573$ students born in January or December; 1,933 enrolled in 3rd grade and 1,640 in 4th grade.

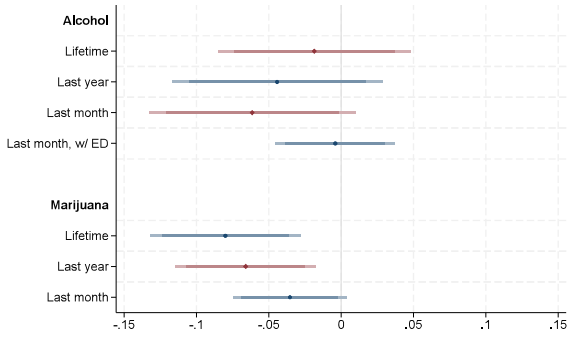
Figure 8: Students born in January 2004 vs born in December 2003



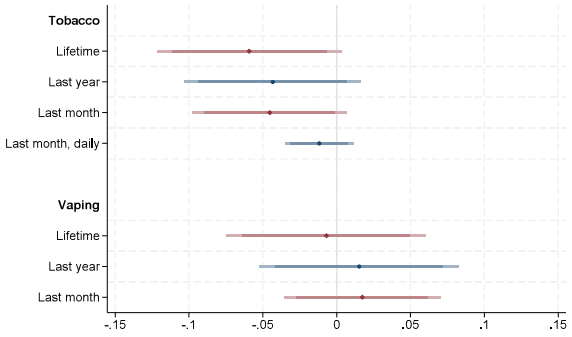
(a) Gambling and gaming



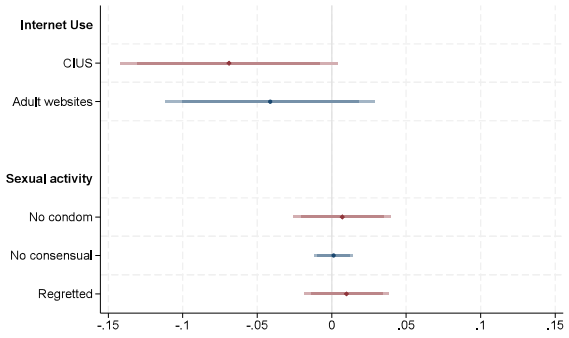
(b) Non–prescribed tranquilizers' use



(c) Alcohol and marijuana use



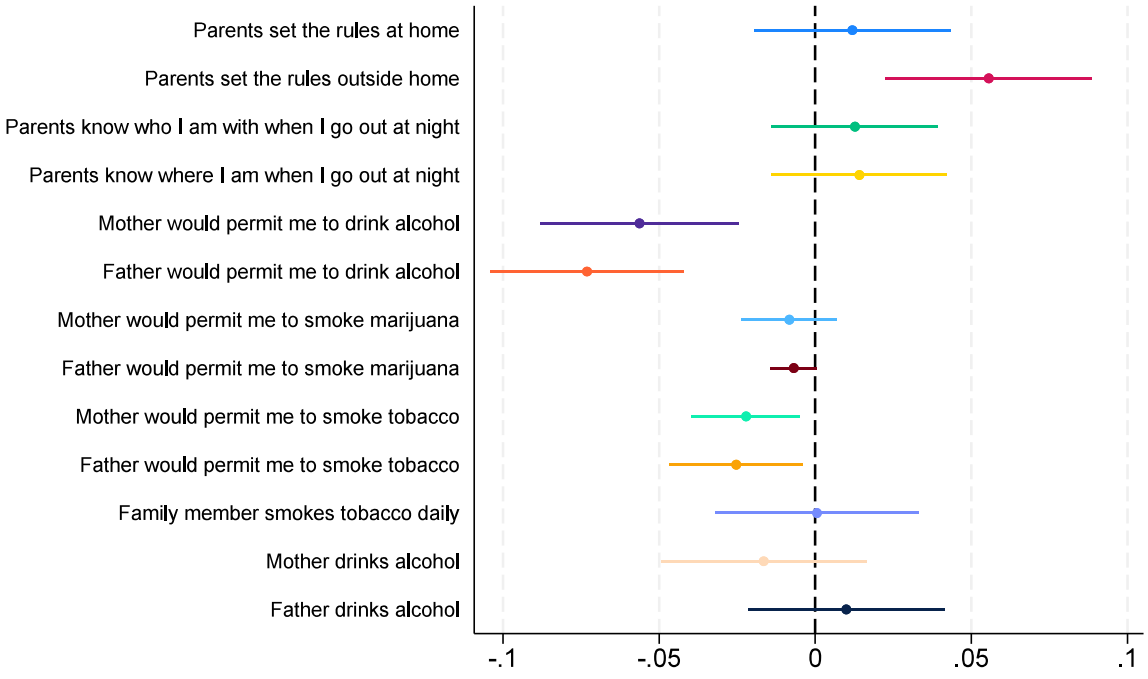
(d) Tobacco and vaping



(e) Other risky behaviors

Note: $N = 1,480$ students. 780 students born in December 2003; 700 students born in January 2004. Figures show the OLS estimates of a dummy variable equal to one if student is born in January 2004 and zero if she/he is born in December 2003. One separate estimation for each outcome. All regressions control for socio–demographic variables, repeater, and school fixed effects. ED: energy drinks.

Figure 9: Family norms for young–for–grade versus old–for–grade students



Note: $N = 3,573$ students. 1,857 students born in December; 1,756 students born in January.

Appendices

A Supplementary tables

Table A.1: 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.2: 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.3: Young–for–grade effects in compulsory education — OLS estimates

	Full	Born in 2004	Born in 2003	3rd grade	4th grade	Born in Jan. 2004 vs Dec. 2003
Gambling						
Online	-0.01*	-0.02*	-0.01	-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	0.08***
	(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' use						
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 ycar	-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***	-0.02
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
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	-0.02	-0.03	-0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Marijuana						
Lifetime	-0.04***	-0.03	-0.05*	-0.06***	-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**	-0.06**	-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**	-0.03**	-0.03**	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Vaping						
Lifctimc	-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	-0.01	-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	0.04	-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	1,305

Each cell in each column shows the 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.4: Young–for–grade effects in compulsory education — OLS estimates, including family norms covariates

	Born in 2004	Born in 2003
Alcohol		
Lifetime	-0.07*	-0.08**
	(0.04)	(0.03)
Last year	-0.08*	-0.07*
	(0.04)	(0.04)
Last month	-0.04	-0.06*
	(0.04)	(0.04)
Last month, w/ ED	-0.02	-0.04*
	(0.02)	(0.03)
Tobacco		
Lifetime	-0.04	-0.07**
	(0.03)	(0.03)
Last year	-0.05	-0.06*
	(0.03)	(0.03)
Last month	-0.06**	-0.04
	(0.03)	(0.03)
Last month, daily	-0.01	-0.01
	(0.01)	(0.01)
Observations	1,411	1,596

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

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

Table A.6: Young–for–grade effects in compulsory education, by gender — OLS estimates

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

Each cell in each column shows the 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.7: Young–for–grade effects in compulsory education, by school type

	Full		Born in 2004		Born in 2003		3rd grade		4th grade	
	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*
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Offline	-0.05***	-0.03	-0.02	-0.03	-0.05	0.01	-0.02	-0.03	-0.06**	-0.03
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Online/offline	-0.05***	-0.02	-0.03	-0.05	-0.04	0.02	-0.02	-0.04	-0.08***	-0.03
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Gaming										
Videogames	-0.00	-0.00	0.02	-0.02	0.00	0.02	0.00	-0.02	0.01	0.02
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)
Play e-sports	-0.00	-0.03	-0.00	-0.06	-0.01	-0.00	-0.01	-0.05	-0.01	-0.01
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)
Watch e-sports	-0.03	0.02	-0.04	0.00	-0.01	0.04	-0.05	0.01	-0.02	0.05
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Non-prescribed tranquilizers' use										
Lifetime	-0.02	0.01	-0.01	-0.01	-0.05**	-0.01	-0.01	-0.01	-0.03	0.00
	(0.01)	(0.01)	(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
	(0.01)	(0.01)	(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	-0.02**	0.00	0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
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)	(0.01)	(.)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Alcohol										
Lifetime	-0.08***	-0.12***	-0.12***	-0.17***	-0.02	-0.11***	-0.06*	-0.16***	-0.08**	-0.10***
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
Last year	-0.08***	-0.12***	-0.15***	-0.18***	-0.01	-0.12**	-0.08**	-0.18***	-0.08**	-0.09**
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
Last month	-0.06**	-0.10***	-0.10**	-0.10**	-0.00	-0.10**	-0.05	-0.09**	-0.07*	-0.13***
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)
Last month, w/ ED	-0.03	-0.02	-0.06**	-0.03	0.02	-0.04	-0.03	-0.02	-0.02	-0.04
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Marijuana										
Lifetime	-0.06***	-0.02	-0.05	-0.03	-0.06*	-0.02	-0.05**	-0.06*	-0.06*	0.02
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
Last year	-0.03*	-0.02	-0.04	-0.04	-0.04	-0.03	-0.03	-0.06*	-0.05	-0.01
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
Last month	-0.02	-0.02	-0.02	-0.04	-0.04	-0.03	-0.03	-0.06**	-0.03	-0.01
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(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**
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Last year	-0.04**	-0.06**	-0.06*	-0.07*	-0.04	-0.07*	-0.06*	-0.06	-0.03	-0.11**
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
Last month	-0.03*	-0.08***	-0.08***	-0.09**	-0.00	-0.09**	-0.07***	-0.10***	-0.00	-0.09**
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(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
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)
Vaping										
Lifetime	-0.03	-0.04	-0.01	-0.10**	-0.02	-0.10**	-0.03	-0.08*	-0.03	-0.01
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(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)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(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)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)
Internet use										
CIUS	0.01	0.05*	-0.02	-0.01	0.05	-0.01	0.01	0.01	0.05	0.04
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
Adult websites	-0.05***	-0.05**	-0.06	-0.08*	-0.03	-0.08*	-0.06*	-0.09**	-0.04	-0.01
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
Sexual activity										
No condom	-0.04***	-0.04**	-0.02	-0.02	-0.07***	-0.02	-0.03*	-0.03	-0.06**	-0.07**
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
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)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(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)	(0.02)	(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

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

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

Table A.9: Young–for–grade effects in high school — OLS estimates

	Born in 2002	Born in 2001	1st grade	2nd grade
Gambling				
Online	-0.06***	-0.01	-0.04**	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Offline	-0.04	-0.07**	-0.04	-0.07*
	(0.03)	(0.03)	(0.02)	(0.04)
Online/offline	-0.06**	-0.07**	-0.06**	-0.07*
	(0.03)	(0.03)	(0.02)	(0.04)
Gaming				
Videogames	0.03	0.06*	0.04	0.05
	(0.03)	(0.04)	(0.03)	(0.04)
Play e-sports	0.02	0.05	0.01	0.04
	(0.04)	(0.04)	(0.03)	(0.04)
Watch e-sports	0.00	0.00	0.00	-0.01
	(0.03)	(0.03)	(0.03)	(0.04)
Non-prescribed tranquilizers' use				
Lifetime	0.01	0.00	0.00	-0.01
	(0.02)	(0.03)	(0.02)	(0.03)
Last year	0.01	-0.02	0.00	-0.04
	(0.02)	(0.02)	(0.02)	(0.03)
Last month	0.01	-0.04**	-0.00	-0.04*
	(0.01)	(0.02)	(0.01)	(0.02)
Last month, daily	0.00	-0.00	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Alcohol				
Lifetime	-0.01	-0.03	-0.02	-0.05*
	(0.03)	(0.03)	(0.02)	(0.03)
Last year	-0.03	-0.03	-0.02	-0.05
	(0.03)	(0.03)	(0.03)	(0.03)
Last month	-0.06	-0.12***	-0.07**	-0.13***
	(0.04)	(0.04)	(0.03)	(0.04)
Last month, w/ ED	-0.03	-0.01	-0.04*	-0.01
	(0.03)	(0.03)	(0.02)	(0.03)
Marijuana				
Lifetime	-0.03	-0.00	-0.02	-0.05
	(0.04)	(0.04)	(0.03)	(0.04)
Last year	-0.02	-0.02	-0.01	-0.07
	(0.04)	(0.04)	(0.03)	(0.04)
Last month	-0.03	-0.03	-0.03	-0.05
	(0.03)	(0.03)	(0.03)	(0.03)
Tobacco				
Lifetime	-0.04	0.05	-0.01	0.02
	(0.04)	(0.04)	(0.03)	(0.05)
Last year	-0.02	0.03	-0.00	-0.02
	(0.04)	(0.04)	(0.03)	(0.05)
Last month	-0.02	-0.02	-0.01	-0.05
	(0.04)	(0.04)	(0.03)	(0.04)
Last month, daily	-0.01	-0.01	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.03)
Vaping				
Lifetime	-0.03	0.04	-0.03	0.04
	(0.04)	(0.04)	(0.03)	(0.05)
Last year	-0.03	0.02	-0.03	0.01
	(0.04)	(0.04)	(0.03)	(0.04)
Last month	-0.02	-0.03	-0.02	-0.03
	(0.03)	(0.03)	(0.02)	(0.03)
Internet use				
CIUS	0.08*	-0.04	0.05	-0.02
	(0.04)	(0.04)	(0.03)	(0.05)
Adult websites	0.02	-0.04	0.02	-0.05
	(0.03)	(0.03)	(0.03)	(0.04)
Sexual activity				
No condom	-0.03	-0.05	-0.04	-0.06
	(0.03)	(0.03)	(0.02)	(0.04)
No consensual	-0.00	-0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Regretted	0.01	-0.02	0.00	-0.04
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	6,376	6,426	8,256	5,521

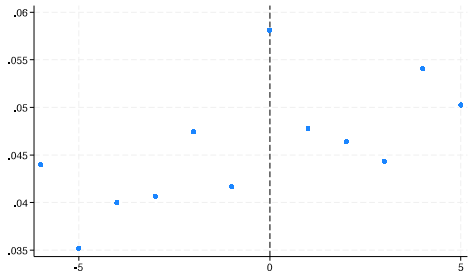
Each cell in each column shows the coefficient of the young–for–grade dummy from a separate regression. Robust standard errors in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Supplementary figures

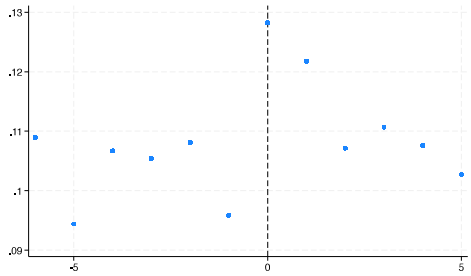
B.1 Descriptive evidence of the effect of the cutoff on risky behaviors

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.

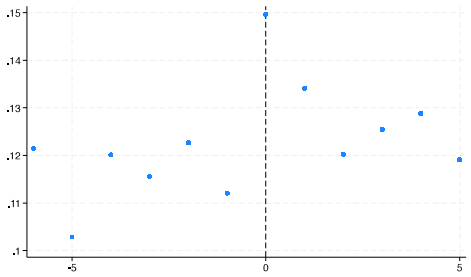
Gambling



Online

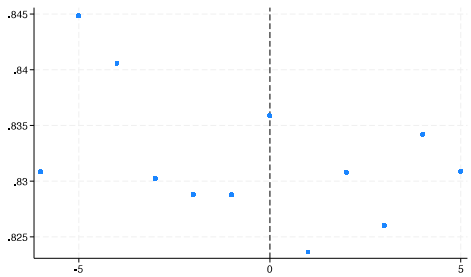


Offline

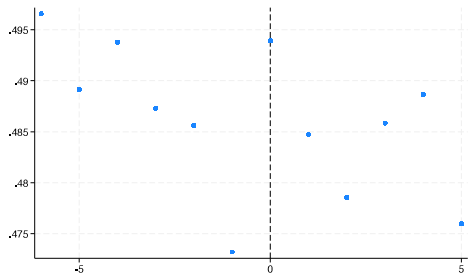


Online/ offline

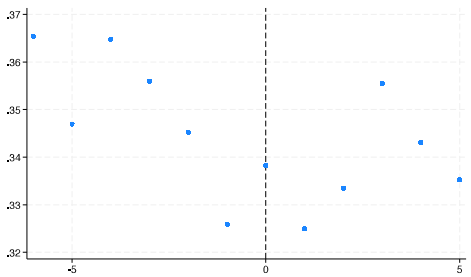
Gaming



Video games

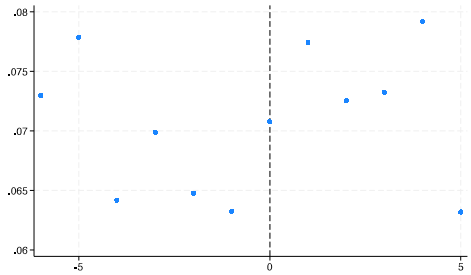


Play e–sports

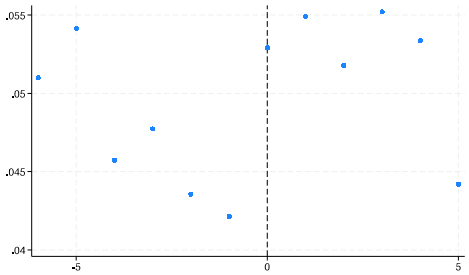


Watch e–sports

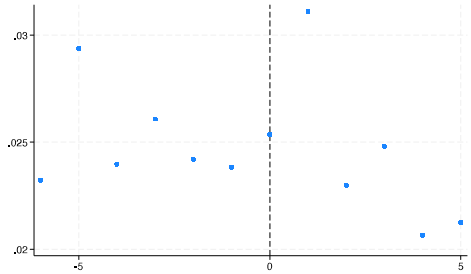
Non-prescribed tranquilizers' use



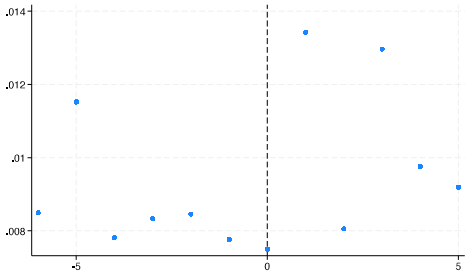
Lifetime



Last year

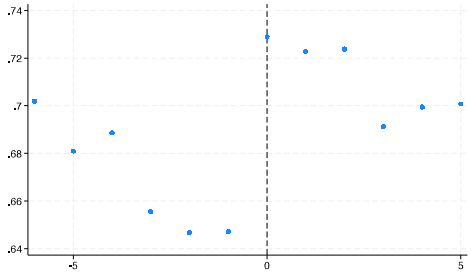


Last month

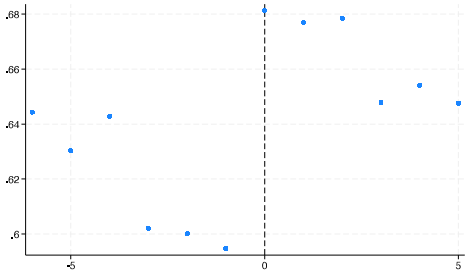


Last month, daily

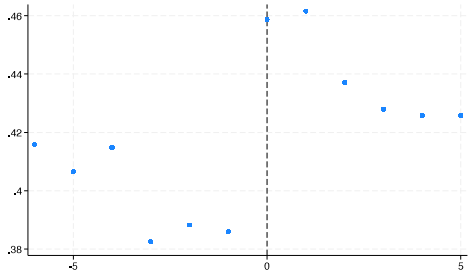
Alcohol



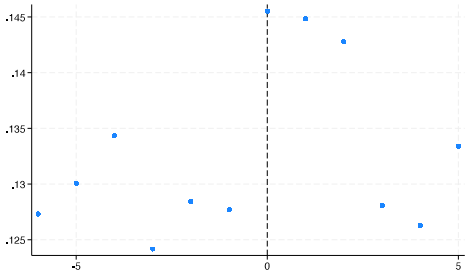
Lifetime



Last year

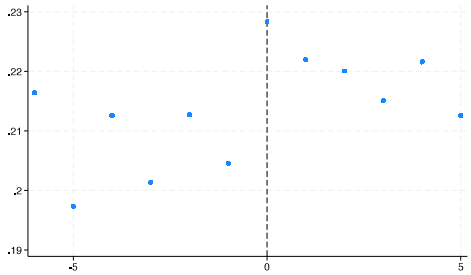


Last month

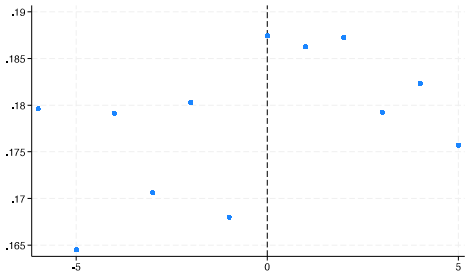


Last month, w/ energy drink

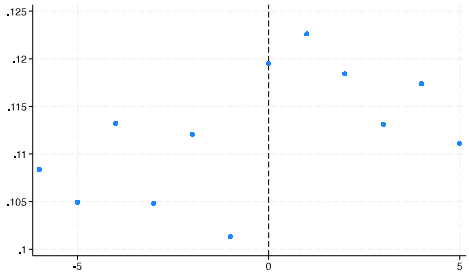
Marijuana



Lifetime

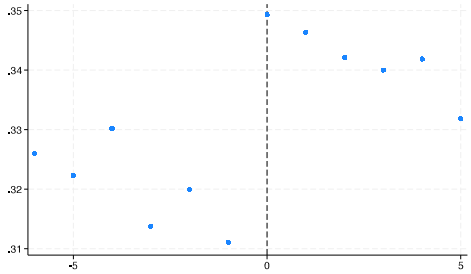


Last year

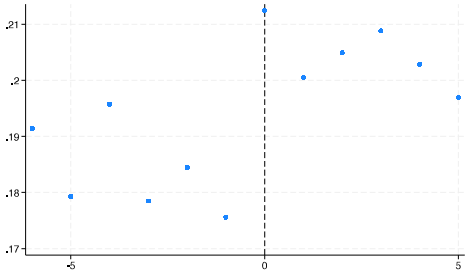


Last month

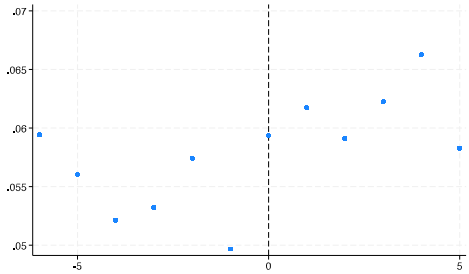
Tobacco



Lifetime

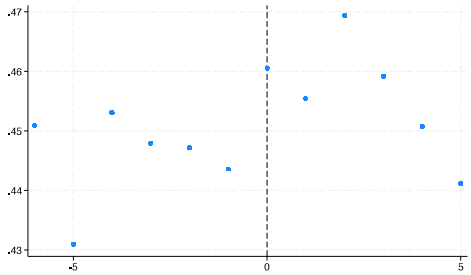


Last month

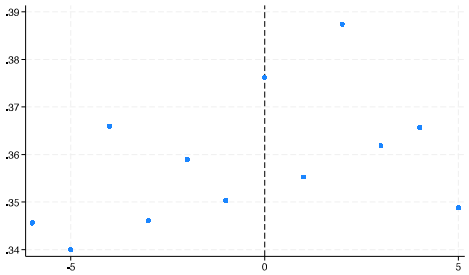


Last month, daily

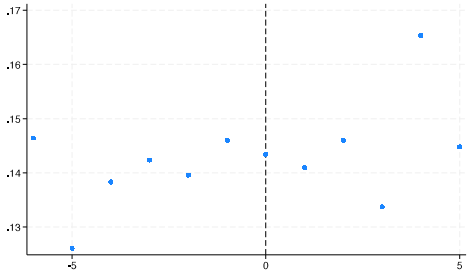
Vaping



Lifetime

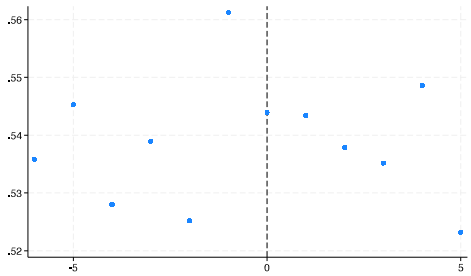


Last year

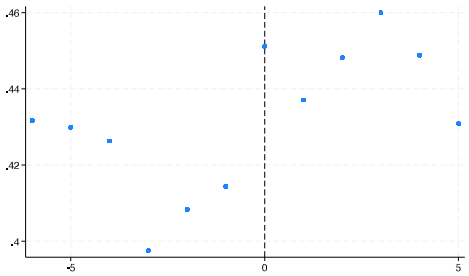


Last month

Internet use

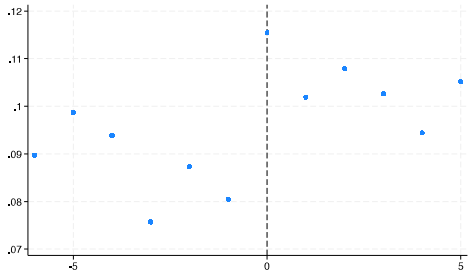


CIUS

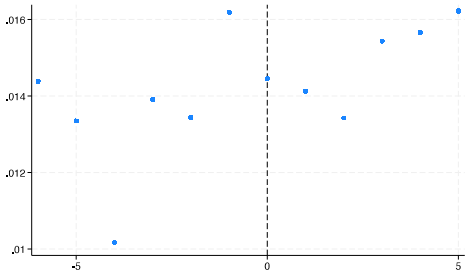


Adult websites

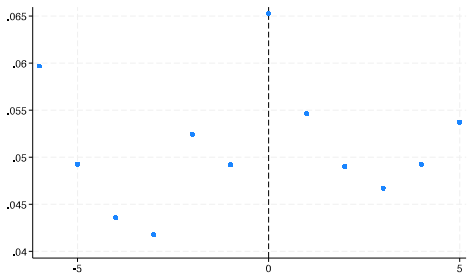
Sexual activity



No condom



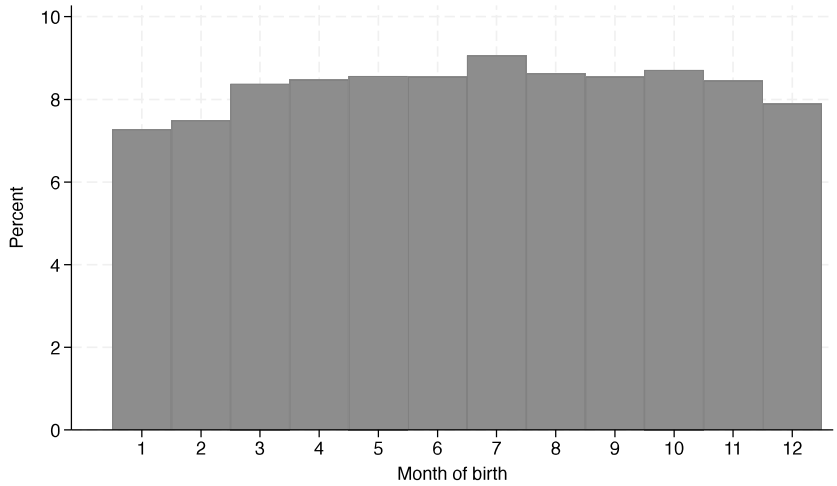
No consent



Regretted

B.2 Other figures

Figure B.5: Distribution of month of birth - Students enrolled in high school

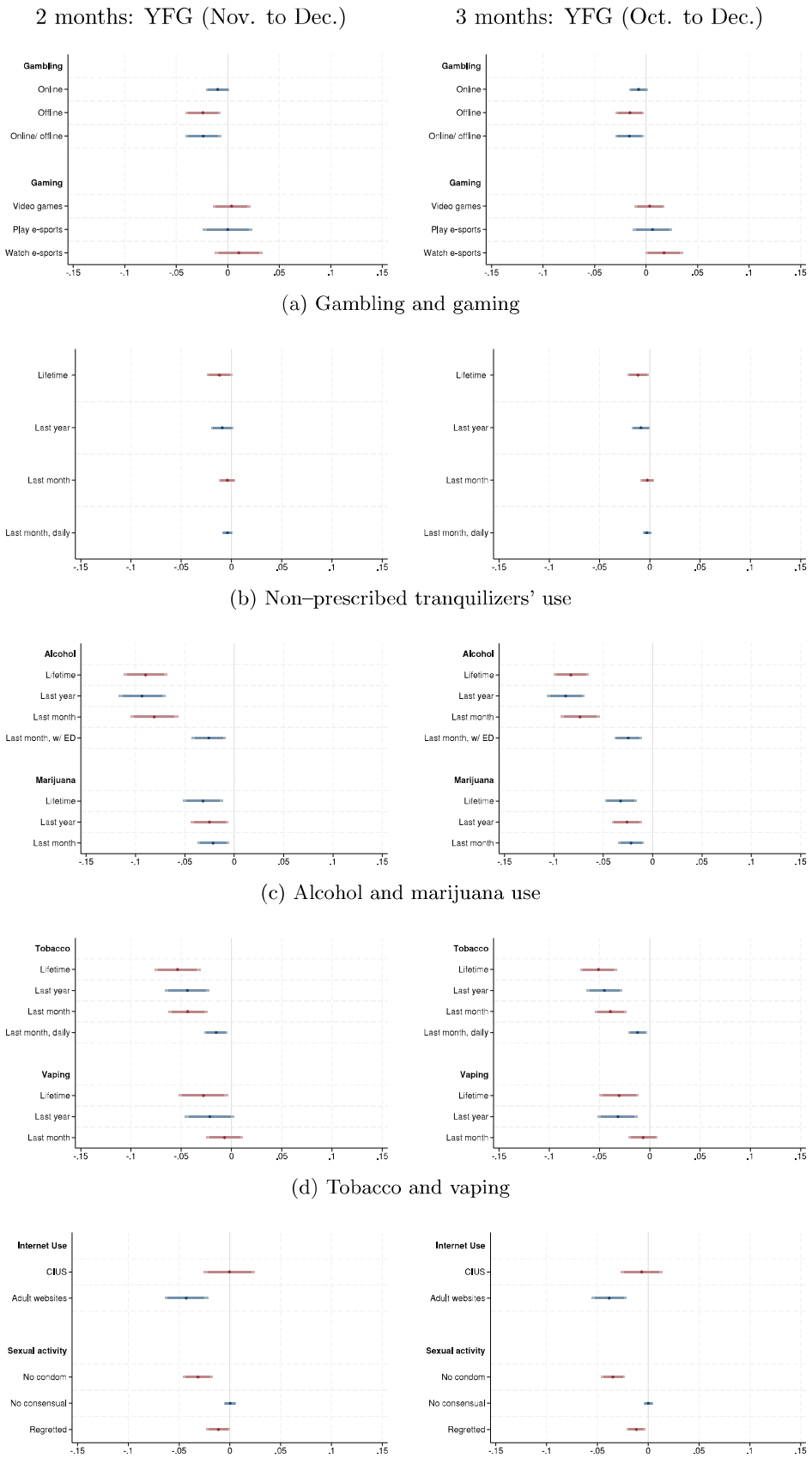


Note: SSSDU 2018 wave, N = 13,777 students.

B.3 Different bandwidths

Figure B.1.1 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.1.1: Young–for–grade results, by different bandwidths



(a) Gambling and gaming

(b) Non–prescribed tranquilizers' use

(c) Alcohol and marijuana use

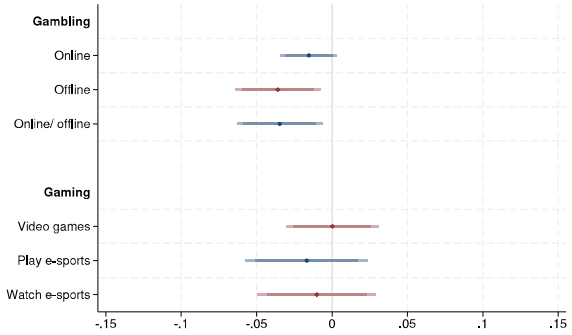
(d) Tobacco and vaping

(e) Other risky behaviors

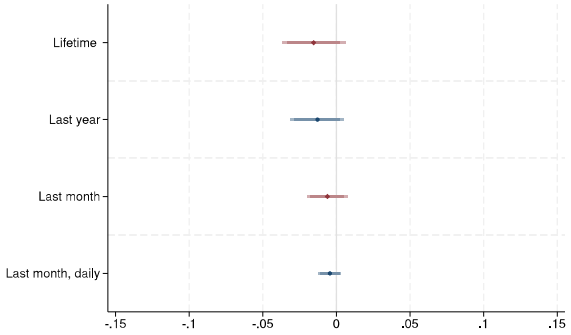
Note: YFG (young–for–grade); 6,912 students born in January to February and November to December; 10,491 students born in January to March and October to December.

B.4 Regression Discontinuity Design

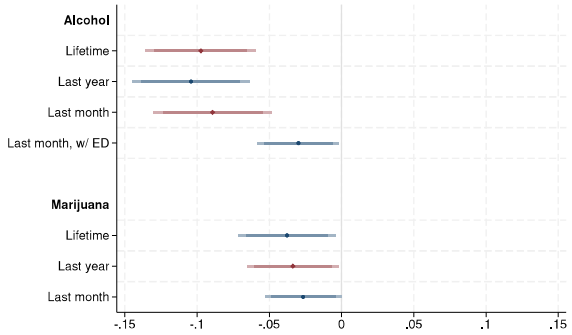
Figure B.1.2: Young–for–grade results (all grades and birth years, 3–month bandwidth)



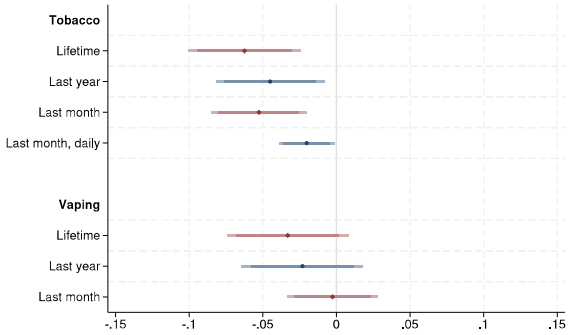
(a) Gambling and gaming



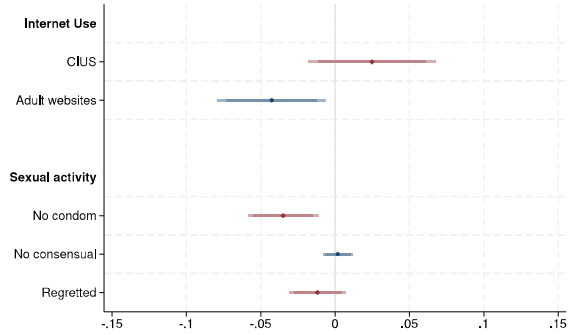
(b) Non–prescribed tranquilizers' use



(c) Alcohol and marijuana use



(d) Tobacco and vaping



(e) Other risky behaviors

Note: $N = 10,491$ students. 5,173 old–for–grade students (born in January to March); 5,318 young–for–grade students (born in October to December). RDD estimates of the young–for–grade dummy using a 3–month bandwidth. One separate estimation for each outcome. All regressions control for a linear function of the running variable, socio–demographic variables, repeater, birth year fixed effects, grade fixed effects and school fixed effects.

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